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OR

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OF

PRACTICAL MEDICINE AND SURGERY

EDITED BY

JOHN FORBES M.D. F.R.S.

AND

JOHN CONOLLY M.D.

EDITORS OF THE CYCLOPÆDIA OF PRACTICAL MEDICINE

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THE
BRITISH AND FOREIGN
MEDICAL REVIEW,

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PART FIRST.

Analytical and Critical Reviews.

ART. I.

1. *Traité pratique des Maladies Vénériennes, comprenant l'Examen des Théories et des Méthodes de Traitmens qui ont été adoptées dans ces Maladies, et principalement la Méthode thérapeutique employée à l'Hôpital Militaire d'Instruction du Val de Grace.* Par H. M. J. DESRUELLES, Docteur en Médecine, &c. &c.—Paris, 1836. 8vo. pp. 668.

A Practical Treatise on Venereal Diseases; containing an Examination of the Theories and Methods of Treatment which have been adopted in these Maladies, and especially the Method of Cure practised at the Military Hospital of Instruction of the Val de Grace. By H. M. J. DESRUELLES, &c. &c.

2. *Practical Observations on the Venereal Disease, and on the Use of Mercury.* By ABRAHAM COLLES, M.D., one of the Surgeons of Steevens's Hospital, Dublin, &c.—London, 1837. 8vo. pp. 351.
3. *Procès-verbaux des Séances tenues par les Médecins de Nantes, dans la grande Salle de l'Hôtel de Ville, pour discuter la Valeur des Doctrines nouvelles, relativement à la Nature et au Traitement de la Syphilis.*—Nantes, 1835. 8vo. pp. 151.

Minutes of the Meetings held by the Medical Practitioners of Nantes, in the Hall of the Hôtel de Ville, to discuss the Value of the new Doctrines relative to the Nature and Treatment of Syphilis.—Nantes, 1835.

4. *Historisch-Kritische Darstellung des Streits über die Einheit oder Mehrheit der Venerischen Contagien.* Von Dr. FRIEDRICH OESTERLEN.—Stuttgart, 1836. 8vo. pp. 343.

Historical and Critical Display of the Controversy on the Unity or Plurality of the Venereal Contagion. By Dr. F. OESTERLEN.—Stuttgart, 1836.

5. *Die Mercurialkrankheit in allen ihren Formen, geschichtlich, patho-*

logisch, diagnostisch, und therapeutisch dargestellt von G. L. DIETERICH, M.D., &c.—*Leipzig*, 1837. 8vo. pp. 422.

The Mercurial Disease in all its Forms, historically, pathologically, diagnostically, and therapeutically represented by G. L. DIETERICH, M.D.—*Leipzig*, 1837.

6. *Die Behandlung der Lustseuche ohne Quecksilber oder die nicht Merkuriellen Mittel und Methoden zur Heilung der Lustseuche.* Von F. W. OPPENHEIM, M.D., &c.—*Hamburg*, 1827. 8vo. pp. 289.

The Treatment of Syphilis without Mercury; or, the non-Mercurial Means and Methods for the Cure of Syphilis. By Dr. F. W. OPPENHEIM.—*Hamburg*, 1827.

7. *Traité pratique de la Syphilis.* Par le BARON PHILIP BOYER, ancien Chirurgien à l'Hôpital des Vénériens de Paris, &c.—*Paris*, 1836. 8vo. pp. 448.

A Practical Treatise on Syphilis. By BARON PHILIP BOYER, formerly Surgeon to the Venereal Hospital of Paris.—*Paris*, 1836.

8. *A Practical Treatise on Urethritis and Syphilis.* By WILLIAM HENRY JUDD, Member of the Royal College of Surgeons in London, Surgeon in H. M. Fusilier Guards.—*London*, 1836. 8vo. pp. 610; *illustrated by coloured Plates.*

9. *A Treatise on the Venereal Disease.* By JOHN HUNTER, F.R.S. *With Notes*, by G. G. BABINGTON, Surgeon to St. George's Hospital, &c.—*The Works of JOHN HUNTER, edited by Mr. PALMER.* Vol. II. *London*, 1835.

If there is one disease which more than another merits the distinction of the term *opprobrium medicorum*, that disease is Syphilis. For little less than three centuries and a half it has monopolized a share of medical literature nearly as large as all the remaining members of the nosological family put together. And what has been effected by all these labours? One of the most obvious results, at any rate, is, that an ample library might be furnished forth, consisting of volumes exclusively devoted to the elucidation of its multifarious obscurities; yet, so far is that elucidation from being accomplished, that the fecundity of the press in works relating to venereal maladies has never been so great as at the present time.

The revolutions which both theory and practice have undergone, in every point connected with syphilis, are certainly extraordinary. The treatment, especially, has vibrated between two extremes, in support of each of which success was confidently appealed to. Till within a very few years, the statements of the old non-mercurialists, if not absolutely rejected as fabulous, were, at any rate, incomprehensible to those who held the opposite doctrine; yet the very system pursued by them rose once more from its obscurity, and, for a time, reached so high a degree of popularity as to threaten the extinction of the mercurial treatment. At present, however, although some theoretical questions may be as far from solution as at any former period, in all that appertains to the grand point at issue—the best means of cure, we may with justice affirm that a gradual convergence of opinion is daily becoming more apparent. This is not, perhaps, the only example to be gathered from the history of medi-

cine in illustration of Goethe's striking remark, that "the human mind is ever on the advance, but it moves in a spiral line."

It would be incompatible with the limits and objects of an article like the present, even to advert slightly to the various disputed questions branching out of this complicated enquiry; we shall therefore confine our attention, as much as possible, to such points as have a practical tendency. After taking a general survey of the history of syphilis during the last fifty years, which is that portion of it relating to the rise of the new doctrine, we shall arrange our materials under the two heads of the Natural History of the Disease and the Treatment, and conclude with a brief notice of the share which the several works enumerated at the head of the article may claim in the discussion, and of the information they communicate upon the different branches of the subject.

I. Into the long-agitated questions relative to the early history of syphilis, it would be foreign to our purpose to enter. Whether it arose at the latter end of the fifteenth century, as an entirely new disease, or by the graft of a disease then existing upon a group of disorders well known to the ancients, or whether it has existed in its present form from the earliest ages, is of little practical importance. Excepting by those writers who have a sort of personal interest in maintaining the last-named hypothesis, it is now generally acknowledged that no satisfactory evidence has ever been advanced in its proof; whilst the belief in the modern origin of the disease is strongly supported by historical testimony. We must protest, therefore, against the arbitrary conclusions of M. Desruelles advanced in support of the theoretical and practical views of his own party, which are grounded upon the assumed antiquity of the venereal disease, and consist in a repetition of the doctrine of Hensler, Swediaur, B. Bell, &c. His assertion, "that not only primary venereal affections, but vegetations, pustules, rhagades, dartres, and all the exanthemata now called syphilitic, were known to the ancients, and had been described before the epidemic of Naples," is not borne out by his own historical survey: whether the syphilis of the present day is identical, in all respects, with the epidemic of Naples, is another question. Passing over, therefore, the first two of the three epochs into which M. Desruelles divides the history of the disease, we shall take up the thread of his narrative at the commencement of his third epoch, and, with some assistance from other sources of information, endeavour to extract therefrom a brief sketch of its history during the last fifty years, a period in which a more correct knowledge of syphilis, and more sound principles of treatment, have been acquired than throughout the preceding three centuries.

"It was about the year 1783," says M. D., "that many practitioners, from observing facts proving that venereal maladies are capable of cure by the unaided efforts of nature, were led to a more careful study of these affections; and, consulting the annals of science, could not but be struck with the vagueness and confusion which had hitherto reigned in the treatment. Hensler, Hunter, and Jourdan may justly be considered as the chiefs of this third epoch. The first, by his learned researches into the antiquity of venereal diseases, and the second, by bringing forward his new theory, commenced the present reform. Jourdan, by making us acquainted with Hensler, contributing his own observations, and arranging the principal facts physiologically, has also promoted it."

Before discussing their opinions, M. D. recapitulates, in chronological

order, the attempts made, nearly in every country, to modify the received doctrine of venereal maladies. From 1517 to 1575, Poll, Massa, Fracastorius, Fallopius, Fracacianus, Fernelius, and Le Paulmier, rejecting mercury as the general method, employed guaiacum and a rational dietetic system. From 1575 to 1774, Forestus, Senertus, de Blegny, Blancard, Musitano, Boerhaave, Ritter, Balfour, Peyrilhe assert the evils produced by mercury, and either avoid it altogether or give it in a very moderate quantity. In 1779, Cockburn affirms that all ulcers will heal under a local treatment, and that syphilitic symptoms rarely follow: he proposes that mercury should be resorted to only where constitutional affections arise. In 1782, Benjamin Bell draws a distinction between urethritis and syphilis. Hensler demonstrates, in 1783, 1st, the antiquity of primary venereal diseases; 2d, denies the necessity of a specific treatment; and, 3d, quoting Boerhaave's opinion relative to mercury, "*quod semper corpori nocet nec necessarium sæpe habetur*," objects to it as both needless and dangerous. In 1784, Hunter produced his theory, which is now almost generally adopted by the partisans of a venereal virus and of mercury; the consideration of which we shall carry forward to the natural history of syphilis. "In the result of his labours," says M. D., "we recognize the prelude to a more attentive examination of venereal diseases." We cannot follow our author in his historical survey of the numerous writings, from those of Girtanner down to those of Mr. Bacot, in proof of the practicability of curing every variety of venereal disease without mercury. For the results of the enquiry, instituted by order of the British government, into this matter, we refer to Mr. Bacot's excellent work, p. 52-3.*

That the non-mercurial method exhibits less decided success in the hands of English practitioners, M. D. attributes to the employment of purgatives, and the use of stimulants both locally and generally, and remarks that "their pretended simple treatment is far from deserving that name, and that neither their discussions nor practice are sufficiently enlightened by physiology." This harsh censure may in some instances be deserved; but, before we yield thus much to M. D., it might not be amiss to enquire what is considered as sound physiology by him, and see the conclusions to which his principles have led him. This, however, belongs to a future division of our subject; and we shall therefore pass on to the interesting researches carried on, in the same path of enquiry, by several of the continental states, since that period.

In the year 1822, the Royal Council of Health in Sweden, having been charged by the king to conduct a series of experiments upon the different modes of treating venereal diseases, and compare their results, in order to check the abuse of mercurials in a country where the climate rendered their use so dangerous, reports from all the military and civil hospitals were ordered to be drawn up annually. These reports establish the inconveniences of the mercurial system, and the superior advantages of the simple treatment: the former method has been gradually replaced by the latter, and at present more than two-thirds of the patients are treated by the simple method. In the various hospitals of Sweden, 40,000 cases have been under treatment, one-half by the simple method,

* *Observations on Syphilis.* London, 1821.

the remaining half by mercury: the proportion of relapses has been, in the first instance, seven and a half, in the second thirteen and two-thirds, in one hundred. That which the Swedish physicians term *cura famis*, "cure par la diète," is nothing more than the treatment by regimen. M. D. compares it with the French and English simple method; but it is much more rigorous, and is, in fact, a revival of the practice of Fallopius and Fernelius, or rather of Palmarius.

Dr. Fricke's experiments in the Hamburg General Hospital were first made public in 1828. In four years, out of 1649 patients of both sexes, 582 were treated by a mild mercurial course, and 1067 without mercury: the mean duration of the latter method has been fifty-one days, that by mercury eighty-five. He found that relapses were more frequent, and secondary syphilis more severe, when mercury had been given; when the non-mercurial treatment was followed, they rarely occurred, and were more simple and mild when met with. Dr. Fricke has not observed caries, loss of the hair, pains in the bones, &c. where mercury has not been given; and, in all cases of caries which have come under his care, much mercury had previously been taken. In communications to M. Desruelles, from 1829 to 1833, Dr. F. states that his practice is still attended by the same results. The four following indications are the chief points insisted upon by him in his non-mercurial treatment:—The observance of strict cleanliness; perfect repose; a rigid diet; and the employment of antiphlogistics. He has treated "more than 5000 patients without mercury, and has still to seek cases in which that remedy may be advantageously employed:" we have the means of knowing that, up to the present period, the opinions of Dr. Fricke remain unaltered.

In 1833, the French Council of Health published the reports sent in by the physicians and surgeons attached to regiments and military hospitals, in various parts of France; the general results of which are, that, although primary disease will yield to the simple treatment, in relapses and secondary affections it is not sufficient; a revulsive or contro-stimulant action is required. Sudorifics, antimony, carbonate of ammonia, mercury, and salts of gold, they admit, have proved successful; but, of all these means, the different preparations of mercury have seemed to merit the greatest confidence. "It is so much the more certain, as, during its employment, patients are excluded from all other causes of excitement, by confinement, rest, and diet; the latter carried even to extreme abstinence." Some of the reports are more favorable to the simple treatment. M. Devergie has treated, between the years 1815 and 1835, at the hospitals of Gros Caillou and the Val de Grace, and in private, more than 6000 cases. The duration of the mercurial treatment has been from eighty to ninety days; of the non-mercurial, from thirty to fifty. He is not an exclusive partisan of the simple method, but adopts M. Desruelles' principles. In a report, by Dr. Rufz, of the comparative results of the two methods in 520 cases, at the Hôpital des Vénériens, in 1830, it is stated that the cure by mercury was one-third longer than that by the simple method; but it would not be just, observes M. D., from this statement to draw conclusions too favorable to the antiphlogistic treatment, as the cases treated by the former were more serious and extensive than those submitted to the latter; and this circumstance, we are

convinced, has been too much overlooked in examining the results of comparative experiments. In Dr. Heisch's thesis on the treatment of venereal maladies, he gives an abstract of his observations at the Strasbourg Hospital, in 1834, under M. Kayser. Out of 486 cases of primary and secondary affections, (the latter occurring in a very small proportion,) a few obstinate affections only required mercury, and that in very minute doses. Since 1831, 5,271 patients have been treated by this combination of the two methods, and the number of relapses and secondary affections calling for the employment of mercury has been very small. No case of caries, and only one or two instances of exostosis, have been observed. Full reliance may be placed on these facts, as regiments remain in garrison at Strasbourg for five or six years.

From the records of the hospital at Rennes, under M. Rapatel, furnished by M. L. Desruelles, brother of the author, it appears that, between 1826 and 1835, 7,317 cases have been treated by the two methods, the results of which are carefully drawn up in a tabular form, and a number of conclusions appended. MM. Rapatel and L. Desruelles rely on the simple treatment in the majority of cases. In ulcers they have sometimes been obliged to resort to mercury, and have also found it useful in certain consecutive affections, prefacing its administration always by antiphlogistics and strict diet, and continuing these means throughout. The deuto-chloruret (oxymuriate) of mercury has been the preparation employed, combined with opium, in doses of a quarter-grain each. They also give the proto-ioduret and cyanuret of mercury, with opium.

In 1827 and 1828, M. Desruelles published two volumes of Statistical Memoirs, containing 1,312 observations, collected at the Val de Grace, from 1825 to 1827, on the two methods of treatment. The comparative results led him, in 1827, to confine himself to the simple treatment as a general method, and administer mercury only in certain exceptional cases. From that period till 1835, 8,810 cases have been under his care, and the results entirely accord with those obtained by Rapatel, Kayser, Fricke, &c., and the Swedish Council of Health above cited. Upon this experience he lays down ten precepts, in which the principles of his treatment, to be hereafter detailed, are distinctly embodied, and closes this portion of his work by disclaiming any intention to reject the mercurial treatment, which he believes, on the contrary, may be useful in certain cases, associated with the simple method, and in the hands of able practitioners.

To the above contributions to our experience of the effects of the simple treatment of syphilis, we may add a few experiments extracted from the work of Mr. Judd: his conclusions therefrom we shall notice in a future page.

"*Exp. 1.* Ten primitive venereal sores and buboes were taken promiscuously as they offered, and treated without mercury. After a lapse of three years, but two of the patients out of that number suffered from secondary symptoms; one of them presenting an eruption of lichen, and the other a set of maculæ.*

"*Exp. 2.* Ten other primitive cases were then taken promiscuously, and treated with small portions of mercury during nine days, and by the end of the first year they had furnished so many as five cases of secondary symptoms,—viz. an eruption of

* "Thirty other primitive venereal sores were also treated without mercury, and but five cases of secondary affections were the result of the whole of them."

puniceous patch with iritis; of lichen with effusion; of ecthyma with nocturnal pains; ecthyma with periostitis; and of herpes with cynanche.

"*Exp. 3.* Ten more primitive cases were taken promiscuously, and treated by keeping the patient's mouth sore during a fortnight: from these, at the end of two years, but a couple of secondary cases resulted,—viz. ecthyma with an exfoliation of an alveolar process, and lichen with a sore throat.

"*Exp. 4.* Ten fresh cases were next taken promiscuously, and each was treated by the mouth being kept sore during three weeks: from these, after two years, there resulted but one secondary symptom,—viz. a venereal sore throat.

"*Exp. 5.* Ten other venereal patients, taken promiscuously, had their mouths kept sore during a month each; and from all these, after a similar period, there resulted only two sets of secondary symptoms,—viz. iritis and lichen, with sore throat and nocturnal pains." (P. 537.)

We thus find, then, that the sum total of cases submitted to experiment in the above reports amounts to about 80,000; indeed, they might be fixed at a much higher number: and it appears that the proportion of relapses or secondary affections, where the primary symptoms have been treated without mercury, is, at its lowest estimate, reduced to *ten*, at its highest to *twenty*, in the hundred. We are perfectly willing to admit that the great irregularities which characterize syphilis, and the very different conditions under which it appears, give less force to statistical evidence applied to its elucidation, than to that of many other diseases; yet, making every allowance for data thus collected, and taking into consideration the incompetency of many of the individuals to observe accurately, and the party-feeling of others, together with the perplexities arising from the mal-administration of mercury, &c., it cannot be denied that a sufficient mass of observations remains to establish the fact that a large majority of primary syphilitic affections get well, like ordinary ulcers, under simple treatment, or even by the unaided powers of the constitution; and that, of those cases of secondary disease which do occur, although, perhaps, subject to more frequent relapses, the greater number will ultimately wear out or be overcome by the mere action of the secretory and excretory functions; thus leaving but a small remainder of inveterate instances to be combated by other means. Although a stanch advocate for the mercurial method, properly conducted, Dr. Colles thus expresses himself with respect to the non-mercurial system of treatment, of which, by the way, he admits that he has made but one full year's trial.

"We must acknowledge that the profession is highly indebted to those who have lately introduced the non-mercurial plan of treatment; for we have now not only acquired a second line of treatment for venereal cases, but, what is of the highest value, we have been released from an inveterate and deep-rooted error; from an unfounded conviction that the venereal disease could not be cured by the innate powers of the system, unless aided by mercury. I need not add that all the opinions and practices consequent on this prejudice have been subverted." (P. 319.)

II. If the simple or non-mercurial plan of treating syphilis had produced no other benefits, medical science must lie under eternal obligations to it for the light which it has thrown on the Natural History of this disease; and we shall now proceed to notice some of the important facts flowing from this source, as well as others, tending to the same end, derived from other quarters.

Before the time of Hunter, experiment and induction had never been brought to bear on the difficulties which beset the whole question of

syphilis: it being regarded by each sect according to the philosophical theory professed by it. "His invaluable treatise," to quote the words of Dr. Colles, "poured a flood of light, not only on the natural history of this disease, but also on its pathology and treatment."

The discussion as to the immediate agent of syphilis is not a mere dispute about words, nor does it involve questions alone of a purely theoretical character, since our views of its nature and treatment are materially modified by the doctrine we may hold on this subject. Whether it be termed virus, contagious principle, or irritation, its presence can only be detected in its effects. The question therefore is, Do these effects appear to be regulated by any fixed laws, in which, making due allowance for disturbing influences, we are enabled to trace a clear chain of cause and effect, and to distinguish them certainly from other diseases? Or, do they present themselves under aspects so various and uncertain as to baffle our efforts to reduce them to any precise order, group them in one family, and derive them from any one specific source? In other words, Do venereal maladies exhibit those characters which distinguish diseases arising from morbid poisons? Or are their characters similar to those produced by common causes of irritation?

The solution of this question—or these questions—will be found in the examination of the characters of the disease, which are, it must be admitted, sufficiently various. In these varieties of manifestation, syphilis differs, as in many other points, from contagious diseases in general; and this uncertain character of its symptoms, which deprives us of much of the assistance to be derived from an appeal to analogy, has been one of the most fruitful sources of dispute. While one party contends for a single contagious principle, ascribing the diversity of forms in which it manifests itself to idiosyncrasy, climate, habitude, age, greater or less intensity of the virus, mode of its absorption, &c., certain writers have attempted to trace this diversity to a corresponding variety of poisons; as to the precise number of which, however, no two of them are agreed. Under these circumstances, it is not surprising that a third party should have sprung up, denying that we have any just grounds for deriving from a specific virus of modern origin, or including under one head, an assemblage of disorders of various kinds, to which the human race has always been subject.

The first attempt to distinguish diseases of the sexual organs arising from impure connexion, from those produced by other causes, was made by Mr. Hunter, whose views were most zealously adopted by Mr. Abernethy. The division, by these two observant writers, of this class of diseases into syphilitic and syphiloid, or pseudo-syphilitic, originated principally in an impression that syphilis "had no tendency to cure itself, or that the constitution was unequal to the cure of the disease;" which is but another mode of expressing their conviction that mercury was the sole and indispensable remedy. The fallacy of this ground of distinction being now fully recognized, the so-called syphiloid and pseudo-syphilitic affections resolve themselves—(1) into cases of syphilis, which, proving intractable under the mal-administration of mercury, were allowed to get well without it; (2) forms of that disease which did not happen to correspond with arbitrary definitions; and (3) other diseases which have been improperly confounded with syphilis. The knowledge, acquired chiefly

by the medical officers serving in the Peninsula, that a large proportion of primary venereal affections were curable without mercury, modified the doctrine of Hunter and Abernethy, and led to the researches of Carmichael, Evans, and others, who were still impressed with the idea that genuine syphilis absolutely required the specific remedy. The results obtained by these gentlemen, too hastily generalized, tempted them also to lay down arbitrary distinctions which subsequent experience has not confirmed; or, rather, it has suggested the probability that several of their varieties were the same lesion observed at different stages and under different aspects.

These distinctions gave birth to the theory of a plurality of venereal poisons; an opinion which is carried to an extreme length by one of the authors under review. Mr. Judd believes that the venereal virus originated in a disorder (the pseudo-syphilis of Mr. Abernethy,) generated by promiscuous intercourse, the acrid matter of which, when brought into contact with an abraded surface, produced genuine syphilis. As this genuine syphilitic virus consists, at the present day, of the union of an indefinite variety of morbid poisons, there is no difficulty, according to Mr. Judd, in accounting for the variety of primary and secondary affections resulting from the contamination of a single primary sore.

“To illustrate this by a tangible and more familiar example,” says Mr. Judd, “first, let us suppose a person with a pimple on the glans has connexion with a prostitute; secondly, that another with a vesicle has commerce with her, and likewise a third with a pustule or an open sore. Now, we will suppose there lies a collection of poisonous fluids in her vagina, each capable of producing its own peculiar form of disease so soon as it is placed in the living body; and this is that sort of mixture that we are in the habit, from prejudice and from its being generally inoculated through one sore, of considering as consisting of but one poison; whereas, in reality, it is a mixture of poisons, or a compound amalgam of modified ones. Next, let us take vaccine lymph, and mix it with varioloid pus, and both with herpetic fluid, and then I think you may imagine you are approaching to (though wanting many ingredients of) the compound venereal virus of the present day.” (P. 149.)

Mr. Judd enumerates nine varieties of primitive affection, from “a superficial redness” to the “phagædenic bleeding ulcer,” all of which, he says, are followed by secondary disease; but he specifies no diagnostic marks by which they may be distinguished from similar local maladies arising from ordinary causes.

It remains to be seen whether the disciples of the new doctrine have been more successful in solving this difficulty, since they claim an emancipation from the prejudices flowing from the idea of a specific disease and a specific remedy. In this enquiry we shall take M. Desruelles as the representative of the class to which he belongs.

After objecting to the existing definitions and classifications; M. D. proceeds to divide venereal maladies into primary, secondary, and consecutive. The first and third correspond with the primary and secondary diseases of former classifications; the second class comprises bubo and similar affections intermediate between the two others. The second and third classes may depend on the absence of all treatment, on incomplete treatment, or on a simply local treatment of the primary affection.

The forms and characters of these diseases vary according to the organic conditions of the tissues affected, the manifestations of vital action, and the mode of application of the contagious cause. They may be

arranged under the erythematous, the ulcerative, the phlegmonous, and the vegetative forms.

The erythematous form, accompanied by purulent secretion, is (according to M. D.,) the origin of all the others, and the most frequently met with. Where the irritation is equally spread over the surface of a tissue, without concentration or confinement, it is never followed by ulcer or abscess, but by injection of the minute vessels, with an increase of the normal secretion, and generally terminates in resolution. Urethritis, balanitis, posthitis, and vaginitis, (inflammations of the urethra, gland, prepuce, and vagina,) are examples of this. When the irritation is concentrated on one or more points, insinuates itself into the sebaceous follicles, or in any way penetrates beneath the surface, small purulent cysts are developed, the pustule breaks, and ulceration is produced. But ulceration is not exclusively dependent on venereal causes, nor is it peculiar to the genital organs; and what character has the venereal ulcer to distinguish it from that produced by other causes? M. D. asserts that it has none, citing in proof of this the experiments of Dr. Fricke and others, in which ulcers, bearing all the characters of the Hunterian forms, were developed by placing, for twenty-four hours, between the prepuce and the gland, a grain of deuto-chloruret of mercury, a piece of agaric or any other irritating substance. Mercury, he continues, was once looked upon as a touchstone of their venereal origin, when that was uncertain; but a recourse to such a diagnostic in the present day would be absurd. If the history of the supposed origin of these diseases did not settle the point, inoculation has been resorted to: against this test, however, M. D. joins Cullerier and Ratier* in protesting strongly, at the same time that he avails himself of M. Ricord's experiments on the subject, which coincide, in the main, with those of Mr. Wallace, to be noticed hereafter. From all these premises, M. Desruelles concludes "that there is but one primitive kind of venereal disease, the *erythematous*; that no venereal disease possesses characters sufficiently distinct to indicate infallibly the nature of its cause; and that inoculation does not throw sufficient light on this matter to dissipate all doubt."

If the characters of syphilitic primary affections are not sufficient to enable us to distinguish them with certainty from similar affections arising from ordinary causes, neither do they afford us sure grounds for any practical distinctions between each other. The only circumstance in which all writers, from Astruc and Hunter downwards, agree is, that ulcers of an obstinate disposition, attended or followed by induration, are those most liable to be succeeded by constitutional disease; but the conclusion that the indurated form alone characterizes genuine syphilis, according to the dicta of Hunter, Evans, and Carmichael, were it true, would confine syphilis within very narrow bounds; for the genuine Hunterian chancre is now extremely rare. Hunter, in point of fact, abandoned his own definition in practice; Astruc laid quite as much stress on the epithet *contumacia* as on *callosa*, applied to chancres; and we think all candid practitioners will allow that the following opinions and definitions of Dr. Colles are much more consonant with general experience than the partially accurate but too confined views of John Hunter.

* Dictionnaire de Médecine et de Chirurgie pratiques. Art. *Syphilis*.

"Although every surgeon must admit that Mr. Hunter's description of a chancre is correct, and drawn from nature, still I believe few will confine this term, or that of primary venereal sore, to those ulcers only which answer to his description. As the result of long, attentive, and anxious observation, I should say that primary venereal ulcers present an almost endless variety of character. I would define a primary venereal ulcer to be 'one which is remarkably slow in yielding to ordinary, mild, local treatment, but which is curable by mercury, and which, if not so cured, is likely to be followed, in two or three months, by secondary symptoms, which again also are curable by mercury.' If, then, there be, as I affirm there is, an almost endless variety in chancres, how can we decide on the nature of primary ulcers, so as to pronounce some to be syphilitic, and others to be mere common sores or simple excoriations? I reply, that we are to be guided in our decision by observing, first, that many of these suspicious ulcerations cannot be referred to any class of common ulcers, as they strikingly differ from them; and, secondly, by attending to the course which these take, when not interfered with by any stimulant or caustic application, and when treated only with some mild ointment or cold water. If, under these circumstances, we find that, after eight or ten days, such ulcers show no disposition to heal, and if at the same time there be a total absence of any cause, such as defect in the general health, to account for this obstinate condition of the local disease, we may then pronounce them to be syphilitic." (P. 75.)

The secondary or constitutional effects of the syphilitic infection are, if possible, more variable and uncertain in their characters than the primary. And here we would remark, that we are more disposed to agree with those writers who place bubo among the primary symptoms, than to assign it a separate class with M. Desruelles. It is, in fact, an accidental affection, depending merely on anatomical structure and function of the parts liable to it, and neither deserves the prominent station to which he has raised it, nor the importance bestowed upon it by M. Boyer,—that of being the diagnostic mark of genuine syphilis.

As in the primary, so also in the secondary symptoms of the venereal disease, a correct knowledge of their natural course and termination was long retarded by the belief in their intrinsically destructive character, and the necessity of a specific remedy to overcome it.

"It was Hunter," as Dr. Colles observes, "who first demonstrated with clearness and precision that secondary symptoms succeed the primary at an interval of six or seven weeks, and that they are generally preceded by an eruptive fever, which is ordinarily of an inflammatory type: he also described the third order of symptoms; and all this he has done with an accuracy and fidelity which the subsequent experience of the profession has amply confirmed." (P. 3.)

Dr. Colles laments that Mr. Hunter "did not prosecute his enquiries still further; that he did not, for example, trace the full progress of secondary symptoms, or mark their natural tendency to amend, and then again to relapse, each unfavorable change being preceded by constitutional disturbance; and that he has omitted to notice the probable period of time which these symptoms occupy, from their origin to their acme, and thence to their decline." There can be no doubt that Mr. Hunter's further enquiry was arrested by the impression above alluded to; consequently, that he was not justified in withholding mercury after the distinct development of secondary symptoms. "But," says Dr. Colles, "does it not appear strange that subsequent writers have not made some effort to supply these deficiencies?"

It is true that no writer, as far as we can recollect, has done so in the systematic manner which Dr. C. requires; but we shall find that much information has been stored up on this stage of the course of the disease,

and it will be our business to enquire how far it corroborates the following observations of Dr. Colles.

"I have observed, that when the local symptoms have become fully established, which is probably in two or three weeks after they make their appearance, they then become stationary, and the constitution is relieved from the febrile disturbance. In this quiescent state the symptoms may remain for about three weeks, they will then show a strong disposition to amend, and sometimes they will proceed so far, as to impress the surgeon with a hope, and the patient with a firm conviction, that he is about to get perfectly well. But this illusion, having lasted for two or three weeks, is dispelled in general by a fresh attack of eruptive fever, and by an eruption or sore throat, although sometimes other symptoms, (iritis for example,) be added to those under which the patient had been previously suffering. How long the disease might continue in this condition of alternate improvement and deterioration, I cannot pretend to say, as I have not had an opportunity of witnessing in the same individual more than two, or at the utmost three, of those revolutions: from what I have seen of such cases, I am led to say we may calculate upon each relapse as likely to recur every third month. These remarks apply to the secondary symptoms, as observed for two, three, or four of the first revolutions. At a later, and sometimes, though rarely, in an early stage of the disease, we find a somewhat different process ushering in a new attack; thus we sometimes observe that the patient who during the four or five preceding weeks had been improving in flesh, colour, appetite, and strength, now begins to exhibit a different aspect; his countenance alters considerably, it becomes sickly, his complexion assumes a waxen hue, and he evidently loses flesh from day to day, and all this takes place whilst the patient himself is not at all aware of the change, and is still less suspicious as to its cause. In some time, however, the patient complains of night sweats, want of sleep, loss of strength, and declining appetite, so that in the course of two or three weeks he is reduced to a state of great weakness and emaciation: this downward course proceeds steadily until a fresh order of symptoms appears and becomes established."

"After an attack of this lengthened nature we seldom if ever observe a distinct eruptive or premonitory fever appear in this patient; and I may also add that the local syphilitic symptoms cease to present their characteristic signs as strongly marked as heretofore. Thus it would seem as if each eruptive fever, or rather each succeeding attack, brings the constitution into a weaker and weaker state. In the very advanced stages of the venereal disease we do not see those periodical changes; the constitution then appears unequal to any struggle, so that one continued and increasing state of debility, with slow fever and great emaciation, are conjoined to the local symptoms, while the latter also are but little disposed to undergo any change, except a slow and gradual deterioration: thus, I have sometimes, though rarely, seen a tubercular eruption, combined with pains of bones and joints, continuing during four or five years, and undergoing no material change, being at one time a little better, and at another a little worse."

"When the disease has arrived at that advanced stage in which the general health has been broken down, and the local symptoms are so much changed as scarcely to be recognized as venereal, it is still a matter of uncertainty how long the patient (if not relieved by art) is to drag on a miserable existence, or in what manner a termination is to be put to all his sufferings. Many of its unfortunate victims are destroyed by what may be considered a continuation of the disease; but by far the greater number appear to be carried off by other complaints to which we may presume they were naturally disposed, or to which they were rendered liable by the very weakened and reduced state of their general health."

"From this account then it is very evident that there is no certain period after the appearance of the primary disease at which death determinates such protracted cases of syphilis. We may say that in general this event takes place between the second and fifth or sixth year." (Pp. 8, 10, 11, 12.)

In proof of the naturally protracted duration of the disease, Dr. Colles selects two strongly-marked cases from many of a similar character, (see

p. 194,) and in his Chapter on the administration of Mercury, where he ascribes the masking of the syphilitic symptoms to a mismanagement of mercury, or to its timid employment, he adds that although a very few may get well under the non-mercurial plan "many more have been allowed to sink into an untimely grave, by the slow and silent, but certain operation of the venereal disease, the symptoms of which have become so changed or masked that common observers could not recognize the features of the original case." Dieterich refers the masked syphilis of the writers of the sixteenth and seventeenth centuries to the abuse of mercury, and not to a combination of the effects of mercury with the syphilitic poison, an idea entertained by Hunter, but in which, as Dieterich shows, Paracelsus had preceded him. His arguments to prove this, however, are far from satisfactory, since there is, unquestionably, a marked distinction, between the diseases arising from an undue administration of mercury for the cure of syphilis, and those common among workmen exposed to the fumes of mercury, as well as the effects produced by its too copious use in other diseases. "Since 1816, (says Oesterlen,) Biett has annually passed in review from five to six hundred individuals impregnated thoroughly with mercury in the prosecution of their business (gilders, pewterers, &c.) and has never remarked in them the slightest affection of the bones, &c. They come in crowds to the Hôpital St. Louis in order to use the steam-baths for the relief of mercurial palsy."

That this masked syphilis, as a natural character of the disease, is so common as Dr. Colles believes, we are very much inclined to doubt. Where the health is undermined, and death is produced by morbid changes in the vital organs, supervening on protracted syphilis, we think the explanation of Mr. Hunter more rational, viz. that "the venereal disease often becomes the immediate cause of other diseases by calling forth latent tendencies into action."

Every person, as Dr. Colles observes, who has paid even moderate attention to the secondary forms of the venereal disease, will admit the accuracy with which Mr. Hunter has described the order of parts successively attacked. Exceptions to his rule occasionally occur, which derangement, Dr. C. ascribes, in some cases, to the mal-administration, in others to the excessive use of mercury. The form of venereal ulceration of the throat, described by Hunter, "may be looked upon as the type of the genuine venereal sore throat; still we must not rely (solely,) on the present appearances, however strongly marked, but should trace back the previous history, and enquire into the treatment of the primary disease;" and this advice is applicable to the management of all varieties of secondary syphilis.

A vast variety of ulcerated throats, in a descending series, will be seen between this Hunterian sore throat and the aphthous-looking ulcer, frequently the appearance put on by the relaxed sore throat. These varieties depend, doubtless, on idiosyncrasy, treatment, &c. and the surgeon must exercise his judgment in their management. Unless we take the history and previous treatment as our guide, we shall find it difficult to distinguish between many of these affections of the throat, ulcers and fissures of the palate and tongue, ozæna and iritis, arising from syphilitic infection, and those from scrofula and other causes.

With respect to venereal eruptions, Dr. Colles does not believe that

they are at all characteristic of distinct forms of syphilis; 1st, he has not been able to trace back particular forms of eruption to particular forms of primary ulcer; 2d, he has not unfrequently observed varieties of eruption existing together in the same individual, the papular and pustular, for instance; 3d, after the removal of the first eruption by mercury, or by other means, the second crop will frequently prove of a different kind; thus the papular will be succeeded by the pustular variety; and, finally, any form of eruption may be converted, by injudicious treatment, the excessive use of mercury in bad habits for example, into one which is most obstinate and severe, that of rupia, namely. In proof of this irregularity, there is no lack of evidence. Speaking of Mr. Carmichael's classification, Oesterlen observes, that "experience shows that no constant connexion exists between his primary and secondary affections." He refers to Hennen's work, particularly to a case in which nearly all Mr. Carmichael's varieties of eruption followed a Hunterian chancre, and quotes his conclusion, which is similar to the opinion expressed by Mr. Samuel Cooper, that "from one and the same ulcer may follow eruptions of an entirely different character, whilst from totally different ulcers may proceed one and the same affection of the skin." Mr. Bacot also shows the contradictions and inconsistencies of this classification, and adds that contemporary practical surgeons have sought in vain to follow it at the bed-side. "Biett, notwithstanding all his acuteness, (says Oesterlen,) has found the classification and marked diagnosis of syphilitic cutaneous affections a most difficult matter; else why should the appearances at different stages form the ground of his classification of all other diseases of the skin, whilst the source alone forms that of venereal eruptions, which are thrown together under the term syphilides? Cazenave and Schedel also in vain strive to demonstrate the possibility of a sure diagnosis."

These objections to classification, with our present ignorance of the laws to which syphilitic consecutive affections are subject, apply, at least with equal force, to that of Mr. Judd. In the face of them, however, he insists, at p. 171, "on nature's universal plan of formation and reproduction by the like of its kind," and is of opinion "that the primitive pimple, vesicle, pustule, or sore, (if it contaminates,) invariably leads to and produces an eruption of a similar kind, and that this sort of virus, after connexion, again produces eruptions in others of a like sort, and so on ad infinitum." At the same time, he admits that increased degrees of irritation will convert a primitive pimple into a vesicle, and the latter into a pustule,* which modifying influence aids him further in explaining apparent discrepancies with his theory. Having estimated the primitive syphilitic affections at nine, he divides cutaneous eruptions into the same number; the following passage reveals the process by which this problem was worked out.

"In limiting the number of sores and primary affections to about nine, (but without including their modifications,) I do imagine that I have come very near to the true number met with in England, Scotland, and Ireland: for, from the numerous drawings I had made of sores, and sketches of various venereal eruptions, and from the

* Biett's experience is opposed to the opinion that primary sores originate ordinarily by a vesicle or pustule; Cazenave states that they commence "the most commonly by a red spot, a true ulcerative inflammation, without elevation of the epidermis."—REV.

many manuscript accounts I have collected during a period of near twenty years, I can assert with truth, that on summing up all the various appearances of the sores, and comparing them and the number of the eruptions, I actually found their numbers exactly corresponded. At this I rejoiced, and hailed it as a happy criterion, showing that I had approached as near to the true number as the difficulty of the subject, the features of sores, and the variety of cutaneous affections probably admit of." (P. 185.)

However gratifying this coincidence may have been to Mr. Judd, the whole classification, it is plain, is perfectly arbitrary; if we are to derive each variety of secondary from a corresponding primary affection, and both, with the author, from a distinct poison, there is no reason why they should be limited to nine, or even twenty, since the varieties of both forms are sufficiently numerous to warrant it; at any rate, the practical results would still be much the same.

Mr. Babington declares the variety of venereal eruptions to be so great as to baffle description. The more distinct forms, he thinks, may be arranged under the four heads of Tubercles, Lichens, Psoriasis and Lepra, and Rupia. The most important practical point, as nearly all writers agree, arising from these distinctions, is, that rupia and ecthyma are met with in conjunction with a highly dangerous general depression of the health, and require the greatest caution in their management.

On the subject of nodes, pains in the limbs, hectic fever, and the later phenomena of syphilis, we shall speak when treating of the use and abuse of mercury, from which cause they more frequently result than from the unmixed action of the venereal infection.

On one class of venereal disorders, congenital syphilis, namely, which differs in some particulars from the diseases with which we have hitherto been occupied, Dr. Colles furnishes much interesting information. "The phenomena, he observes, of the syphilis infantum, or hereditary form of the disease, deviate in so striking a manner from those of the disease as it affects adults, that some authors have not hesitated to deny its venereal origin." Most writers are now agreed in the opinion that it is communicated by the mother during utero-gestation, and not during parturition; consequently, that it is an instance of the transmission of secondary syphilis. The disease may exist at birth, or may not appear for some days, or weeks, after birth. It is indicated by copper-coloured spots about the anus and genitals, running into excoriation; a peculiar shrill, hoarse voice; ulcerations and excoriations of an aphthous character at the angles of the mouth, and on the tongue, palate, and throat; and, in its more advanced stages, obstruction of the nose, emaciation and a senile expression of countenance, enlargement of the glands and general cachexia, leading on to death. The child may also be infected after birth, by a nurse suffering under syphilitic ulceration of the nipple, or by its mother under the same circumstances, if the disease of her nipple has been derived from a strange child; but no instance is known of a child infecting its own mother, although it will immediately communicate disease to a strange nurse: of all these circumstances Dr. Colles relates instances.

The infection of the nurse manifests itself in an ulceration of the throat, which cannot be distinguished from that succeeding primary disease, cutaneous eruptions, and the formation of moist excrescences about the pudenda, which are capable of infecting her husband, in whom they are, likewise, followed by constitutional effects. In him the ulceration of

the throat has not the same true venereal character; it is superficial, and its surface is covered with patches of whitish lymph. Dr. Colles believes that the disease may be further imparted to other members of the family by contact, use of the same utensils, &c. for it is remarkable, he adds, that its contagious property increases in proportion as it extends further from its source. Its symptoms, in different individuals, bear an exact resemblance to each other, which is not the case in the ordinary syphilis of adults. In this third remove, too, from the source of contagion, it is permanently fixed in the parts it first seizes and is of a much milder nature. Hunter believed that secondary affections could no longer infect; but, as Mr. Babington justly observes, "it is impossible to admit Hunter's argument that secondary syphilis never contaminates. The facts are so well established, that it is more easy to question the principle than to doubt the facts." No one, who has seen much of venereal diseases, will now dispute this. An interesting case of disease thus produced is given by Dr. Colles (p. 265), the symptoms of which tally remarkably with those of the disorder communicated to the nurse by a diseased infant. Whether the contagious principle of the disease passes through several grades of decreasing intensity, as Dr. Colles is disposed to believe, or whether the peculiar deviations, described by him, depend upon complications, or modifying causes, of which we are ignorant, we shall not attempt to decide.

On the whole, then, we think a sufficient analogy with specific diseases has been made out to justify us in classing venereal maladies among them. "This circumstance alone," observes Oesterlen, "bears us out in deriving syphilis from a specific source; that a certain form of disease, if it does not reproduce precisely the same form in the next individual, will do so in the third or fourth." Although this author will not venture to decide the question of the unity or plurality of the morbid agent in syphilis out of the vast mass of conflicting testimony he has sifted, the above argument, as well as the preponderance of authority throughout his work, is distinctly favorable to the belief in a unity of poison. That this one virus undergoes modifications in its course is, also, rendered very probable by many of the facts we have detailed. If the results of Dr. Wallace's experiments on it, by inoculation, are verified, and there is no reason to doubt their accuracy, we may set this down as positively determined.*

Assuming, then, a morbid poison as the immediate cause of syphilis, in opposition to the doctrine of the Broussaisists, to what does our actual knowledge of the remaining chain of effects, which constitute the natural history of the disease, amount? Examining the links of connexion, we shall find them placed, with occasional exceptions, in the following order: At an uncertain period, within given limits, after intercourse with a person capable of communicating infection, of which power the results are often the only absolute proof, an erythematous excoriation, or an ulcer, may appear on the sexual organs. This excoriation or ulcer, termed the primary affection, does not, *per se*, exhibit characters which will *infallibly* distinguish it from similar affections arising from ordinary causes. Its syphilitic character is to be inferred, with more or less probability, from

* See his excellent Lectures in the *Lancet*.

its aspect and an intractable disposition, which probability is much increased by the foregoing suspicious connexion, but can only be positively established by the subsequent occurrence of certain other affections, termed constitutional, which develop themselves comparatively seldom, but often enough to authorize us to conclude that a relationship of cause and effect exists between them. These constitutional symptoms have the property of affecting certain parts in a regular order of succession, not, however, constantly followed, or of breaking out in the same parts, with more or less frequency according to the peculiar constitution, habitude, management, &c. If these modifying circumstances are unfavorable, the symptoms proceed, in a series of recurring exacerbations, from bad to worse, till they are arrested by the only remedy which has control over them, or till death from cachectic fever and exhaustion, attended, sometimes, by other morbid complications, closes the scene: but, in the majority of instances, these unfavorable modifications not being present, the constitutional affections gradually wear themselves out, a consummation which may be considerably forwarded by rigid attention to Hygienic rules, and a judicious administration of mercury.

III. It is fortunate that upon the most important branch of the subject, *the Treatment*, the opinions of practical men do not vary so much as on theoretical points; and there is good reason for the belief that the debateable ground is daily becoming narrower. Allowing for shades of difference, two distinct systems of treatment, only, can now be specified. According to the first, both primary and secondary symptoms are invariably treated, under ordinary circumstances, by simple means, and mercury is kept in reserve for obstinate and intractable cases alone. The second method takes as its principle, that the occurrence of constitutional affections ought never to be permitted; and to prevent this, mercury is given at the onset, and continued until the surgeon feels perfectly secure that there is no danger of such a result. Into some details respecting each of these systems, we now propose to enter, in order to ascertain, if it be possible, which of them has the best claim to the merit of removing syphilis *tuto, cito, et jucunde*.

1. The simple method of treating syphilis, is divided into the internal or medical, and the external or surgical. The former consists of two parts; the observation of certain hygienic rules, and the employment of general therapeutic means. The diet must be light and mild,—meat, and all stimulating viands retarding the cure; even with the lightest diet, the hunger must never be quite appeased. As the cure advances, the quantity may be increased, but not until it is complete is even a gradual return to the ordinary regimen to be allowed. In proportion to the intensity and duration of the disease, to the youth, vigour, and excitable character of the patient, must be the austerity of the diet. Diluent beverages, decoctions of barley, liquorice, and linseed, alone or mixed with milk, should be taken freely; to the amount, indeed, of several pints each day. In summer, lemonade, orangeade, &c. may be substituted. Perfect repose must be ensured by confinement to the bed. Constipation must be obviated by the use of emollient clysters or mild laxatives. Purgatives, however, require caution in their administration. The air should be pure, and maintained at the same temperature; this is

an indispensable precaution in chronic, consecutive, and mercurial affections. Exercise is only to be recommended in the convalescent stage; it is then a valuable auxiliary, combined with country air. In chronic syphilis, it may be often carried to fatigue with advantage. As the passions frequently lead to an aggravation of venereal diseases, by interfering with the necessary calm and repose, tranquillity of mind must be strenuously insisted on. Tepid baths, repeated every four or five days, are always attended with benefit. General bloodletting is often required where the primary disease is intense, or the system excited, and the patient plethoric; but is injurious when indiscriminately adopted.

In instituting the simple external or surgical treatment, strict attention to cleanliness, and the position of the diseased parts, is never to be lost sight of. Emollient decoctions as fomentations, or dressings of simple cerate, are the best applications; and the dressings should not be too frequently renewed. Leeches are generally necessary, and are more serviceable when applied on the diseased part than in its neighbourhood. A succession of leeches, few in number, but frequently repeated, so as to keep up an almost constant flow of blood, (*saignée permanente*), produces the most beneficial results. In cases of ulcer their application is most efficacious to the ulcerated surface; and the inconvenience occasionally produced by the leech-bites taking on the same action, is thus avoided. The same rule applies to their use, in consecutive ulcers of the nasal fossæ, and soft palate. The greatest benefit is derived from the external use of a concentrated solution of opium (in the proportion of about 3j. to ʒj. of water); it soothes excessive irritability in all cases. M. Desruelles has frequently seen vegetations entirely removed by it. When the suppuration is moderated, and the surface of the ulcer cleansed, stimulating dressings, consisting of solutions of the sulphates of alum and copper, the nitrate of silver and subacetate of lead, favour cicatrization.

Although the simple treatment, traced out above, suffices generally for the cure of primary and consecutive venereal affections, it is not the sole treatment by which they may be combated. But, whether the revulsive method be employed in the first instance, or resorted to at a later period, it is established, says M. Desruelles, by the fullest experience, that the simple treatment should form the basis of a rational practice; otherwise, the patients are exposed to more serious dangers than the venereal affections themselves.

The mercurial treatment of syphilis is regarded as revulsive. This may be either external or internal. M. Desruelles is opposed to frictions, whether by the mercurial ointment or that composed of the proto-chloruret and deuto-chloruret of mercury. Baths, containing the deuto-chloruret, are dangerous. He objects to fumigations by cinnabar generally and locally, that they irritate the skin, and that the amount of absorption cannot be regulated. Frictions and fumigations, under proper precautions, are still occasionally advisable.

Among the mercurial preparations exhibited internally, the black oxyde, or soluble mercury of Hahnemann, has numerous partisans in Germany, and often produces good effects. The proto-chloruret is given, at the Val de Grace, combined with extract of cicuta, in pills containing, of each, one grain. At the commencement one or two pills are given every

morning, and their number is gradually increased, up to twenty, twenty-four, or thirty daily. The medicine acts by inducing diarrhœa and salivation; and must be abandoned if the former becomes profuse. It is most useful in chronic inflammation and ulceration of the throat, and in chronic enlargement of the testicles. United with opium, and superadded to the antiphlogistic treatment, it cannot be too much recommended. Biett employs the same treatment in secondary syphilis, taking care, however, to avoid salivation.

M. Desruelles administers the deuto-chloruret in the forms of tincture and pills. Of the first he gives a portion containing one-eighth of a grain, combined with three-eighths of a grain of opium, in decoction of marshmallow, every morning, and gradually increases the dose to half a grain of the oxymuriate, and three-quarters of a grain of opium; continuing it according to its effects. The pills contain a quarter of a grain of the former, and half a grain of the latter, and the dose is increased to double that strength. This preparation is particularly efficacious against ulcers with a hard base, which have resisted the simple treatment, or have been but imperfectly modified by it. Ioduret of mercury was proposed, in 1820, by M. Coindet, in venereal affections complicated with scrofula, and has been since taken up by Biett, as a remedy in chronic and constitutional syphilis. The proto-ioduret and deuto-ioduret are more stimulating than the chlorurets, and demand the greatest caution in administering them. The cyanuret of mercury has been recently again brought forward by Parent, who believes that venereal diseases disappear more rapidly under its influence than under that of any other mercurial preparation. He employs it in the forms of tincture, solution, pills, gargle, and externally in an ointment. Its effects are more prompt and energetic than the oxymuriate, and it is not so easily decomposed.

In his practical reflections on the action and use of mercury, appended to this portion of his work, M. Desruelles observes, that, although he is now convinced, that tumefaction, and induration of the periosteum and bones, may be produced by the venereal irritation alone, yet, those who abstain from mercury altogether, or to whom it is exhibited with great reserve, will rarely be attacked by such symptoms. "No conscientious observer at the present day, however, would venture to assert, that the simple treatment will prove a perfect security against affections of the periosteum and bones." He admits that certain ulcers, as those known by the name of the Hunterian chancre, the phagedenic, and such as leave an indurated cicatrix, are more frequently followed by consecutive symptoms, when treated simply, than when mercury has been properly used. He therefore adopts the following line of practice: in the early stage, if there is no reason to believe that the venereal influence has spread beyond the genital region, he insists on the strict observance of the simple treatment, and prohibits mercury. On the other hand, after having quieted the general system, if the ulcers have existed for twenty or thirty days, if their period of incubation has been short, and they maintain an unhealthy aspect in despite of the simple treatment, he has recourse to mercury, preferring the forms mentioned above.

Consecutive ulcerations of the soft palate and nasal fossæ he treats by antiphlogistics, merely as a preparation for the "revulsive" treatment under which he has found them most tractable, namely, the pills of

calomel and cicuta, with antimonials; and, as local means, astringent and opiate gargles, and the solution of nitrate of silver. When these diseases coincide with pains and affections of the bones, the macerating practice, "cure par la faim," is indicated, and mercurials contra-indicated: opium and extract of henbane are preferable to it; yet M. D. has known exceptions to this rule, in which such affections have vanished as if by enchantment under the influence of mercury.

The objection raised against the efficacy of the simple treatment, on the score of its being attended by a greater number of relapses, M. Desruelles affirms to be unjust. As they occur after every method of treatment, the comparative frequency can only be determined by an appeal to facts, which, he contends, are in favour of the simple treatment. He concludes the enquiry by the following recapitulation of the opinions of M. Cullerier regarding the new doctrine, extracted from M. Lucas Championnière's practical researches on the treatment of syphilis.

"1st. That the relapses, after the employment of the simple treatment regularly administered, are extremely rare; but that they occur at a very early period after the primitive infection; 2d, that those after primitive symptoms, abandoned to themselves, or of which the cure has been accelerated by cauterization, are not rare, but that in general, they are not very serious; 3d, that the relapses after incomplete mercurial treatment, are very common; and that consecutive symptoms of all kinds and of all degrees of severity manifest themselves at every period; lastly, that the relapses among individuals who at every appearance of primitive symptoms have undergone a mercurial treatment in a manner the most complete, amount to a fourth part in the sum total of those he has observed; that they are excessively severe, and, almost always consist of affections of the fibrous and osseous systems, chronic tubercular affections of the skin or extensive ulcerations of the mucous cavities."

To the examination of the claims of other reputed anti-syphilitics, M. Desruelles devotes a section. He believes that gold does, unquestionably, possess a high modifying power; and that in all cases where the nature of the syphilitic affection, and its long action on the constitution call for such an influence, prompt and energetic in its effects, gold may be advantageously resorted to.

Guaiacum is little employed at the present day, except in combination with sarsaparilla, which is preferred to it. The latter forms the basis of all the pharmaceutical preparations which are now recommended against chronic venereal maladies and mercurial affections.

Those who seek for further information on any or all of the remedies which have been proposed as anti-syphilitics will find it in the work of Dr. Oppenheim, of Hamburg, the sixth on our list. In his appendix Dr. Oppenheim refers to the advantages derived from the internal use of iodine in many cases of ulceration of the throat; and as this medicine, in one of its forms, has lately acquired some celebrity as an anti-syphilitic, we shall here say a few words on the subject. The author of one of the works at the head of this article, Mr. Judd, announces, in his title-page, a substitute for mercury, which proves to be the hydriodate of potash, administered internally in doses of five grains, continued, for some time, two or three times a day. Three cases, two of them chronic syphilitic affections attended by nocturnal pains, treated in 1832-3,—the third a cutaneous eruption, constitute the amount of his recorded experience in the successful employment of this remedy; and yet he observes regarding it, "a few more such cases in addition to those

already detailed, are all in which the author has as yet employed this new remedy for venereal disease: but if its curative properties in syphilis are such as they appeared to be in the inveterate disease of D—— N——I, perhaps no future loss of nose or palate will be permitted to occur in England:” a sanguine conclusion this, from such slight premises. Mr. Judd, might, we think, have endeavoured to strengthen the claims of his remedy, by some allusion to the experiments of Dr. Wallace on its effects, which have been from time to time detailed in the *Lancet*. In a Lecture, delivered January 19, 1836, (see *Lancet*, February 6,) that gentleman tells his pupils, “This makes the 124th case of secondary syphilis which I have so treated and carefully noted. Two years and a half have now passed since I commenced the investigation, and I have collected as great a body of facts as have ever, perhaps, been collected respecting the treatment of any one chronic disease by a particular remedy.” In much earlier lectures he brings forward his opinion of its value and mode of action; therefore Mr. Judd has as little excuse for his silence with regard to Dr. Wallace’s labours, as claim to originality in his proposal.

That the beneficial effects of iodine in a cachectic state of system are very great, and that, in the form of hydriodate of potash, it can be more conveniently exhibited than in that of tincture, is not to be questioned; but that, therefore, it is to be hailed as a specific in syphilis is very far, we think, from being established.

2. Before entering upon the consideration of *the mercurial treatment*, properly so called, it may be well to enquire what the advocates of this system advance generally in its defence.

In the first place, then, although fully admitting the poisonous effects of mercury improperly administered, modern mercurialists deny that it is either just or honest, on the part of the advocates of the new doctrine, to accuse the remedy itself of the evil consequences which have arisen from its indiscriminate and profuse employment,—from its abuse, in short. Commenting on this injustice, Dr. Oesterlen exclaims pathetically, “Ingratitude is the world’s wages,” mercury, too, may sigh: “because they have misused me, I now have to bear the blame!” “Nothing,” said Mr. Hunter, with something of the same feeling, “can show more the ungrateful and unsettled mind of man than his treatment of this medicine.”

Secondly, in answer to the charge that the employment of mercury for the cure of the primary affections will not secure patients from the subsequent eruption of secondary syphilis, or in the still stronger language of M. Desruelles, “l’usage du mercure a la plus grand part dans la production des symptômes secondaires,” they say that it is only applicable to its imperfect employment: confident in their principle that mercury, systematically administered during the primary stage of the complaint, will prevent its further development, they look upon the occurrence of secondary symptoms as a proof of the inadequacy of the treatment of the primary affection, and as somewhat discreditable to the practitioner who conducted it. Nevertheless, they do not adhere to this principle as absolute; since constitutional affections, under certain circumstances, will follow in despite of any method of treatment: but these are sufficiently rare, say they, to authorize us to regard them as excep-

tions to the rule, and not as subversive of it. What this systematic exhibition of mercury consists in, we shall now proceed to enquire; and as we look upon the observations of Dr. Colles as the most precise and practical that have yet appeared on the subject, we shall turn to his work for our chief materials.

At the same time that he dissents from the rigid discipline insisted on by our ancestors during a mercurial course, Dr. C., contrary to the opinion of Hunter, and coinciding thus far with the views of the partisans of the simple treatment, urges the necessity of some measures preparatory to the exhibition of mercury in syphilis, believing that much less mischief was produced by the old system than that which accrues from the laxity of modern practitioners in this respect. "Not only may this powerful medicine disappoint our expectations and occasion serious evils when its use is commenced under certain morbid conditions of the system, but the same may happen in the young and healthy if they are hurried into a mercurial course without any preliminary attention to their condition, or without any particular instructions as to their mode of living." Dr. Colles does not specify the measures which he considers necessary, but we may gather from various parts of his work, that these consist chiefly of moderation in diet and regular habits of life, with strict attention to the proper action of the secretory and excretory functions.

Before entering into the particulars of his mercurial treatment, Dr. C. offers a few observations on the curative action of mercury, which he believes to be almost always coincident with its affection of the salivary organs; "it is surely, therefore, (he says,) a fair and legitimate conclusion to affirm that ptyalism marks the salutary operation of this mineral." He is anxious, nevertheless, that it should not be imagined, from the foregoing statement, that he is disposed to measure the efficacy of mercury by the amount of salivation which it excites. A slight increase of secretion, accompanied by swelling and superficial ulceration of the gums, is all that he aims at effecting; and this he regards as indicating, first, that the mercury is acting in a salutary manner upon the system; and, secondly, that it displays a degree of power which will suffice to eradicate the disease.

Dr. Colles, then, considers moderate ptyalism as essential to and the test of the beneficial operation of mercury. A timid use of the medicine, that is to say, either a great diminution in quantity, or an absolute abandonment as soon as salivation comes on, (at which time a chancre will frequently put on a more unfavorable aspect,) is worse, both in primary and secondary syphilis, than proscribing its use altogether; as, by this proceeding, the health becomes disturbed, and the local disease takes on a stubborn character, which is not soon removed; and, where secondary disease exists, the symptoms become masked.

"The true rule by which we should guide our practice is this: to desist from the further use of mercury in the advanced stage of a mercurial course, as soon as we perceive a decided change for the worse in the ulcers (or rather symptoms). Let us not confound together the bad changes which take place in the onset of mercurial treatment before this medicine has taken full hold of the system, with those bad changes which we occasionally witness in the advanced stage of the course, when the mercury has for some time had full power over the system." (P. 172.)

When mercury does not produce salivation at, or soon after, the usual

period, but its effects are confined to ulceration of the gums and fever, the "dry course" of the lower orders in Ireland, Dr. Colles has often succeeded by reducing the doses and lengthening the interval between them. In whatever form the remedy is used, the general rule attempted to be followed is, to induce ptyalism, without any severe constitutional disturbance, within from seven to ten days. If salivation is suddenly excited, the curative action of mercury is not to be relied on; the primary disease may be removed by it, "but secondary symptoms will not fail to make their appearance in full vigour. After such sudden and profuse salivations, (adds Dr. C.) I have uniformly found the symptoms very unmanageable, and by no means yielding in the usual way to a subsequent use of mercury." The first six or eight days, therefore, being the most critical period of the course, during that time great caution must be enjoined, as he is convinced that many of the failures in the mercurial management of syphilis arise from inattention to this important rule. When once the system has been brought fairly under the influence of mercury, there is then little to fear from its protracted use, provided it is not allowed to pass beyond the line already pointed out, and the state of the general health is not neglected.

Dr. Colles gives a clear account of the various idiosyncrasies observed in regard to the action of mercury, and points out the different morbid affections produced by it. One of the most unerring proofs that the medicine is too powerful in its operation relatively to the constitution of the individual, is the occurrence of excoriation round the anus. Mercurial erythema is only to be apprehended at that period of the course when ptyalism is *first* excited. It must, therefore, be a rule to watch for it then, and to discontinue the mercury instantly it appears. There is no danger of this affection at any subsequent period. Mercurial crethismus, on the contrary, only arises late in a course, and generally comes on when an increased dose has been given to excite salivation, or when that has been allowed to subside and we are endeavouring to reproduce it. The author is perfectly confident that it will not attack any person in a state of moderate ptyalism.

Dr. Colles's treatment of the individual forms of syphilis is marked by his usual practical good sense. His opinion that the rapid healing of primary sores under stimulants is no security against consecutive affections, has been already noticed. In the management of the true Hunterian chancre, the local applications ought to be of the blandest nature, so as not to alter the natural features of the ulcer; from observing which, he is convinced that so many useful indications to guide us in the administration of mercury, may be drawn. On the other hand, he believes that the occurrence of secondary symptoms is not prevented by the early application of caustics, or even by cutting out the chancre, of the failure of which he has seen very decided instances. He does not agree with Hunter in the opinion that the circulation may be long loaded with mercury before the chancre becomes affected by it. Ordinarily the constitution is brought under its influence between the third and eighth day; and he feels no hesitation in saying, that as soon as this takes place, we shall observe a striking change in the appearance of the ulcer. The first remarkable change is, that the chancre appears a little larger, but, at

the same time, less deep; we next find that the surrounding hardness declines, that granulations begin to arise, that the discharge becomes purulent, and that the entire surface of the ulcer becomes clean and red. In a few days more, the ulcer contracts, and a thin cuticle forms on its edges, and this daily increases until it is finally healed. Some degree of hardness, however, remains for several days after it has healed, but this also disappears and leaves the part possessed of its natural softness.

"I shall only add, (pursues Dr. C.) that, in general, it will be prudent to continue the use of mercury not only until all hardness be removed, but even for a few days longer. I think we may lay it down as a rule, that the course of mercury, even when it has been well conducted and has agreed with the patient, should be continued, to the extent of moderate pyalism, for not less than one month."

The advice given by Mr. Judd on this point is the same.

"If mercury be given for the removal of primary contamination by venereal virus, the use of that mineral should be continued from twenty-four to thirty days after the gums become affected by the remedy; and an adequate quantity should be taken to keep up a peculiar coppery taste in the mouth, especially on waking in the morning, and also to cause a slight but steadily continued tenderness and redness of the gums, perceptible to the eye on inspecting that portion supporting the incisores teeth. If a less quantity than that be administered, the patient will run all the risk of injury, without deriving the benefit of security: on the contrary, absorption will have been promoted, and in many instances cause the secondary symptoms—those direful affections which we are called upon either to prevent or remove." (P. 555.)

We shall not follow Dr. Colles through his details relating to the treatment of secondary syphilis under ordinary circumstances. The same principles apply to this as to the primary affection, with the distinction that in this form the action of the medicine will usually require to be kept up, in ulcerations of the throat especially, for eight or ten weeks. His observations on inveterate syphilis, relapsed sore throat, pains in the limbs, nodes, and venereal swelled testicle, must not be passed over so cursorily. We have before referred to his opinion that the injudicious use of mercury, or the misconduct of the patient during its exhibition, has, still, a large share in inducing some of these pernicious consequences and in rendering many of them most unmanageable. These cases were a stumbling-block to the old mercurialists; and it is no wonder that the terrible consequences resulting from their persistence in an unmeasured use of mercury, should have led to its recent proscription. The practice of Dr. Colles differs very little from that of the moderate adherents of the new doctrine, who, whilst they deny the claims of mercury to the character of a specific in syphilis, turn to it as the last and only resource in inveterate secondary affections. He employs it in such cases, in smaller doses and with greater precautions than under ordinary circumstances, combining with it means for the improvement and support of the general health; and he finds his principle of treatment, namely, gentle pyalism steadily kept up, here, also, completely borne out by success. In venereal hectic fever, which has hitherto been looked upon as a state peremptorily forbidding the use of mercury, Dr. C. is convinced that it may be administered not only with safety but a certain expectation of effecting a cure. He begins it, under these circumstances without preparation, in minute doses, prescribing *Pilul. Hydrarg. gr. iij. with Extract. Cicutæ gr. ij.*, to be taken once or twice a day, combined with *sarsaparilla*.

In extreme cases, Dr. C. disregards the two maxims not to give mercury during febrile excitement nor whilst an eruption is coming out; but the state of eruptive fever, as well as that of hectic and exhaustion, require that the mercury should be employed in a manner peculiarly suited to such conditions.

"Were we to use mercury with these, as we do with venereal patients in general, I believe we should commit most serious mischief. In these cases we should not commence with a larger dose than ten grains of ungt. hydr. fort. every morning, or with an equivalent of blue pill; that is, about gr. iij. mane nocteque. However inconsiderable such doses may appear, still we shall be gratified to find that on the third or fourth day the mercury is acting in the most favorable and salutary manner on the system; and it but seldom occurs that these effects are deferred to the sixth or seventh day." (P. 234.)

In his treatment of syphilis in scrofulous patients, Dr. C. has no dread of the injurious effects of mercury; he deals with this complication exactly as if scrofula were not present, with this distinction that he is anxious to excite a smart pyalism with the least possible delay.

The general principle on which Dr. Colles conducts the mercurial treatment of syphilis, is amply supported by practical experience both in this country and on the continent. In Germany, Louvrier, Rust, Wedemeyer, Simon, &c. maintain corresponding views; and the minutes of the meeting at Nantes contain a respectable mass of evidence in its favour derived from our French brethren. A large majority of the speakers declared themselves for the mild mercurial treatment in both primary and secondary disease, at the same time recognizing the value of a previous and contemporaneous employment of antiphlogistic measures.

In the treatment of syphilitic diseases, then, the surgeon has the choice of two lines of conduct. He may, in the first place, endeavour to cure the local malady by the simple method, to accomplish which no better rules can be suggested than those laid down by M. Desruelles, although they form a tolerably severe system of discipline; and if, on the failure of this attempt, or the subsequent occurrence of consecutive symptoms, the specific nature of the disease is more fully displayed, he can then recur to the specific remedy.

Or, secondly, he may give mercury soon after the outbreak of the primary disease; not, however, neglecting hygienic measures; and continue it until he is satisfied that all danger of consecutive disease is at an end; by this proceeding he will be enabled, except under circumstances of rare occurrence, confidently to assure his patient that no ulterior consequences will trouble him; but, at the same time, he will have treated by a process tedious, disagreeable, and sometimes injurious, a large proportion of cases which would have rapidly got well under ordinary measures.

IV. It only now remains to examine briefly the merits of the several works at the head of the article, and the share which they have respectively had in supplying the materials for its composition.

1. M. Desruelles, the author of the first work on our list, has long been distinguished as a zealous disciple of Broussais. After serving as a regimental surgeon in the French army for some years, he was appointed to

the charge of the venereal wards of the Hospital of the Val de Grace in the year 1825. In 1827, he published a volume of memoirs on the treatment of venereal maladies without mercury, and in 1828 and 1829, contributed papers "on the comparative results obtained by the mercurial and non-mercurial methods of treatment employed at the military hospital of the Val de Grace," to the 25th and 27th vols. of *Mem. de Médecine et de Chirurgie Militaires*. The work before us contains the results of his experience from August 1827, to April 1835; during which period he treated 8810 cases of venereal disease. It is divided into two parts; the first contains a general view of the history, theories and treatments of venereal maladies; the second, a description of them in detail, with indications of the suitable modes of treatment according to the principles of the new doctrine. To the end of the work is appended a formulary of the various medicinal substances which still maintain a reputation in the treatment of these diseases; they are arranged in alphabetical order, and, under each remedy will be found the most approved formulæ for its exhibition. Nor must we pass unnoticed a copious bibliography at the commencement, in which the pages of the treatise, where any particular author is referred to, are indicated.

An elaborate sketch of the history of syphilis occupies the first 147 pages. This is, in fact, almost exclusively, a report on the rise and progress of the non-mercurial treatment, and is, on that ground, well deserving attention. Many of the early facts on the comparative merits of the mercurial and non-mercurial systems of treatment are derived from British sources, the beneficial influence exercised by them on our practical views having been already shown by Mr. Bacot in his very perspicuous monograph of these maladies. But the additional experiments, some of them conducted on so large a scale, which have been industriously collected by M. D., furnish us with much more extended and valuable data on which to found an opinion.

Any lengthened discussion of the author's too ingenious and accommodating theory, a mere expansion, as he avows, of the principles of Broussais, would lead us into a critique on the doctrine of the latter. Were it not that M. Desruelles deviates in practice from his theoretical dogmas (in which he is by no means singular among writers on syphilis,) we should be tempted to apply to him and to his coadjutors the words of M. Cruveilhier, in his inaugural discourse for 1836, "*Ce sont les métaphysiciens ou les poètes de la science, ils ne seront jamais praticiens.*"

The ease with which every phenomenon of the disease is accounted for by this theory ought to have raised a suspicion of its validity; and, doubtless, some such misgiving did cross the author's mind when he thought it advisable to state apologetically that he "attached comparatively little importance to it; but that he had traced out some hypothetical views, because it was necessary to harmonize the facts of the ancient doctrine with those of the new."

In that portion of the work entitled a consideration of "some questions relative to venereal maladies," a great mass of information on their propagation, characters, progress, and termination are brought together, showing how much has been effected within the last few years towards illustrating the natural history of the disease.

The second part of the treatise, also, contains much valuable matter.

The mercurial treatment on which he relies when simple measures are unsuccessful, both in the preparations employed and the manner of exhibiting them, scarcely differs from that followed by many sound practitioners in this country. There is, however, this material distinction between them,—that M. D. cures as many local or primary diseases as he can by the same means he would resort to in accidents where no venereal origin is suspected, reserving the more potent remedy for obstinate primary, and for consecutive affections, which latter he looks upon as extreme cases under this proceeding.

Regarded as a work of science, its great blemishes are the hypothetical views, above alluded to, and its partiality. Everywhere the author betrays an undue feeling of partisanship. An aggravated picture of the evils of the mercurial treatment is continually placed before the eye, in order to enhance the merits of the simple treatment. In his reports and examination of facts drawn from the history of the disease, impelled, no doubt, by his anxiety to “harmonize” those of the old and new doctrines, his appreciation of the evidence, and of the parties from whom it is derived, is not always quite honest. Whenever the testimony is favorable to his system, the writer is introduced to us as a “physiologiste profond,” “praticien distingué,” “médecin expérimenté,” &c. On the other hand, those who furnish evidence which does not accord with the author’s principles are sometimes stigmatized, most gratuitously, as unworthy of belief.

These drawbacks notwithstanding, M. Desruelles has displayed a talent and research in conducting his enquiry into the nature and treatment of a disease involved in so much perplexity, which entitle him to high praise.

2. As the treatise of M. Desruelles may be designated a manifesto of the new non-specific doctrines, so may that of Dr. Colles be fairly considered as embodying the opinions, theoretical and practical, of that large and, we venture to affirm, increasing class of practitioners, who still hold that syphilis is a disease of a specific nature, and that mercury is the only remedy on whose curative power we can safely and confidently rely. It is not to be compared with the work of Mr. Bacot, as a clear and compendious digest of the entire subject; in fact, that has not been the author’s aim. He has scarcely alluded to the labours of others, but lays before us a simple statement of his own views, drawn from a long and extensive experience in the management of the disease, together with certain theoretical opinions naturally suggested to his mind by an attentive observation of its various phases.

We have given so prominent a station to Dr. Colles’s opinions upon all the most important parts of our subject, that we might fairly leave the reader to form his own judgment of the merits of the work; but we feel that we have not been able, within limits so narrow, to do justice to its great practical value. In the present vacillating state of opinion on the treatment of the disease, in which neither system has fair play allowed it, we must rank as a most important accession to our knowledge, Dr. Colles’s demonstration of the different effects produced by different modes of administering mercury; its beneficial application to varieties of inveterate syphilis, which assumed so destructive a character in the hands

of practitioners of the old school, and so frequently baffle the efforts of disciples of the new doctrine, if fully confirmed, will restore mercury to its former high station, from which an indiscriminating routine practice has deposed it. The author may, possibly, feel too sanguine with respect to the general success which he anticipates from his modified treatment, but the practice of the most experienced surgeons of the present day goes far to corroborate his statements; and it is worth noting, that those who dispute the value of the system, advance no satisfactory method of treatment as a substitute for it. We therefore strongly recommend Dr. Colles's observations to the consideration of practitioners of every shade of opinion, confident that they will rise from the perusal, if not with a clearer insight into the nature of the disease, at any rate with more fixed principles for treating it.

3. *The Minutes of the Meeting at Nantes* is a thin volume of some 150 pages, which has made its appearance at a highly seasonable juncture. It is interesting as a summary, to a certain extent, of the state of opinion regarding the two methods of treating syphilis, and as a proof of the jealous scrutiny to which the new doctrine is submitted by our neighbours on the other side of the Channel.

In March, 1835, a proposition was made by the members of the medical section of the Royal Academic Society of Nantes, to those societies with which it was in correspondence, that a discussion should take place on the two questions of the existence or non-existence of a syphilitic virus, and the comparative advantages of the mercurial and non-mercurial plans of treatment. At the same time, the practitioners of Nantes were invited to assemble, and organize a regular series of meetings, at which the opinions of each individual might be made known and collected. On the 2d of July, 1835, the first meeting was held in the hall of the Hôtel de Ville of Nantes; upwards of fifty physicians of that town and its vicinity being present. Among them M. Devergie, author of the "*Clinique de la Maladie Syphilitique*," took his place, and was charged by M. Desruelles to express his regret that he could not also be present.

The discussion continued during four meetings, and was conducted in a manner highly creditable to the ability of the parties engaged in it. How great soever the differences with regard to theory may have been, as far as practice is concerned, the average opinion expressed indicates a blending together of the old and new doctrines to a greater or less extent; the principles and practice of the former have been favorably modified by those of the latter; but, in the extreme views for which they contended, M. Devergie and his companions found the strength of the meeting, both in number and argument, arrayed against them.

4. The work of Dr. Oesterlen is an enlarged edition of an Essay on the Unity or Plurality of the Venereal Contagion, to which a prize, offered in 1833, by the Faculty of Tübingen, for the best essay on that subject, was adjudged. It is divided into two parts; the first contains an examination of the question as to the identity or difference of the gonorrhœal and chancreous contagions; the second takes a general survey of the various disputed points with regard to the identity or difference of the syphilitic and the so-called pseudo-syphilitic contagions. In the prosecution of this enquiry, Dr. Oesterlen has exhibited great research

and a considerable share of discrimination. He has brought together English, American, French, and German authorities, and contrasted them with impartiality and acuteness. We have availed ourselves of the materials collected by him, whenever they had a marked practical bearing, but have carefully avoided plunging into the intricacies of this question, which, after all his labour, he confesses his incompetency to decide. Those who desire to enter more fully into the numerous questions springing from this discussion will nowhere find a more clear and comprehensive display of them.

5. The work of Dr. Dieterich is rich in lore, but, like too many of the productions of the German press, is verbose and diffuse. It contains, however, so much valuable information on the subject to which it is devoted, that we may perhaps return to it on another occasion.

6. The excellent work of Dr. Oppenheim contains many judicious views and much important knowledge, conveyed in a concise form. The comparatively old date of the publication would render any lengthened account of it in this place improper; but we recommend it to the surgical student as a valuable document.

7. In the work before us, M. Baron Boyer* sets out with the assertion that the diagnostic mark between syphilitic primary affections and similar diseases arising from ordinary causes, is the coexistence of a bubo with the former. "The development of a bubo, where primary disease exists, however small it may be, is not to be attributed to sympathy, but to the syphilitic infection; and the coincidence of bubo with inflammation of the urethra and vagina, or with an ulcer, proves that the individual has contracted syphilis." Although M. Boyer adopted this opinion three years back, he candidly acknowledges that he has not hitherto succeeded in convincing those of his friends to whom he has communicated his ideas. We doubt if he will be more successful in his endeavours to convert others. General experience militates so strongly against such a notion, that it is not worth while to attempt a formal refutation of it. Into the author's classification of primary and secondary syphilis we cannot enter. It is plain that he has laid his peculiar opinions too hastily before the public to have been able to test them fairly by experience. His matter, therefore, has not quite value sufficient to compensate for the rather dogmatic manner which he has generally adopted. He has added little or nothing to our knowledge of the theory or treatment of this class of diseases.

8. If the work of Mr. Judd is to be received by our continental brethren as a specimen of the manner in which the complicated questions regarding syphilis are treated in this country,—as our contribution, in 1836, towards their elucidation,—it will not, we fear, tend to remove the imputation cast by M. Desruelles on the qualifications of British practitioners. The very title-page is calculated to produce, at the outset, an impression unfavorable to the author, which, we are sorry to think, other parts of the work serve rather to increase. Some peculiarities in the manner in which he lays his information before his readers we ourselves ascribe purely to a want of good taste; but he must not be surprised if they excite, in the less charitably disposed, a suspicion that he is more

* Son of the late celebrated chief surgeon to the hospital of la Charité.

desirous of winning golden opinions of that "indulgent public," to whose decision he appeals in the preface, than simply of "contributing his mite towards the general good." The volume consists of two parts; the first embraces urethritis, and its consequences; the second is devoted to syphilis, and is preceded by a series of anatomical and pathological observations on the "dermoid coverings," prefatory to his classification of syphilitic cutaneous affections. We have elsewhere glanced at Mr. Judd's theoretical views, as well as his classification of secondary diseases, and noticed his "proposal of a substitute for mercury." With the exception of a few very meager observations at the end of the volume, the reader is left to glean the author's method of treatment from his tediously detailed cases, which occupy little less than three-fourths of the whole work. His chief practical deductions are, that small quantities of mercury, employed for a short period, instead of curing the disease, lead rather to contamination; and that ptyalism is not always a specific. Mr. Judd's language is sometimes so perplexed as to be with difficulty intelligible; a defect which appears to arise as often from confusion of ideas as from obscurity of style merely dependent on a want of familiarity with his pen.

9. Of the *Treatise of the Venereal Disease*, by Hunter, it is unnecessary to say much, as both its excellencies and defects have been long properly appreciated by the profession in this and other countries. Although, as Mr. Babington remarks, "tainted, more than any of the other works of John Hunter, with the vice of a too-hasty generalization, yet it is full of practical observations of the highest value, and must always form an essential part of the study of every surgeon who wishes to make himself acquainted with the disease of which it treats."

Under the hands of Mr. Babington, who has performed his task as editor in a very exemplary manner, the work has assumed quite a new value, and may now be as advantageously placed in the library of the student as in that of the experienced surgeon. We cannot characterize the improvements in the new edition with more accuracy and truth than in the concluding words of the editor's preface. "It has been attempted to render it more generally useful, by ingrafting on it the labours of other surgeons. The opinions of the author have been treated with the respect which is due to his high reputation; but, where important facts have been brought to light by others, either in confirmation or in correction of his principles, they have been presented to the reader, in order that he may have before him in one view the present state of the science, and that, by comparing the reasoning of others with that of the author, he may be enabled to form a just opinion of the truth or the error of his conclusions." It may account for the slight use we have made of Mr. Babington's notes in the present article, to state that the work, although only published in the present year, was really printed in the year 1835.

ART. II.

1. *Lehre von den Augenkrankheiten. Zum Gebrauche für practische Aerzte und Wundärzte, so wie zur Benutzung als Leitfaden beim klinischen Unterrichte, abgefasst.* Von Dr. ANTON ROSAS.—Wien, 1834. 8vo. pp. 599.
The Study of the Diseases of the Eye, for the Use of Medical Practitioners and for Clinical Instruction. By Dr. ANTON ROSAS.—Vienna, 1834. 8vo. pp. 599.
2. *Dell' Ottalmoscopia e dell' Introduzione allo Studio dell' Ottalmologia. Dissertazione del Dottore LUIGI MARCHETTI DA CREMIA.*—Pavia, 1834.
A Dissertation on Ophthalmoscopy, and an Introduction to the Study of Ophthalmology. By Dr. MARCHETTI.—Pavia, 1834. 8vo. pp. 207.
3. *De l'Emploie de l'Excision et de la Cauterisation dans l'Ophthalmie blennorrhagique.* Par E. F. JULLIARD.—Paris, 1835. 4to. pp. 88.
On the Use of Excision and Cauterization in purulent Ophthalmia. By E. F. JULLIARD.—Paris, 1835. 4to. pp. 88.
4. *The Cyclopædia of Practical Surgery.* Edited by W. B. COSTELLO, M.D. Part I. Art. Amaurosis, by FRED. TYRRELL, Esq.—London, 1837. 8vo. pp. 112.
5. *A Manual of the Diseases of the Eye.* By S. LITTELL, Jun. M.D.—Philadelphia, 1837.—8vo. pp. 255.

THERE is no fact more striking in the annals of medicine than the rapid strides towards perfection which have been made in the domain of Ophthalmology within less than the last half century; and it may be justly affirmed that the most important epoch in its history dates from the foundation of the Imperial School of Vienna. Austria has the merit not only of having been the first to establish an academy for the advancement of ophthalmic science, but of having based it on liberal and rational principles; and, among a people so distinguished for zeal and industry, no sooner was an impulse given, than men, alike distinguished as sagacious observers and faithful interpreters of nature, succeeded not only in raising from ignominious abasement this department of science, but in imparting to it a degree of precision, and advancing it to a pitch of perfection, which no other branch of the healing art can even yet be said to have reached. Schmidt and Beer stand forth preeminent among those who first attained an exact knowledge of ophthalmic diseases. Setting aside all preconceived notions and theories, they never stepped beyond the boundaries of strict observation, nor permitted themselves to enter into the visionary and delusive speculations that then pervaded German medicine. They chose neither to believe nor to assert where evidence was wanting. They knew that to paint things as they really are requires minute attention: hence, their descriptions of disease, copied from nature, have all much of the freshness and liveliness produced by actual perception. The exquisite tact they evinced in discriminating the shades of organic lesion was acknowledged by their contemporaries, and their prelections and clinics were frequented by a concourse of students from every civilized country. A death-blow was thus given to the spirit of

empiricism, which then prevailed to an immoderate extent. For it may be truly said, that, until the beginning of the present century, the prerogative of treating the diseases of the eye had been in a great measure usurped by quacks and itinerant impostors.

The impulse being thus given, ophthalmic institutions sprang up in other countries. Skilful practitioners undertook the discharge of the important duties attached to them; and, as they pursued the same line of research, they proved greatly instrumental in extending still further the useful applications of this branch of medical science.

If slow to enter on this career, and although certainly still behind Germany in the general knowledge of ophthalmological science, England must now be admitted to have been for some time actively and effectively engaged in cultivating this most interesting and important branch of surgery. The last few years have seen produced in this country more works exclusively dedicated to the pathology of the organ of vision than were published during the whole of the last century. The reason of this is obvious; this class of maladies is no longer the exclusive property of the so-called oculist, but, on the contrary, forms an integral part of the knowledge of the well educated physician or surgeon. Five and twenty years ago the profession, especially in the provinces, was avowedly and lamentably ignorant of the first principles of this branch of medical science; while at the present day, from the increased facilities afforded by the numerous ophthalmic hospitals in the metropolis and in most of the large country towns, the study of diseases of the eye forms no unimportant branch of the labours of the student, although, perhaps somewhat unwisely, it forms as yet no part of the examinations at the chartered medical colleges.

In the present Article we purpose to pass in review a few of the works on diseases of the eye which have been gradually accumulating on our hands, noticing, at any length, such matters only as appear particularly interesting or are of much practical utility.

Comprising, as it does, all the organic textures distributed throughout the body, besides some peculiar to itself, as the cornea, iris, choroid, and retina, the eye has been truly represented as a sort of miniature or recapitulation of the whole animal fabric. Thus, for example, the osseous system is present in the bony shell of the orbit; the dermoid system, in the palpebral integument; the mucous, in the conjunctiva and lachrymal apparatus; the serous, in the cornea, in the choroid, iris, lens, and hyaloid membrane; the fibrous, in the sclerotic, the sheath of the optic nerves, and periorbital investment; the vascular, in the arteries, veins, and more especially in the choroid; the muscular, in the muscles of the eyelids, eyeball, lachrymal organs, and, according to some authors, in the iris; the glandular, in the conjunctiva, the meibomian follicles, the lachrymal gland and caruncles; lastly, the nervous system, in the ophthalmic nerves and retina. It is accordingly found that the eye is subordinate to the same physiological laws, and liable to the same morbid influences, as other parts of the body. Diseases of a strictly peculiar kind cannot, therefore, be said to exist in the eye: all its affections being more or less identical with those of the rest of the organized tissues; springing from the same causes, modified by the same principles, whether general or specific, and exhibiting the same phenomena. Hence, scro-

fula, rheumatism, gout, catarrh, syphilis, the exanthemata, dropsy, blennorrhœa, present similar phases, observe similar stadia and select similar textures for their seat in the eye, as in other parts of the body. Indeed the further we prosecute this train of investigation, the more abundantly and strikingly will analogies present themselves. Every successive step will but corroborate the soundness of that scheme which teaches us to regard the eye not as an isolated or peculiar structure; and that the treatment of its diseases, in order to be rational, must repose on the same bases as that of other distempers.

I. The first work on our list, that of Professor Rosas, is rather a compendium than a complete treatise; it comprehends the pathology and treatment of most of the diseases of the eye. The arrangement of its contents is on the whole good and in some respects original, although contaminated with the besetting sins of German systematic writers, the love of infinitesimal division, the splitting of species into innumerable subspecies, and the introduction of the most elaborately-compounded words of Greek extraction to express the simplest phenomena.

The author's descriptions of diseases are generally accurate, and his rules for surgical manipulation clear and simple; but his method of treatment is often faulty, from the mass of obsolete drugs with which it is complicated.

The author commences his work by detailing the different affinities subsisting between the eye and the other organs and systems of the body. Among these the principal are, 1st, its connexion with the remaining organs of sense; 2d, with the brain; 3d, with the viscera of the abdomen; 4th, with the skin; 5th, with the generative system. Most of these sympathies he attempts to explain by the physical connexions of the nerves of the different parts, but unsuccessfully of course. We know that sympathies, for the most part, depend on a more general cause. The influence of congestion and disordered function of the abdominal viscera in determining or modifying ophthalmic affections is well known. Saburral conditions of the primæ viæ, excess or acrimony of the biliary secretion, or the presence of intestinal worms, prove the fruitful source of disease in remote organs, and, among others, of the eye. Venous congestion in the abdomen produces like effects. This is shown by the fact often witnessed that where a load of recrementitious matter has been removed by purgatives, ophthalmic diseases have promptly subsided.

The sympathies that have been traced between the organs of sight and respiration have been overlooked by the author. They are, however, often very remarkable. Jüngken tells us that he was acquainted with "two persons so habituated to the stimulus of light that its presence became indispensable towards the due performance of respiration, insomuch that they were instantaneously seized with asphyxia when they found themselves in a place from which light was excluded; and suddenly awakened from sleep with a sense of approaching suffocation if by any chance the tapers in their chamber came to be extinguished."* This case strikingly resembles one recorded by Laennec: a nobleman, in whom attacks of

* Die Lehre von den Augenkrankheiten, p. 12.

asthma invariably came on when any person inadvertently shut his bedroom door, or his night-lamp happened to go out.*

The sympathy between the visual and generative organs is often remarkable. A gonorrhœa suddenly suppressed frequently excites the most virulent ophthalmia; affections of the testis and uterus are fertile sources of diseases of the eye; and, conversely, some of the most intractable forms of such diseases yield on the establishment of the catamenia, or are quickened into activity on the cessation or interruption of that function.

In the author's history of individual diseases, many things are well worthy of notice both for their novelty and their importance. Under the head of *Neuroses* we find the following account of "Iridalgia," which, however, we must greatly abridge.

"It is a painful affection, seated in the ciliary nerves of the iris, and extending to the contiguous nerves. The individuals most liable to its attacks are those whose eyes are much engaged in reading, writing, or contemplating minute and brilliant objects; or who require to make frequent transitions from an obscure or highly illuminated place. Hence, literary men, lace-workers, printers, especially compositors, are chiefly subject to this disease. Repletion favours the supervention of a paroxysm, which is for the most part confined to one eye. The following are the symptoms. At the instant of seizure the patient finds himself dazzled, and the central point of any object that may be present to him seems enveloped in mist. He next observes a black spot, which gradually enlarges and after the lapse of a minute or two appears encircled with a transparent, coloured, or pale halo, having a zig-zag oscillatory movement. This zone, at the beginning narrow, becomes bigger and bigger in proportion as the mid-point gets distinct, and emitting sparks progressively advances to the circumference of the iris, when it vanishes. Although the eye be open or closed, the phantasm is always present, with this difference that it is more marked in twilight or in the dark. The paroxysm varies in point of duration. The spectrum lasts from a few minutes to half an hour. Up to this time and immediately after evanescence no particular pain is felt, but a sort of obtuseness, troubled sight, and weight in the head, in some instances a degree of dazzling and cloudy vision, are the sole precursors of the neuralgia, and these so trivial as hardly to attract the patient's attention.

"On the subsidence of the spectral illusion, transient darts of pain are experienced in the eye and corresponding temple; the whole eyeball next becomes uneasy, and the least pressure occasions sudden sharp pain. There is a feeling of tension, fulness, and violent compression, as if under the blow of a hammer. The greatest suffering is in the superior and internal parts of the eye, and is not uniform, but observes remissions and exacerbations, without entirely ceasing, augmenting in intensity about every ten or fifteen minutes and persisting several hours or even days. Vision, smell, hearing, taste, are all during the paroxysm dulled; the eyes are red and suffused, the pupil is contracted. Exposure to light renews or aggravates the fit. The slightest noise is intolerable. There is anorexia and sometimes vomiting; disposition to drowsiness; but the intellect remains clear."

There is nothing particular in the treatment recommended. Darkness and quietude, mild narcotics internally and locally, attention to the state of the stomach, and bloodletting if there is plethora, are the chief measures.

Dr. Rosas has furnished some useful directions in reference to the best modes of applying galvanism in paralysis of the eyelid or "Blepharoplegia." He insists particularly on the superior efficacy of the negative over the positive pole in the treatment of paralysis.

* Translation of Laennec, p. 414.

"In order to stimulate the ophthalmic nervous apparatus through the medium of the fifth pair, the negative pole is to be connected with one or other of the branches of that nerve in the proximity of the supra-orbital notch, the inner surface of the nose or upper jaw, or the temples. To secure coaptation, a somewhat concave metallic disc may be employed: it must be gilt within, to prevent oxidation, and lacerated without and over its rim, that it may not touch the skin, and must have in its concavity a bit of thin sponge, moistened with some saline solution, fixed by a band drawn through the disc, so as to compress it against the part to be galvanized, and fastened by an eye to the wire of the negative pole. From ten to twenty-five pairs of plates are required.

"The physiological effects of that modification of the application of voltaic electricity, called the 'galvanic puncture,' are as follows:—So soon as the circle is complete, or the wire of the battery in communication with the needle, the patient experiences proportionately to the intensity of the action, a violent, darting, tearing pain, felt not only during the period of contact but for many hours after, though in a lower degree." . . . "A contraction and oscillation of the adjacent muscles is another effect of the galvanic puncture. The surrounding skin and the portion intermediately betwixt the points of insertion alternately corrugates into small folds. And we observe in a little while, about the points of insertion, a circumscribed red areola of from two to three lines diameter." . . . "Where the zinc pole acts, the inflamed halo rapidly appears; the integument becomes hard and of a bright scarlet; and there issues from the puncture at first watery serum, but afterwards thick and turbid lymph. On the other hand, a conical bluish-grey elevation surrounds the needle connected with the negative pole, which contains air diffused throughout minute cells, bursting with a crackling noise after the withdrawal of the needle. Here a plain surface covered with the raised but not discoloured integument remains behind, while in the former case the surface is for a considerable time, hard, prominent, and red." . . . "After the lapse of three or four days we find in the above situations circular sores of from one to two lines deep. The ulcers determined by the zinc pole have a redder margin and are more inflamed than the others."

The galvanic puncture is considered more energetic in its action than the simple shock, inasmuch as the needle in union with one or other pole, by penetrating the flesh, comes into closer contact with the nerve.

In operating for cataract of late years, the superior has been preferred to the inferior section of the cornea. This mode is attended with less risk of protrusion of the iris or discharge of the vitreous humour, and is seldom followed by much vascular reaction. But, on the other hand, if the cornea be relaxed and flaccid, it is difficult, nay sometimes impossible, to procure union of the edges of the flap. Dr. Rosas recommends the *external lateral* section in consequence of the favorable result of twenty-one operations performed partly with Beer's knife and partly with his own two-edged knife. He does not consider so limited a number of trials sufficient to warrant the general introduction of this method into practice; but leaves its merits to be decided by subsequent experience.

In cases of inveterate opacity of the cornea and which have resisted all ordinary remedies, the author proposes, in addition to the artificial enlargement of the pupil, or the insertion of a seton through the cornea by means of a finely-polished curved needle, as suggested by Pellier, the inoculation of the matter of blennorrhœal ophthalmia. In this way the obscure cornea is occasionally restored to its normal transparency. We must, however, regard inoculation as a very hazardous experiment. The difficulty that is met with in restraining the violence of so distressing a disease as purulent ophthalmia, independently of the alarming structural changes incidental to it, are obstacles too serious to counterbalance any probable good that may ensue, more especially as the formation of arti-

ficial pupil supplies an almost infallible and comparatively safe means of counteracting the defect in question.

It was formerly a generally received opinion among ophthalmological writers, that penetrating wounds of the lenticular capsule are necessarily followed by cataract. This is true, however, only in reference to lacerated wounds. Dr. Werneck not long ago instituted a series of experiments on the eyes of living animals, for the purpose of ascertaining the effect of wounds of the capsule and lens. Among other results Dr. W. found that if the outer lamella of the capsule be punctured; incised, or even cauterized, no scar is left; nor even when both lamellæ, along with the membranous coat of the lens, are pierced. But that in every instance of laceration of the fibres a permanent opaque spot was the invariable consequence. Dr. Rosas, in describing wounds of the lens, corroborates the above conclusion.

"When wounds go deeply into the lens, or when the anterior segment of the capsule is cut in several places or torn across, so much more certainly is cataract developed. Should the iris and ciliary body have been simultaneously injured, then is the accident complicated with exudation of lymph and adhesions of adjacent parts."

We conclude our notice of Dr. Rosas' book with the abstract of a remarkable case of aneurism of the orbit discovered during life, and the only one ever witnessed by the author.

"The subject, a girl eighteen years of age, had previously laboured under scrofulous disease, and irregular menstruation. The aneurismal affection was imputed to a severe blow near the eye. She complained of frequent dull pain in the bottom of the right orbit, from whence the eye to a certain extent protruded, but might still be covered in a great measure by the eyelids. No morbid change of structure could be detected. Any undue determination of blood to the head produced redness of the eye with a sense of heat and throbbing in the orbit, vertigo, tinnitus aurium, and imperfect vision. These symptoms were always present on the approach of the catamenia, which were scanty and irregular. On examination, a distinct pulsation might be felt deeply seated in the orbit along with a thrilling murmur. Repeated venesection in the foot; leeches to the genitals; stimulant foot-baths; and emmenagogue medicines, restored the faulty menstruation. By antiphlogistic regimen, cold applications to and occasional leeching of the eye, the aneurismal disease was much relieved."

II. In the work of Dr. Marchetti we find a critical investigation and arrangement of whatever has been done in modern times for general *ophthalmoscopy*. It is, as its title implies, a guide-book for the examination of the diseases of the eye; which, according to the German method, are treated of *subjectively* and *objectively*. However copious the work may be, it would have been more complete had the author introduced the mode of using the lens; as, by its means, certain affections of the anterior and posterior chambers of the eye, can be alone observed and discriminated.

The utility of a preliminary work of this kind for the student and junior practitioner is incontestable.

"The young student while entering upon the study of ophthalmology finds himself in a state of suspense and uncertainty as to how he should proceed. Every thing seems to float in confusion before him; he feels himself incapable of recognizing the minute changes in internal structures; of distinguishing that which belongs to one texture from that which belongs to another, that which is natural from that which is the result of present or former disease. The importance of this rudimental branch of

ophthalmic study is abundantly evidenced by the fact, that many practitioners, otherwise skilful and well-informed, commit gross and irremediable errors in the treatment of ophthalmic diseases, from mere inattention to the morbid phenomena."

The author, although at times tediously circumstantial, discusses fundamentally the *objective* appearances—those which are or ought to be appreciable by the practised eye of every professed oculist. He commences, in the order of enquiry, with the accessory parts, and thence passes on to those concerned in the formation of the eyeball, as follows:—

"I. External parts surrounding the globe of the eye; *a*, supraciliary region, supraciliary arch, eyebrows; *b*, palpebræ, palpebral margin, eyelashes.

"II. Lachrymal organs: *a*, lachrymal gland, lachrymal duct, tears; *b*, lachrymal puncta, and canal; *c*, lachrymal sac; *d*, nasal duct.

"III. Integral parts constituting the globe of the eye: *a*, *external parts*, conjunctiva, sclerotic, cornea; *b*, *internal parts*, anterior chamber, iris, pupil, posterior chamber, bottom of the eye.

"IV. Examination of the entire globe as respects volume, situation, consistence, mobility, direction, aspect, configuration, regard."

Subjective ophthalmoscopy, or that derived from the narration and interrogation of the patient, embraces

1st, the nature and degree of vision; 2d, optical illusion; 3d, pain; and the sympathies subsisting between the eye and other parts, whether effected through the medium of the nervous, the vascular, or the lymphatic system.

Dr. Marchetti's remarks on diseases are sometimes interesting.

"There is a peculiar ill-conditioned ulcer of the cornea of traumatic origin occurring in hot seasons, and characterized by having its exterior in whole or in part covered by white flakes like snow. I witnessed an instance in which the entire surface of a large ulcer presented this remarkable white appearance, as if a delicate white pellicle had been diffused over it, and which is an infallible sign of the establishment of mortification—of the *white gangrene* of the cornea."

In reference to the various degrees of sensibility and the consequent liability to inflammatory diseases of the eye, Dr. Marchetti says that if the iris be blue the organ is endowed with but little excitability, from the lower development of the sanguineous system of the choroid and iris. Hence it is less disposed to inflammatory than to catarrhal and neuralgic affections. If, on the contrary, the iris be dark, then is there a copious supply of blood, high excitability and proneness to vascular congestion, apoplexy, and amaurosis. Is not this remark more the result of theory than observation?

The particular morbid condition denominated *friability* of the iris, met with in syphilitic, arthritic, and cachectic subjects, merits notice on account of its forming a principal contra-indication to the operation for artificial pupil. It is characterized

"By our being no longer able to discover the natural vascular circles; by the iris assuming a dirty-green wash colour; by its surface presenting bluish spots, in which the texture appears wanting; by its loss of motion; and by our knowledge of the fact of its having been subjected to a severe, long-protracted, chronic inflammation."

That severe blows on the eye are not necessarily followed by serious mischief is shown from the results of two cases recorded by the author. In one the sclerotic was ruptured close to its junction with the cornea; the subsequent inflammation was very slight, and the patient suffered only

weakness and confusion of sight. In the other there was procidentia of the lens at the pupil, with no other effect than a little diminution of vision.

III. The work on *Purulent Ophthalmia*, by M. Juillard of Geneva, is a thesis, and, although not the actual production of M. Sanson, must be regarded as containing an exposition of his views and practice, and as stamped with his high authority.

M. Sanson, struck with the dangers attending the different means heretofore employed against blennorrhœal or purulent ophthalmia, and the *absolute inefficacy* of the antiphlogistic plan, conceived that, by conjoining excision and cauterization, we might derive an advantageous result. The problem for solution was the following: to destroy the secreting organ wherever it exists, and to bring into relation parts whose vital properties being differently modified are therefore less disposed to adhere the one to the other.

Instead of attempting to restore the urethral discharge, which he has never seen altogether suppressed, M. Sanson practises, in the first place, a full bloodletting of from twenty-four to thirty ounces; not with the view of arresting the course of the affection, but of counteracting symptoms that might be developed, in consequence of a painful operation he proposes to perform and upon which he does not lightly decide. If contrary to expectation this primary evacuation be followed by a marked melioration of the symptoms, a fresh quantity of blood is abstracted and to a greater amount than the preceding time, leeches are at the same time applied around the orbits; in a word, the antiphlogistic method is pursued with great vigour. But, if the ophthalmia instead of abating under the influence of such treatment goes on increasing in intensity; or, remaining stationary, does not seem disposed to yield to a treatment energetic proportionately to the constitution of the sufferer, M. Sanson then renounces a procedure inappropriate to the nature of the malady, and resorts to the excision of the conjunctiva. This he performs in the following manner: the patient reclining upon a bed, one assistant keeps the head fixed upon pillows, while another separates the eyelids from each other, holding them fully everted at the same time. The surgeon then seizes with a pair of dissecting forceps the salient portions of the ocular conjunctiva, and snips away, with scissors curved on the flat as completely as possible, all the tumefied parts of that membrane, as far as and including the conjunctiva to its point of reflexion: it is, however, frequently difficult to push the operation so far. The surgeon then permits a flow of blood more or less considerable; and when he believes a sufficient local emission to have taken place he carefully wipes away the blood mingled with pus, bathing the parts, and passes a pencil of nitrate of silver over the whole internal surface of both eyelids held everted. So soon as the cauterization is at an end, the surgeon, maintaining as far apart as possible the eyelids, is to wash freely with water the cauterized membrane, in order to remove any nitrate of silver remaining uncombined with the tissues, which might adhere to the cornea and alter its transparency.

The success of the operation now described depends in a great measure on the care employed in cauterizing to a suitable degree the palpebral conjunctiva. For this purpose, we must pass lightly the pencil of lunar

caustic, endeavouring to make it act more particularly on those points where the granulations are most developed and numerous. If the conjunctiva still presents folds, we must try to introduce the nitrate of silver so as to repress as far as possible all the papillary bodies there concealed. The patient should be bled, and have his feet bathed in warm water containing mustard several times a day; in fact, he is to be treated as in ordinary cases of ophthalmia. Great benefit will be obtained from the application of cold, which acts here as an astringent. The pain, very intense at the moment of the operation, is allayed so soon as the caustic touches the conjunctiva. Were we to judge of the violence of the symptoms that might be developed, from the acuteness of the pain felt during the operation, we should have much to dread; but, happily, experience has seldom justified these grave anticipations. Cauterization even to a considerable extent has been practised at the ophthalmic clinic of the Hôtel Dieu of Paris, with the nitrate of silver, without any subsequent inflammatory mischief; so true is this that M. Sanson, who at the outset never failed to prescribe, immediately after cauterizing, abundant bloodlettings both general and local, has never recourse to them now, unless there be some special indication present to necessitate their employment. M. Sanson believes that by pursuing this method not only do we fulfil the essential therapeutic indications, but we save moreover the patient the evils incident to other plans of treatment.

In reference to the indications, it may be remarked that the secreting organ is removed in certain points and cauterized in others. Secretion becomes then impossible. And if, as Jüngken affirms, the danger in blennorrhœal ophthalmia consists particularly in the acrid and corrosive qualities of the matter furnished by the papillary bodies, we shall thus have exactly fulfilled the leading indication in their obliteration.

It may be said in the second place that M. Sanson's method exposes the patient less than most others to serious consecutive symptoms. In effect, two surfaces are brought into contact whose vital properties are very different. One of these, the bleeding surface, will cicatrize more rapidly than the other, presenting an eschar profound in proportion to the action of the caustic; there will be developed in this last an *eliminatory* inflammation which, requiring a longer or shorter time to run its course and detach the eschar, will expose a denuded surface at the period only when the process of cicatrization is complete in the other. It may be supposed that the coalition of the eyelids with the eyeball will be much less likely to occur, if it be possible, under the operative procedure.

Lastly, this mode of treatment is in certain instances the only one applicable. When, for example, after a patient has been submitted to a rigorous treatment, the other eye becomes in its turn affected, it is evident that copious depletions proportioned to the gravity of symptoms can no longer be resorted to, without compromising the patient's existence. It was this circumstance which suggested to M. Sanson, at the bedside of a patient, the idea of excising all that was possible of the conjunctiva, and destroying the secreting organs beyond the reach of surgical manipulation. This plan of treatment, or some other analogous, will be alone admissible when we have to deal with subjects advanced in years or of a feeble constitution. It is well known that sanguine emissions, even to a limited extent, may under such circumstances prove highly prejudicial.

Two cases of gonorrhœal ophthalmia are reported by M. Juillard, in which the above method effected a speedy cure.

IV. We have introduced into our list of books at the head of the present article, the first Part of the Cyclopædia of Surgery, for the purpose of noticing the Treatise on Amaurosis contained in it. This, the production of Mr. Tyrrell, is by far the best article in the fasciculus, and is highly creditable to its excellent author. We do not agree with Mr. Tyrrell in all respects, and we shall not hesitate to state the points whereon we differ from him: but we strongly recommend his paper to the attention of all ophthalmological surgeons, as a work of great practical value.

Mr. Tyrrell sets out with a remark, which might lead the reader, were he not otherwise better informed, to suppose that the term Amaurosis had at one period comprehended all the diseases which obscure the sight; whereas it is a word introduced by the later Greek writers as synonymous with the amblyopia of Hippocrates and the melania of Aristotle, names which were always limited to affections of vision independent of visible change behind the pupil. A good historical dictionary of medical terms is a desideratum: well executed, it would afford much amusement as well as instruction, and prevent unlearned writers from falling into many sad blunders.

After explaining that amaurosis may result from disease in the retina, the optic nerve, or the brain, Mr. T. observes that "from the known principles of nervous power, it must be obvious that any disease affecting the cerebral portion of this nervous apparatus, or the optic nerve, must influence the extreme part of the nerve, or the retina, so that disease cannot exist in either of the former without producing a diminution or destruction of the functions of the latter." (P. 77.) This statement, if left unqualified, might mislead. That disease in the brain, or in the optic nerve, may obliterate the power of vision, is readily granted; but that disease in either, even to the extent of complete blindness, necessarily affects the peculiar functions of the retina, we apprehend to be an error. The functions of the retina are to receive the impressions of light and transmit them to the optic nerve; and these functions may be perfectly performed, although, from disease in the brain or in the optic nerve, no perception of light is retained. Nay, farther, the functions of the optic nerve may be perfectly performed, along with those of the retina, although, in consequence of disease existing in the tubercula quadrigemina, the patient may be totally amaurotic. The function of the optic nerve is to transmit the impressions made on the retina to two quarters: first, to the third pair of nerves, by which the iris is moved; and secondly, to the brain, where alone visual perception is effected: and we sometimes find the former part of the function of the optic nerve performed and the pupil moved truly according to the degrees of light to which the eyes were exposed, although the latter part is null and the patient totally blind.

Mr. Tyrrell adopts the old distinction of amaurosis into functional and organic; the former division comprehending, as he says, "those which are not attended with any appreciable change in structure," and the latter "those which are attended with some change." (P. 77.) It is plain that every thing regarding this distinction hinges on the meaning given to the words "appreciable" and "structure." Now, ninety-nine out of one

hundred inspectors of dead bodies, on looking at a retina affected with neuromata, would in all likelihood discover nothing wrong in it. It would only be the cautious microscopic observer who would detect these minute tumours, scattered through the medullary layer of that membrane. Thousands of pathological changes not yet appreciated, are no doubt appreciable, and will yet be appreciated.

Then, as to "structure:" are changes in the blood-vessels and in the blood to be deemed structural changes or not? Is the term "organic disease" to be restricted to those morbid alterations in which there is an obvious and *permanent* change in structure; or are we to extend the meaning of the phrase, and include the primary and secondary changes resulting from inflammation? Not long ago we drew blood from a patient labouring under arthritic amaurosis; and, on submitting a little of that blood to the microscope, it was evident that the red globules were deficient in quantity and unnatural in their appearance. This surely was an organic change, although without this peculiar mode of examination no such change could have been appreciated. What Haller remarked regarding disorders of the mind, is still more applicable to the subject before us. "*Id utique adparet, plerumque in mentis vitiis encephalum pati: et si aliquando rariori exemplo non visum est pati, potuit vitium in minoribus elementis latuisse, aut incisori patientia defuisse.*" (*Elem. Phys.*, v. 574.)

At page 95, Mr. Tyrrell even talks of functional disturbance of the orbital portion of the optic nerve from the effect of pressure, perhaps absorption, of the nerve, from exostoses and other tumours. Surely Mr. T. cannot suppose that the contact of a morbid growth in the cavity of the orbit will destroy the function, without affecting the material elements of the optic nerve. If any disorder to which the human frame is subject deserve the name of an organic disease, it is such a one as this, where a tumour or an exostosis, perhaps palpable to the observer even during the patient's life, and perfectly appreciable after death, prevents the nutrition of the optic nerve, compresses its medullary substance, and at last causes its absorption.

Besides the distinction of functional and organic, the following classification of the particular varieties of amaurosis has been adopted by Mr. Tyrrell:—I. Functional amaurosis of retina. II. Organic amaurosis of retina: 1, from inflammation of the retina, acute and chronic; 2, from inflammation of the choroid, acute and chronic; 3, from inflammation of the globe generally; 4, from lesions of the globe; 5, from scrofulous and malignant disease of the retina. III. Functional amaurosis of optic nerve. IV. Organic amaurosis of optic nerve. V. Functional amaurosis of cerebrum. VI. Organic amaurosis of brain or its membranes. VII. Functional amaurosis from excess of vascular action. VIII. Functional amaurosis from deficient supply of red blood. IX. Congenital amaurosis.

Under these heads, Mr. Tyrrell has communicated much useful information on the symptoms, causes, and treatment of the various affections of which he speaks. The treatment which he recommends is on the whole highly judicious, and his observations on diagnosis are particularly valuable. "I have alluded," he says, "to the close resemblance which exists between some of these cases, [cases of asthenic amaurosis,] and those in

which the amaurosis is caused by cerebral pressure, and the consequent liability to mistake in diagnosis and treatment." This point he illustrates by cases, one of which we cannot forbear quoting.

"A young and delicate female, about seventeen years of age, having great nervous susceptibility, and disposed to hysteria, was accidentally thrown from a gig with her father, who was a very large man. She fell upon her father, and was taken up without any mark of injury: she was however dreadfully alarmed, and soon after attacked with a violent fit of hysteria: several hours elapsed before she could be roused from this under the use of the ordinary treatment, by stimuli, anti-spasmodics, &c. She then complained of excessive headach, with giddiness, confused vision, and heat of head; the stomach was irritable; she suffered from great mental depression, and could not procure sleep. Whilst in this state her medical attendant (believing that she had received some concussion of brain by her fall) bled her: she fainted after the loss of about eight ounces of blood, and remained a long time in a condition approaching syncope. In a few hours afterwards her previous symptoms were renewed and became much aggravated, the headach almost intolerable, the depression excessive, and the affection of vision increased, so that in forty-eight hours after the venesection she could not discern light. A few hours after the amaurosis was complete, she was brought to me, still labouring under the symptoms last mentioned; but, in addition, I found her pallid, with cold extremities, and a quick, feeble, and easily compressible pulse: the result of the abstraction of blood was sufficiently indicative of the nature of the affection, and to prove its asthenic character. I directed perfect quiet, the recumbent posture, small portions of nutritious matter to be given frequently, ten drops of the *vinum ferri* to be taken in weak wine and water every four hours, and an occasional dose of the compound decoction of aloe with manna, and a few drops of aromatic spirit of ammonia as an aperient. In forty-eight hours from the time I prescribed for her, her symptoms were greatly mitigated, and she began to perceive light again; she soon recovered her ordinary condition of health, and lost her headach, feeling of depression, &c., but the functions of the retina returned very gradually; so that several weeks elapsed before she could tell the letters in the title-page of a common octavo work; being so far restored, I lost all knowledge of her for several months, when she was again brought to me nearly in the same state as when I first saw her: her father had failed in business, and she had suffered much from anxiety and privation, and had neglected all treatment. I put her under the same plan of treatment again, gradually increasing the dose of steel, and occasionally varying the form of medicine: her health was rapidly re-established, and a very slow improvement took place in vision; but after many months' careful perseverance in the above means she obtained only sufficient vision to be able to make out a large print. I subsequently tried blistering, electricity, galvanism, and strychnine, but without any further benefit." (P. 102.)

With regard to the treatment of the different varieties of amaurosis, Mr. T. very properly regulates it according to the particular symptoms present in each case, and gives little encouragement to such general remedies as emetics, arnica, phosphorous, strychnine, and the like. "I have frequently tried the influence," says he, "of strychnine in cases of amaurosis, which I considered most appropriate for its use; but I have been very greatly disappointed in its effects: I have not seen one single instance of benefit from its employment in the manner generally recommended." (P. 105.)

The first variety of amaurosis, according to Mr. Tyrrell's classification, is functional amaurosis of the retina; which he believes may arise from one or other of these four causes, viz. non-exercise of sight, excessive light, concussion of the eye, and narcotic poisons. Mr. Tyrrell thinks that a squinting eye becomes amaurotic because it is not in use; the common notion is probably the more correct, that the eye being defective is not used. That, by looking steadily at the sun, a person may lose his

sight, or that the same result may follow a blow on the eye, will not be denied; but that such effect will arise without inflammation, or some other organic lesion of the retina, is a thing not to be credited. Belladonna applied to an ulcer on any part of the body, or taken internally in a sufficient dose, will enormously dilate the pupil, and thereby render vision for a time indistinct. But this is not amaurosis. This is merely mydriasis; and by making the patient view objects through a small hole, we have always found, in such cases, that the retina completely retained its sensibility.

Under the title of "Retinitis, acute and chronic," Mr. T. has introduced a description of the disease well known to oculists as acute and chronic glaucoma; and under that of "Choroiditis," he has described amaurosis resulting from internal scrofulous ophthalmia, or what Rosas calls scrofulous amaurosis. This changing of names is quite intolerable; and we are sorry that Mr. Tyrrell has added to the evil. In fact, it would require years of study to become acquainted with the mere synonymes, which have been heaped up to such a Babelish height by the writers on eye-diseases. What Mr. Lawrence terms acute glaucoma, Mr. Tyrrell calls acute retinitis. Again, Dr. Mackenzie has described a very peculiar disease of the eye, under the name of choroiditis. His description has been borrowed by Mr. Middlemore, and adopted (with due acknowledgment) by Dr. Sichel. We believe Dr. Mackenzie to be quite wrong in calling this disease choroiditis; it is neither more nor less than the sclerotomalakia of the Germans. But here we have Mr. Tyrrell with an entirely new application of the same term. His choroiditis is nothing more than scrofulous iritis, or internal scrofulous ophthalmia, ending in blindness. Moreover, we are not at all satisfied that glaucoma is essentially an inflammation of the retina; nor that internal scrofulous ophthalmia is so much an affection of the choroid as it is of the retina and iris. Dissections only could warrant Mr. Tyrrell's views of glaucoma and scrofulous amaurosis; but he makes no appeal to evidence of this sort. Certain we are that chronic glaucoma is not a retinitis merely. In fact, in this disease, the retina is often the last part to be affected.

Mr. Tyrrell commences his description of internal scrofulous ophthalmia or chronic choroiditis, by saying that more or less functional disturbance usually precedes the inflammatory action in the choroid. The disturbance he here refers to, consists in the symptom commonly called *muscæ volitantes*, a symptom which in all probability depends almost constantly on a dilated state of the retinal vessels; pressure on the convex surface of the retina gives rise to photopsia and prismatic vision, not to *muscæ volitantes*. This is, at least, the general opinion; and it remains with Mr. T., when opportunity offers, to explain the grounds of an assertion so contrary to the common belief as that this symptom depends on the state of the choroid.

Mr. T. speaks of volitant and fixed *muscæ*, but neglects to draw any accurate distinction between them, or to explain the cause of the difference, if difference there be. It is well known, that, from the cause of *muscæ* rarely existing in the axis of vision, they appear to fly upwards or downwards, or from one side to the other, whence the appellation *volitantes*. These we presume are the volitant *muscæ* of Mr. T. What, then, are the fixed—and what are the evanescent,—which seem to be a third

variety, also left without any accurate distinction? He adopts the popular view of the exciting cause of *muscæ*, that they depend on the stomach. He might as well say the acidity and other stomachic ailments depended on the *muscæ*. Cerebral disease is very frequently the cause of dyspepsia; and along with this the patient is affected with *muscæ*, and perhaps slight amblyopia. A congestive state of the brain, producing the dyspepsia, is attended by a similar condition of the retina, which causes the *muscæ*.

We observe that in speaking of cataract and its diagnosis from glaucoma (p. 83,) Mr. Tyrrell states that the cataractous opacity occupies, in ordinary cases, the central part of the lens. If he means the centre of the lamella forming the anterior surface of the lens, we shall not dispute the matter; but if he means the kernel or centre of the lamellæ generally, we are positive cataract never commences there, but always superficially. We have examined many cataractous lenses immediately after extraction, and in ninety out of the hundred only a few of the exterior lamellæ were affected with the peculiar opaque condition, called cataract; the rest being generally amber-coloured and pretty transparent. In this respect, the cataractous degeneration of the lens is the reverse of the glaucomatous; for the latter affects chiefly the lamellæ immediately behind the kernel of the lens.

Although we enter our protest against the change of names which Mr. Tyrrell has introduced, and must object to the introduction of his *retinitis* and *choroiditis* under the head of amaurosis, still we most willingly testify to the truth and clearness of his descriptions, as well as to the prudence and skilfulness of the treatment which he advises. In both diseases, he trusts much to mercury; and the plan of establishing a full and free mercurial action, and sustaining it for several weeks or months, which was so beneficial in the hands of Heister, appears in those of Mr. T. to have also proved successful even in cases of complete amaurosis.

"One of the first cases of this kind that came under my care was in a powerful man about thirty-eight years of age, from the north of Ireland, who was sent to me by my late excellent friend, Dr. Babington: he had been amaurotic for seven years, and had lost the perception of light; but the globes possessed their natural firmness and elasticity; the pupils were clear but irregular, from many points of adhesion between the pupillary margin of the iris, and the anterior capsule of the lens; the irides were discoloured and dull, and he had the vacant aspect of a blind person. I admitted him into the infirmary, (then in Charterhouse square,) and put him under mercurial treatment, with a nutritious diet: as soon as the mouth became tender, a considerable degree of scleritis occurred, with pain and tenderness of the eyeballs; the plan was, however, steadily continued, and some belladonna was applied night and morning to each eyebrow; he soon became sensible of light, and gradually acquired the power of discerning surrounding objects, and at the same time the adhesions between the iris and the capsule of the lens began to give way, and the pupils to reassume their natural figures; by degrees the vision improved, all appearance of inflammatory action subsided, the pupils became nearly regular, and the irides brilliant; the full mercurial action was kept up for above sixteen weeks, when the amaurosis was completely subdued, and his vision perfect. For sixteen weeks he discharged about a pint and a half of saliva daily; but, in spite of the severe treatment, he came out of the course improved in appearance, and evidently increased in bulk." (P. 92.)

Organic amaurosis from disease of the brain or its membranes is of course a wide field; presenting great variety in the symptoms, according to the seat, nature, and extent of the morbid changes. In some cases, it assumes the form of hemiopia, in others the whole field of vision is

obscured. In some the pupil is dilated and fixed, in others it moves freely. Of this last remarkable fact, Mr. T. attempts no explanation. Double vision, strabismus, and palsy of the muscles of the eyeball and eyelids, are symptoms noticed particularly by Mr. T. as occasionally attendant on this variety. The chief immediate causes which he indicates, are injuries to the head, fevers, intemperance, and excessive mental labour. The treatment is comprised under these three heads: relief of vascular turgescence by general and local bleeding; prevention of further congestion or turgescence by diet, mental and bodily quiet, and counter-irritation; promotion of absorption of morbid deposits, by counter-irritants, mercury, iodine, &c.

Mr. T.'s account of congestive amaurosis, and of that arising from deficiency of red blood, is plain and sensible. He winds up the subject with some hints regarding the pathological anatomy of the disease well deserving of attention. Many of the changes in structure which we find in our examinations of amaurotic cases, he believes to be the effect, and not the cause, of the loss of vision. This happens especially with respect to the optic nerve, which almost always loses its natural character under continued amaurosis, whatever has been its origin.

Mr. Tyrrell concludes the Article with some good remarks on feigned amaurosis, which he has sometimes succeeded immediately in detecting by the following means:

"During conversation dropping some small object, as a knife or pencil suddenly, which has been immediately picked up by the patient—pretending to see something in the room, or out of the window (if near) of curious or unusual character, the patient has been unguarded for a moment, and his eyes have followed the direction I have pointed to; asking how the patient's dress became torn or dirtied, his eyes have immediately been directed to the part mentioned or pointed to; and several other like expedients. In one case, of a little girl, which baffled me for two or three weeks, during which period she had been strictly watched, but nothing elicited; I was engaged in conversation with her about her medicines, which she had much abhorrence of, and after trying to persuade her to take them well, I said I would give her sixpence if she would promise to do so, she assented, and I held out a halfpenny towards her, which she directly said (without touching it) was not a sixpence: she had previously sat for hours together without moving, and would allow me to place my finger or other matter in contact with the cornea without flinching." (P. 107.)

V. Our limits will not allow us to notice the small work of Dr. Littell in detail; but, after an attentive perusal of the whole volume, we confidently recommend it to the senior as well as junior members of the profession. It is replete with information, yet so terse in style and compressed in bulk as at once to entice and repay perusal. We agree in most points with the author's pathological inductions and practical precepts. The language is free from any tinge of Americanism, the descriptions are short but comprehensive, while the treatment is characterized by great prudence; on the one hand, avoiding the charge of inactivity or feebleness, on the other, never risking the more serious results of chronic mischief and broken health from excessive depletion, or the depressing effects of violent mercurial courses; of which faults some of our own countrymen are not entirely innocent.

It is no small triumph to Dr. Littell to be able to say that he has introduced almost all that is valuable, and every thing absolutely neces-

sary to the student, within the compass of 250 small pages; and we would deliberately recommend our young friends to read this work before encountering the voluminous treatises of Lawrence, Travers, Mackenzie, Middlemore, &c. We are in no way inclined to speak slightly of these works: on the contrary, we believe that there is much accurate observation, learned research, and sound practice to be found in them: but the commencing enquirer is startled by their magnitude, and discouraged by the belief that the subject of ophthalmology ought indeed to be cultivated by exclusive practitioners, when he finds such extensive and elaborate treatises devoted to its consideration. Apprehensions and misapprehensions of this sort the small volume before us is well calculated to remove, and we once more earnestly recommend it to the attention of the student.

ART. III.

The Works of JOHN HUNTER, F.R.S.; with Notes. Edited by J. F. PALMER, &c. Vol. I.—London, 1835. 8vo. pp. 643.

Lectures on the Principles of Surgery; with Notes. By JAMES F. PALMER, Senior Surgeon to the St. George's and St. James's Dispensary, &c.

AFTER the lapse of almost half a century from the death of John Hunter, we are for the first time presented, by means of the present publication, with his *Lectures on the Principles of Surgery*. They constitute a second division of the first volume of the new edition of *Hunter's Works*; and, from the facts stated by the editor, no doubt will be entertained of their being perfectly genuine. They are printed, we are informed in the preface, from a very full and accurate copy taken in short-hand by Mr. Henry Rumsey, of Chesham, which the editor has compared with certain other copies extant, and by the aid of which he has in some instances been enabled “to retrench redundancies and repetitions, and to substitute one word for another having a synonymous expression.” However, he has never, by this proceeding, interfered with the true Hunterian character of the style, distinguished as it always is by plainness and vigour of expression, and by a strain of singular candour and honesty in the record of experiments or in the statement of pure matters of fact.

We have perused these *Lectures* with no ordinary feelings of satisfaction. They embody an immense amount of important facts, directed with no common skill to the illustration and improvement of medical science generally, and of the surgical department in particular. Indeed, we have no hesitation in saying that, whatever be the position of the reader in the profession, he will not relinquish the perusal of these *Lectures*, without the consciousness of having usefully employed the time which he may have bestowed upon them. For they constitute, in the fullest sense of the term, a philosophical disquisition on the science of Surgery; and hence, embracing the great principles on which the whole art of healing rests, their interest will be felt by all who regard Medicine as a true branch of science, and who delight to witness the gradual development of principles in the right interpretation of the phenomena of nature.

What tends to give to the present work so much of a *general* interest is the systematic manner in which the author proceeds in the discussion of his subject: commencing by brief explanations of the simplest facts of nature, he leads the student progressively onwards in the consideration of living phenomena, and the mind of the reader is thus gradually brought to the contemplation of the bearings of general laws upon the particular subject to which his attention is especially solicited. And, in this respect, in expounding the principles of surgery as a branch of natural philosophy, the Lectures of Hunter possess distinguishing superiority. They may not be well adapted for a Manual of reference, or as a mere *Vade Mecum* to the practitioner; but, as supplying him with a solid groundwork in the scientific principles of the profession, they constitute, we conceive, a very important addition to the medical literature of our country. It is quite true that objections will, in the present day, apply to some of the doctrines advanced in them; but the same result happens to everything that is written upon a science of experience, which, being essentially progressive, is constantly receiving additions and improvements. And yet there are works, in every department of natural science, that always maintain a certain standard value, from their intrinsic excellence, and are never superseded entirely by newer contributions. In this class we are sure that Hunter's Surgical Lectures must ever command a place. The principles of surgery, as known in his day, are so clearly and fully explained; the immense improvements, wrought by his own almost unaided energies, are so forcibly impressed upon the reader's attention; and the rigorous exactness of the observations and experiments recorded is so admirably established in their relation to principles,—that we can entertain no doubt that these Lectures will long be regarded as an important and valuable storehouse of information, not only by those who are engaged in the actual study or practice of our profession, but by all who are interested in the progress of science in general.

The subject of these Lectures, which occupy twenty-three chapters, naturally suggests a division of their contents into three portions,—physiological, pathological, and practical. The lectures purely physiological are comprised in eight chapters; and when it is remembered that John Hunter was the first who systematically applied the principles of physiology to the elucidation of surgical diseases, his views in this department must possess much interest. We are sorry that our limits will only allow us to notice a few of the more interesting points here discussed, and this only in the most cursory manner.

Hunter devotes a whole chapter to the exposition of his ideas upon the “Vital Principle;” and, as it is well known that a large proportion of his time was given to the elucidation of this subject, his detailed views, though not always in accordance with those which are now most generally recognized, will be found to possess a high degree of interest. The vital principle, with Hunter, is reducible, in all animals, to one simple element, being in itself either something actually superadded to the matter which it vivifies, or an essence arising out of the material arrangement under which life is manifested. The evidences of the endowment of any combination of matter with this principle are the power of self-preservation, and the power of action: these, though arising from the

same source, are very different properties, as the first may exist independently of the second: thus, a fresh egg, though alive, has no vital action, and yet, even prior to incubation, it maintains a very considerable power of self-preservation, and of resistance to ordinary decomposing agency. Life, then, Hunter contends, is not *action*; though it is continued and supported by action, when this is once set up. Thus, in the higher classes of animals, "if the heart acts, the lungs must fulfil their part; the stomach must digest, the other parts subservient to this organ must be put in motion, and the secretory organs, nerves, and voluntary muscles." In this manner, although such a series of movements was not requisite to constitute the simple presence of life, yet, having once commenced, their absolute suspension in any combination of animal materials destroys the vital principle. He maintained that this principle was one simple property in every animal; and he certainly felt the disposition, common to his own, and more especially to the preceding age, to generalize too hastily the phenomena indicative of life, and almost to personify their occult cause as an independent existence. However, when, in any investigation, he proposed to himself to rectify notions which he believed to be erroneous, or to elicit some new truth, he never proceeded upon any such speculative grounds, but boldly interrogated nature by direct experiment. Indeed, we consider that the period in which Hunter flourished may not inaptly be regarded as the transition era of medical science,—as that in which the speculative was exchanged for the inductive philosophy, as the basis of research; and we have alluded more particularly to the hypothetical notions concerning the vital principle, as an illustration of the influence which mere speculation yet retained, even over minds of the most powerful order. For no one acquainted with the history of Medicine will fail to recognize, in the abstract vital principle of Hunter, the lingering shades of the *Archæus* of Van Helmont, or of the *Anima* of Stahl. Of life, beyond its observable effects, we can obviously know nothing; and hence, in the present day, we investigate only the conditions under which it is manifested, and enquire not what are its absolute laws, nor what is its actual essence.

The third chapter is devoted to the consideration of the Physiology of the Blood, which, as all are aware, had generally, before Hunter, been regarded, in his own words, "as a passive, inanimate, moving fluid, found everywhere in the body, deriving motion from the heart for the various purposes of the whole, then returning to the heart to be sent out again." In treating of this subject, Mr. Hunter explains and illustrates his own views regarding the vitality of this fluid. This is done in a very concise and analytical manner; and upon the original notions, propounded on this matter, will always depend a large share of Hunter's well-earned fame. It must also be admitted that even although the idea of the life of the blood should be questioned, the views maintained by him have led to results of practical importance, both in surgery and medicine.

In Chap. iv. we have a short sketch of the Principles of Organization and Action. The first is regarded in a point of view somewhat too mechanical to harmonise well with the doctrines of the present day, as the following extract will evince.

"Solidity, in a certain degree, is necessary for self-motion; for parts cannot pro-

duce motion in one or other without some resistance or fixed point of motion. We therefore find the acting parts of an animal composed of solids, or the parts which compose them could not coalesce together by the attraction of cohesion; and it is necessary that it should be so, as without this no determined action could be produced.

"Now we have gone so far with the materials of an animal, let us next examine how these materials are disposed so as to form an animal. These materials may now be considered in a mechanical point of view, like the component parts of a machine, each of which has its destined use and own peculiarity of form. These are united with each other to form parts, the whole forming organs of various kinds to produce the mechanical effects required. These organs, again, united according to certain established rules, form animals. This compounding of animal matter is what should be understood by organization. Now, if this idea of organization is just, organization and life are two different things; for, according to this definition, a dead body is as much organized as a living one, for in the dead body the same mechanism exists as in the living one." (P. 241.)

We cannot assent to the proposition that "this compounding of animal matter is what should be understood by organization." By the term organization we understand an association of particles of matter in certain mechanical, chemical, and *unknown* relations, and any statement that would define organization merely upon the principles of mechanical or chemical philosophy must always be inexact, if some condition, not yet understood, be not implied in the definition. But herein we have another instance of Hunter's adhesion, in some respects, to the imperfect views of a preceding generation, and certain remnants of the doctrines of the chemical and intro-mathematical schools of the seventeenth century will be traced in this explanation of the nature of organization. When he proceeds, however, to develop his ideas regarding the "actions of animals," he manifests, in a high degree, the triumph of his vigorous and matter-of-fact understanding over the mere opinions and partial views of his own and preceding times, and briefly describes many of the functions of the animal economy, in a manner almost entirely compatible with all that has been revealed by modern investigation and research.

The "Brain and Sensation" constitute the subject of a separate chapter; and herein we have a summary of the state of information in the department of physiology, as it existed in Hunter's day, without any very striking illustrations. The whole of this matter is, however, so extensive, and the advance that has been made within a recent period is so considerable, that it would be impossible, within our limits, to furnish any epitome of Hunter's views, or to attempt any parallel between the existing condition of our knowledge upon the subject and its state in his own day. We must content ourselves with referring the reader to the volume itself.

We can do little more than mention the title of the next division of these lectures: "Of Susceptibility of Impressions; of Stimuli; of Dispositions of the Body; of Habit and Custom." The notions advanced under these heads are almost altogether theoretical; and, as will readily be conceived, are not such as are best calculated to indicate the powerful and energetic character of the mind from which they emanated. It was in dealing with the facts of nature, capable of actual demonstration by repeated and varied experiments, that Hunter's genius displayed itself; and in the ensuing chapter we have presented to us a subject more con-

genial to his mental habits, that of Animal Heat, and one upon which he threw much light by his investigations.

Hunter rejected all merely chemical or mechanical modes of accounting for the production of animal heat, and anticipated the law, recently established in the most complete manner by the experiments of Dr. Edwards, that the animal power of generating heat increases as its external presence is withdrawn. The following experiment, though undertaken with another object, first suggested this to his mind: besides its inherent interest, it is somewhat amusing from the simplicity with which he announces the failure of a grand speculation which he had entertained, and to which we referred in the account of his Life in our last Number.

“In the year 1766, two carp were put into a glass vessel with common river water, and the vessel was put into a freezing mixture. The water surrounding the fish froze very rapidly on the inside of the glass all round. When the freezing process approached the fish, it became as it were stationary; and the remaining water not freezing fast enough, in order to make it freeze sooner I put in as much cold snow as made the whole thick. The snow round the carp melted. I put in more snow, which melted also. This was repeated several times, till I grew tired, and I left them covered up to freeze by the joint operation of the mixture and the atmosphere. After having exhausted the whole power of life in the production of heat, they froze; but that life was gone could not be known till we thawed the animals, which was done very gradually. But with their flexibility they did not recover action, so that they were really dead. Till this time I had imagined that it might be possible to prolong life to any period by freezing a person in the frigid zone, as I thought all action and waste would cease until the body was thawed. I thought that, if a man would give up the last ten years of his life to this kind of alternate oblivion and action, it might be prolonged to a thousand years; and, by getting himself thawed every hundred years, he might learn what had happened during his frozen condition. Like other schemers, I thought I should make my fortune by it; but this experiment undeceived me.” (P. 284.)

The present chapter abounds in the detail of numerous experiments, upon both man and animals, made with a view to determine the vital capability of developing heat. Those which relate to this faculty in the hibernating species possess much interest, as also those which have reference to the power of resistance to the influence of an external temperature, above the natural standard of the body.

The chapter on animal heat forms the last of that part of the work which is strictly *physiological*, and the six ensuing comprise its *pathological* division. Mr. Hunter commences by remarking that, to understand the imperfections or diseases of the body, “it is necessary, in the first place, to describe its perfect or healthy state;” thus recognizing the principle that physiology is the only sure foundation of the whole science of medicine; and towards the adoption of which principle by all enlightened cultivators of our art, his own labours so eminently contributed. Its elucidation forms the material of all his pathological remarks, which, it were almost unnecessary to say, have their principal application to surgery. Any attempt, however, at analysis, within moderate limits, would be very imperfect; and hence we shall select only particular portions which appear to exemplify the state of pathology at this era, and the mode also in which the subject is discussed in these lectures.

The chapter upon “Sympathy” contains some excellent illustrations of its phenomena. Mr. Hunter, having been engaged in the discussion of

what he had regarded as "primary susceptibility of disease," by which was understood the disposition, in any structure, to assume a morbid action, in consequence of the direct application of some exciting cause, proceeds to speak of what he designates a "secondary susceptibility." This, he observes, arises from sympathy, by which "an action arises without any immediate impression in a secondary way, either acting in conjunction with the part immediately impressed, or taking the whole action on itself." Various modes of sympathy are ranked under distinct heads: thus, he speaks, in the first place, of the sympathy of "a local with a local disease," and of an "universal with a local," holding that all sympathies must arise from a local cause. Another classification of sympathies, which he makes, possesses somewhat more importance; it is into the natural and the diseased: and here follow some remarks which we shall insert at length, on account of their own excellence, as well as of their characteristic style.

"Sympathy may be divided into two kinds, the natural and the diseased. The diseased is when sound parts sympathise with the diseased, and probably the diseased with the diseased, which is what I mean to explain. The sympathy of one diseased part with the diseases of another part will include the idea of revulsion, as revulsion consists in the production of a disease in one part to cure a disease in another part; which shows that this one part, while under disease, can be affected by a diseased action being produced in another part, or the cessation of one action in consequence of another having taken place in another part. Natural sympathy takes place more readily, and its actions are more strongly marked, in proportion as the powers of the machine are capable of repairing an injury received. On the other hand, it takes place more slowly, and is less evident, as the powers of life are more languid. In many diseased states, the condition of the whole body is often such that it more readily falls into sympathy at one time than at others. Thus, we find people at particular periods much affected by slight causes, while at other times considerable mischief received will hardly affect them. Some people are naturally more readily affected than others, as will be evident in disease. Sympathy sometimes proves fatal, as in children from teething. But this depends, in great measure, on the parts sympathizing, or the number of parts that sympathize. Sympathies are often not reciprocal: the liver never sympathizes with the shoulder, nor the urethra with the testis; nor, when the glans penis is affected, does any irritation pass to the bladder; but often they are, as, for example, between the head and the stomach. Sympathies are generally simple; we hardly ever find two parts sympathizing with the same cause: however, the spasmodic convulsion of both hands, or hands and feet, &c., as sometimes takes place, may be called a double sympathy. Sympathy is common and uncommon. The first is where it takes place more readily between some parts than it does between others, as between the stomach and head, the stomach and the skin, the testes and urethra. Sympathy may be called uncommon when parts sympathize with diseased parts that were never known to sympathize in health. A gentleman had a sore on the inside of his thigh, which itched so intolerably that he could not avoid scratching it, and, when he did, it always produced tightness in his chest and shortness of breathing, which he never had but at these times. Lord Cavendish's father always felt pain in the left arm from a stone in the bladder: this pain was the only indication of a want to make water." (P. 320.)

In the next chapter we have some very interesting observations on the general nature of diseases, which are divided into constitutional, local, and mixed. The doctrines which are here advanced are such as, with some slight exceptions, will be assented to in the present day; and we think that Hunter, in the present chapter, shows himself decidedly in advance of his own times, in the pathological remarks which he adduces, and more especially in the views which he offers upon the reciprocal

influence existing, in many cases, between local and constitutional maladies; wherein he maintains and illustrates the doctrine of the operation of general disease in the development of local affections, and of the modifications which wounds, or other local derangements, undergo, in consequence of constitutional peculiarities with which they may happen to be associated. Generally speaking, however, there is nothing in these observations which would command any very special regard in the present age, the whole question having, of late years, been so maturely considered, particularly since the appearance of Mr. Abernethy's work "On the Constitutional Origin and Treatment of Local Diseases."

Inflammation and its consequences are treated of in three chapters, and receive, as might be expected, very full discussion; but any sketch of the doctrines propounded, or any detail of the experiments by which they are supported, will fall more properly under notice when the separate treatise on this topic comes under review; at present, we can only state that Hunter enters freely into the matter in the present volume, adducing, however, in illustration of his views, comparatively few of the facts or experiments on which they rest.

The last nine chapters of the volume before us are given to the practical illustration of the principles previously enounced; and, in these, we have some of the principal diseases, falling under the management of the surgeon, explained, illustrated, and their treatment described. Our limits will permit us only to make one or two references; and, for this purpose, we shall select the sections embracing "Injuries of the Head" and "Aneurism." The former topic does not occupy that amount of space, in the text, which its high importance would seem to demand; but the reader is less disposed to be dissatisfied on this account, as the Editor's marginal notes are exceedingly copious, and exhibit the condition of this branch of surgery in accordance with the principles and practice of the most experienced surgeons of the present day.

Mr. Hunter considers the effects of injuries of the head, mainly, under the popular divisions of concussion and compression. The detail which he affords of the symptoms of these cerebral conditions is, in general, precise and accurate, and such as corresponds with daily observation. When, however, he comes to speak of the treatment of these affections, we are strongly reminded of the great improvements which have been wrought in modern surgery, and most especially as regards the employment of the trephine. Hunter justifies the employment of the trephine, in cases of mere fissure, as the operation *can do no harm*; in cases of ascertained depression of the outer table only, since there can be no certainty regarding the state of the inner table, and neglect to operate might produce mischief hereafter; in young children, in whom depression exists without symptoms of compression, with the same fear of future consequences; and, lastly, in all cases of violence attended with compression, even in the absence of fracture, wherein he declares the operation "somewhere or other" to be absolutely necessary. It is almost superfluous to remark upon the immense advance which has, of late years, been made in the management of such cases; in some of which, the operation would be deemed altogether unjustifiable, and in none would it be deemed applicable without some grave and important reasons, and after the failure of other measures.

The improvement, wrought by Hunter, in the surgical treatment of Aneurism, is one of those facts with which every one in the profession must be familiar. The practice of surgery, in this branch, was, previously to his day, in a very imperfect condition. This cannot be better shewn than by inserting the following passage upon the treatment of aneurism, in which he combats the particular views of Bromfield and Pott; and, as it also offers an excellent illustration of the style and manner adopted in the lectures which treat on practical points, we give it at full length :

“Mr. Bromfield objects to every operation, either amputation, or for the aneurism; this would be just if what he asserts was true, viz. that the whole of the arterial system is in general diseased, which however is certainly not the case. He says, too, ‘that the injecting of parts in dead bodies having shown that in particular subjects the branches sent off have now and then formed anastomoses with other branches given off lower down, has led to very extravagant notions of the smaller branches being always able to carry on the circulation; and an extravagant proposition has been suggested by some people to tie up the principal trunk of an artery in the extremities. I once saw an attempt of this kind in a true aneurism of the ham, in which I shall only remark that the patient died; and I do believe that the embarrassments which occurred, as well as the event of the operation, will deter the gentleman’ (meaning me) ‘who performed it from making a second attempt in a similar case.’ Now unfortunately either for Mr. Bromfield or myself, this is the very case from which I have formed favorable ideas of the success of future operations of a similar nature. Mr. Pott, after describing the disease in its last and most violent stage, just preceding dissolution, and when it has done all the mischief it can do without destroying the life of the patient, says, “If a man was to be asked how the disease was to be treated, he would answer, from theory, that the artery should be tied above and below the tumor, and the coagulated blood be evacuated; but that the artery is generally diseased some way above the dilatation, especially the popliteal.’ He also observes, ‘that the want of collateral branches of sufficient size to carry on the circulation is another powerful impediment to the operation.’

“When the aneurism has arrived at the stage which Mr. Pott describes, perhaps the only thing is to amputate above the dilated part of the artery; but Mr. Pott should have considered, that before these threatening symptoms there is a stage when all the surrounding parts are sound. If this be true, would any man allow a disease in a part to go on till the surrounding parts are diseased and past cure?

“The events of all diseases are of two kinds: first, where the termination is certain; 2d, where it is dubious. The aneurism is of the first kind; its event is certain. Now I do aver, then, that there is a stage of the disease in which the operation is safe. I do not, certainly, know how to judge of this state from the external appearance, but from what I have seen of aneurisms, I believe it will allow of considerable latitude. My opinion is, that the operation should be performed, 1st, when the disease has done no mischief to the surrounding parts; 2d, where it is distinct and circumscribed, not connected with parts which may not be curable when exposed, as bones; 3dly, when there is a distinct pulsation. How early the operation may be performed I do not certainly know, but my opinion is that it may be done as soon as the aneurism is known to exist. By some it has been recommended to permit the disease to exist some time first, because, say they, as the circulation becomes obstructed, a freer communication will take place between the branches above and below; but this would not be until the obstruction had subsisted some time, and I would not wait for this, for fear of the consequences described by Mr. Pott.

“When the disease is in an advanced stage, I agree with Mr. Pott in thinking amputation necessary and preferable, but not under the circumstances above mentioned. The earlier, therefore, the operation for the aneurism is performed the better, not waiting with the expectation that an increased size of the aneurism will produce an increased size of the collateral branches. That the popliteal artery, according to Mr. Pott, is oftener diseased above the aneurism than other arteries, I cannot well

determine, but can see no reason why it should be so. If the artery, however, cannot be tied above the aneurism in the operation, where can it be tied if the limb be amputated? Why not tie it up higher in the sound parts, where it is tied in amputation, and preserve the limb? The circumstances to be regarded chiefly turn upon the collateral branches being sufficient to carry on the circulation. The only branches which admit of this question are the popliteal, femoral, and brachial; the other arteries either having a very free anastomosis, or being out of the way of any operation.

"In this account it may be supposed that I carry my notions too far; but it is to be understood that I only give my own feelings upon this subject, and I go no further in theory than I would perform in practice, if patients, being acquainted with the consequences of the disease, would submit to, or rather desire, the operation; nor do I go further than I now think I would have performed on myself were I in the same situation. Not that I would have it supposed that I would recommend this at large: I would have no one perform an operation that he is not clear about the propriety of himself, especially when it requires more anatomical skill than falls to the share of most practitioners." (P. 647.)

In giving directions for the operation, Mr. Hunter does not, in the present volume, describe the one which usually goes by his name, and the introduction of which into practice will ever constitute one of his strongest claims to the gratitude of mankind, that of tying the artery at a distance from the tumour, and between it and the heart,—but he directs that which was most commonly in use previously. The reason of this is explained by the Editor, in a marginal note, from which it appears, by a comparison of dates, that this lecture was only delivered about a year after the first performance of this latter operation; and it seems that, with his wonted philosophical caution, he did not yet think himself justified in pronouncing definitely upon its superiority.

We cannot bring our notice of the present volume to a close without offering our testimony to the admirable way in which the editor and annotator has fulfilled his part of the undertaking. The advancement and improvements that have been effected, up to our own day, not only in practical surgery, but in all the collateral departments, are constantly brought before the reader's attention in clear and concise terms. The errors of expression also, into which Hunter was apt to fall, are judiciously pointed out, and rectified where, from want of precision, the student might be led into error. Indeed, taking these lectures in conjunction with the notes, we hardly know any work that will be read with greater advantage. The student may obtain from it an excellent initiation in the principles of his future profession; the surgeon will receive many excellent hints which he may readily discover opportunities of applying in practice; and the physician may gather from its contents just and philosophical notions of the true theory of the whole art of healing, which cannot fail to heighten his devotion to its pursuit. We think, moreover, that no one will finish the perusal of this publication, without some feeling of honorable anxiety to imitate the virtues and labours of John Hunter, who not only now, but probably in all future time, must be regarded as the pride and boast of British surgery.

ART. IV.

Essai sur la Colique de Plomb. Par AUGUSTIN GRISOLLE, M.D., Membre de la Soc. Méd. d'Obser. de la Soc. Anatom., &c.—Paris, 1835. 4to. pp. 84.

Essay on the Colic produced by Lead. By AUGUSTIN GRISOLLE, M.D., &c. &c.

Few subjects connected with medicine have engrossed a larger share of the attention of medical men than the effects of lead on the animal economy. The physiologist, the toxicologist, the pathologist, have, in their respective walks, laboured in the solution of a variety of problems concerning them. The result is, that we possess much apparently valuable information on the phenomena which attend the introduction of lead and its compounds into the human body; and, what is most important, on the treatment of the morbid states to which it gives rise. But, on close enquiry into the character of the pathological facts, especially, which are looked on as established, we are forced to confess that vagueness and insufficiency of proof mainly distinguish them; and although there is, perhaps, no affection more completely under the control of the healing art than saturnine colic, such variety exists in the modes of treatment, and the alleged superiority of any mode above the rest is so little evident, that it must be admitted that we are, as regards that affection, far from possessing any thing like a perfect therapeutical code. The reason of this seeming wealth and real poverty is simple. Writers on this, as on a multitude of other medical questions, have contented themselves with recording a series of general conclusions founded on the hasty and insufficient observation of a few facts. The inductive method has been here on the same pretexts which have been hitherto so plausibly and too successfully urged against its use in other medical researches, utterly neglected. Yet it is impossible perhaps to find a disease so fitted in every point of view for its effective application as that which forms the subject of the treatise before us; for the objections most commonly urged against the value of the inductive and numerical methods, namely, the disparity of years, the difference of sex, of trade, of ordinary habits, existing between the individuals whose cases furnish general conclusions, in this instance scarcely exist. In a series of patients affected with saturnine colic, it will be generally found, that their trade is the same, their age almost constantly so, that they are with rare exceptions all males, their weekly gains exactly alike, and consequently their habits of living almost the same.

M. Grisolle, author of the work before us, has in it shewn that he fully understands the value of the numerical method. The importance of his results amply justifies his use of it, and is the best reward of his diligence. His position as house physician to the Hôpital Beaujon, where a great proportion of the cases of lead colic in Paris are treated, gave him opportunities which are rarely met with. The reader must not expect to find in this work a number of brilliant speculations, but what is infinitely more valuable, old opinions disproved or substantiated by the analysis of numerous and accurate facts, and some original propositions established in the same manner.

The author's results are obtained from fifty-eight cases of lead colic

collected at the above hospital in 1834. The patients were chiefly supplied by the manufactories of ceruse at Pecq and Courbevoie.

In obedience to the precept of Cicero, "*omnis institutio debet a definitione proficisci, ut intelligatur, quid sit id, de quo disputatur,*" our author commences by this concise and clear definition:

"The disease is characterized by violent abdominal pains, not increased by pressure, occurring in paroxysms, accompanied by bilious vomiting, obstinate constipation, slow pulse, and lastly by cramps and other painful sensations of the extremities." (P. 12.)

Passing to the causes of the affection M. Grisolle examines the influence of the seasons on its production. Having first ascertained from the heads of the establishments at Pecq and Courbevoie, that they had about the same number of workmen in employ at all periods of the year, he consults the hospital register, and finds that in the course of eight years 285 ceruse workers had been admitted. The facility of forming a correct diagnosis in cases of lead colic removes all fear of error in the register. These 285 cases occurred in the different months as follows:—

January 22.	April 19.	July 23.	October 28.
February 23.	May 24.	August 35.	November 35.
March 23.	June 21.	September 19.	December 13.

These numbers are extremely variable; and even by grouping the four hottest months, May, June, July, August; the four coldest, November, December, January, February; and the four of medium temperature that remain, the difference is trifling; the sums being 103, 93, and 89. Our author observes that "such difference is not considerable enough to allow us positively to affirm that season exercises a direct influence in the production of the affections resulting from the poison of lead," (p. 14.) The fact that the smallest and largest numbers are found in two adjoining winter months would, we think, warrant a more absolute assertion than this. The figures are of importance, inasmuch as they shew the general impression of the agency of season in causing the disease is erroneous; as also, to a certain degree the statement of Professor Chomel, who says (*Dict. de Méd.* 8, 383,) "*lead colic is very rare at some periods of the year and becomes very frequent at others . . . especially in summer.*" This increase in the summer months is generally, he conceives, dependent on an increase in the activity of the manufactories at that period, and a consequent increase in the number of workmen employed and of hours devoted to labour daily. But M. Grisolle, as we have stated, ensured himself against this source of fallacy; his results therefore must be received as correct. The question is evidently important in a hygienic point of view.

Lead colic is liable to attack those who use the metal itself or any of its preparations: a variety of trades are consequently exposed to its ravages. But it would appear that some conditions of the metal are more deleterious than others, to a degree which could not be supposed *a priori*. Our author's researches shew that, while 285 workers of ceruse were treated at the Hôpital Beaujon in eight years, only thirty-two painters and colour-grinders received medical aid there during the same period. It must not be forgotten that the number of persons employed in the manufacture of carbonate of lead is much less considerable than that of the other classes just alluded to.

One half of M. Grisolle's fifty-eight patients were between thirty and forty years of age, but he justly remarks that before concluding hence that a particular age predisposes to the affection, it would be necessary to know the ages of all the workmen employed in the manufactories in 1834. He arrives at a result of some value by other means: "having arranged my patients in four sections, comprising those from *æt.* eighteen to thirty, thirty to forty, forty to fifty, and fifty to seventy, I found the mean length of their respective sojourns in the manufactories, sixty-five, sixty, fifty-eight, and thirty-seven days. Hence it appears that the number of days necessary to catch the disease diminished as the age of the subjects increased." (P. 15.)

Women are rarely employed in ceruse manufactories; hence colic is seldom observed amongst them. In the three cases which M. Grisolle met with, the affection was contracted in three months, whereas the mean stay of the males at Courbevoie was 101 days. Were these numbers on a larger scale, they would indicate a greater proneness in the female than in the male constitution to suffer from the noxious influence of lead.

Authors have laid down as an established fact that the manufacture of minium is more dangerous than of ceruse. The very contrary is shewn to be the truth by the calculations of our author: "eleven labourers were able to continue working at minium for seventy-three days without an attack of colic, while sixty-five days was the mean time that twelve others, employed on the carbonate, were able to continue their work," (p. 17.) M. Grisolle says nothing on the relative intensity of the colic when once declared. Our experience would lead us to consider that caused by minium as the most violent; we have not however tested the correctness of this opinion by the numerical method.

Foreign bodies are introduced into the economy in three ways: by cutaneous absorption, by absorption from the surface of the alimentary canal, and from that of the air passages. It becomes a question to determine by which of these ways lead penetrates into the interior of the body. M. Grisolle rejects the agency of the skin as wholly unproved. He founds his rejection on the following grounds: the frequent surgical use of preparations of lead, particularly the experience of Dupuytren who constantly employed the acetate as a local application to burns, and consequently to a denuded surface, and without the occurrence of any accident that could be ascribed to it. The instances quoted by Wall and Percival, in which colic and paralysis followed its external employment, appear to our author "unworthy of confidence on account of the total absence of detail in their relation. Instead of a complete description of the previous history and symptoms of the patients, I find only the opinion of the writer, of which it is impossible for me to ascertain the correctness," (p. 20.) But the facts recorded by Wall, if not accompanied with sufficient accuracy of detail to prove absorption through the skin, do not appear to us to be of a character to be thus set aside. The infant state of the physiology of cutaneous absorption leaves us without any *à priori* notions on the subject. We can scarcely doubt that, as M. Grisolle observes, if penetration do take place through the skin it must be a rare phenomenon in labourers, whose skin is rough, thick, and almost impermeable.

Our author considers the activity of pulmonary absorption in the introduction of lead established by numerous facts. We deem it unnecessary

to enumerate them; the supervention of the disease in individuals who had simply slept in a newly painted room, (a case of the disease contracted under these circumstances was some time past observed by M. Louis,) is in itself sufficient to demonstrate the reality of such absorption.

M. Grisolle's cases prove in a remarkable manner that preparations of lead applied in a state of molecular division to the mucous membrane of the air passages, need not act as irritating bodies. "All my patients, with one exception, were totally free from cough, though working in an atmosphere thick with white lead; yet four of them had old thoracic affections. Three had been several years asthmatic from pulmonary emphysema, the fourth presented the symptoms of a collection of tubercles under the right clavicle," (p. 21.) Besides, not a single patient suffered from inflammation of the mucous membrane of the mouth and fauces, parts so evidently exposed to the constant action of the volatilized ceruse. Commenting on these facts, our author remarks, that daily observation proves the slight effect solid bodies reduced to an impalpable powder exercise on the mucous membrane of the bronchi. We must beg to offer our protest against this assertion. The researches of Drs. Hastings and Knight as clearly point out the destructive influence of volatilized mineral matter on the bronchial membrane, as they demonstrate that such influence does not induce tuberculization.* The number of cases alluded to by M. Grisolle is too small to warrant a general conclusion; or we might infer that lead in a pulverulent state is less deleterious as a bronchial irritant than other powders. In the celebrated case of the flint-cutters of Blois, Andral found that the molecules from the stone (to the inhalation of which the prevalence of pulmonary disorder among them was universally attributed,) did not and could not reach their mouths at all. Such an explanation of the harmlessness of the white lead cannot be admitted, for the whole atmosphere of the manufactories is charged with it in a pulverized state.

The effects of using water received in leaden cisterns in contact with the air, formerly a prolific source of colic at Amsterdam; the occurrence of saturnine colic from the use of wine adulterated with litharge, (a sort of epidemic of the affection has been more than once so produced at Paris); the occasional appearance of the disease in individuals taking preparations of the metal medicinally,—seem to M. Grisolle more powerful proofs of absorption by the digestive surface than can be furnished for either of the other modes. A question of practical interest springs from this part of the subject, viz. the quantity of lead which the economy can bear as a medicine. It unfortunately varies so much in different individuals that no general rule can be laid down. Thus M. Chomel has seen the exhibition of a drachm of liquid acetate of lead followed by colic; and a case has been given by M. Devergie in which such serious symptoms attended the ingestion of three pills of the same salt, that the attention of the magistrates was roused, and the apothecary who prepared the pills cited before them: on analysis, M. Devergie found a single grain only in

* Dr. Knight's case, in which the cicatrization of caverns took place while the patient was constantly exposed to the irritating influence of his trade, affords perhaps, as far as a single case goes, the most admirable refutation of the doctrine of irritation that the science possesses.

each of the remaining pills. On the other hand, Barbier (Mat. Méd. 3, 627,) has given 240 grains of the acetate in thirty-seven days, without any apparent ill effect on the digestive and nervous systems. M. Fouquier has given eighteen grains per diem for three months without intermission with similar freedom from accidents. We have no doubt but that some common character exists in the different series of patients on whom the action of the medicine is so widely different. This common character can only be discovered by accurate observation and the numerical method.

We gave some important observations on this subject, in our last volume, by Professor Mitscherlich. (See Selections from the Foreign Journals, p. 208.)

The presence of the acetate in the blood has been ascertained by Tiedemann. He detected it in the mesenteric and splenic veins of dogs to whom he had administered that salt. Hence, say the analogical reasoners, we must find evidence of the presence of lead in the blood and secretions of individuals affected with lead colic. Yet M. Grisolle sought in vain for any traces of its existence in the urine, in the matter vomited, in the blood, saliva, peritoneal serosity, bronchial mucus, and feces of his patients. The experiments were made with sulphureous preparations. The same is not the case with ferrugineous compounds: iron has been detected in the urine in the state of prussiate by Mojon of Genoa.

M. Grisolle extended his researches on the effects of lead to the inferior animals. He found them very different in different species. Thus, at one manufactory he saw horses that had been employed for ten and fourteen years, and which were covered with a coating of white lead, and yet enjoying perfect health. Cats bring forth their young in a bed of ceruse, and are perfectly exempt from its ill effects; yet cats and dogs die, almost without an exception, from frequenting the establishments.

On the general action of lead before the supervention of colic, and the precursory symptoms of the latter, our author has given two chapters: for these we must refer our readers to the original.

All the symptoms of the affection, and their progress, are placed before us with clearness and accuracy; each symptom is then examined apart in all its details. We shall content ourselves with noticing a few points among these.

It is matter of common belief that the abdominal pain in lead colic may *always* be relieved by pressure. Such is not the truth, however. In forty out of fifty-two cases, the pain was assuaged by gradual and continued pressure; in seven it was uninfluenced by it, and in the remaining five augmented. We have ourselves observed cases of the latter peculiarity at Beaujon: it appeared to us dependent on an inflammatory complication. Retraction of the abdominal parietes (another symptom announced as constant in its occurrence, by some authors,) was only observed fifteen times in forty-six cases; in the remaining thirty-one patients, it had its ordinary degree of development. The fact is, that not unfrequently the size of the abdomen is considerably increased.

The appearance of the tongue (a point of so much importance, according to the doctrines of the physiological school, and which the accurate M. Louis has shewn to be of infinitely less importance than that school would have it,) was not neglected by our author. He found it

humid and of natural colour, and slightly furred in half his cases. In one instance during the treatment, the tongue became greyish, furrowed, and dry; the pulse rose to eighty-eight, with febrile heat. These symptoms lasted only twenty-four hours, when the tongue recovered its moisture. These results coincide exactly with those obtained by M. Louis in the same affection. (*Fièv. typh.* 2, 103.)

According to the assertion of Stoll, patients who vomit during the colic recover more easily and rapidly than those who do not present this symptom, though the sufferings of the latter are less acute. The result of M. Grisolle's observation is completely at variance with the opinion of the celebrated German. Thus, the mean duration of the affection in patients who vomited was eight and a quarter days; in those who had slight nausea and bitter taste in the mouth, five days; in those who were without gastric symptoms, four and a quarter days. The experience of M. Grisolle is again in contradiction with that of Stoll on the character of the pulse. The latter states that its hardness is uniformly greater than in any other affection: M. Grisolle, having attentively sought this peculiarity, found it in three cases only.

The modifications of the urine were slight. They were confined to trifling decrease in quantity, red colour, and deposit of sediment. Its excretion was seldom attended with scalding. M. Grisolle never observed a sudden stoppage of the stream of urine; a phenomenon stated to occur sometimes, by Stoll, Bauer, and Dance, and attributed to sudden contraction of the urethra.

We are next presented with an elaborate examination of the various lesions of sensibility noted in the progress of the disease. These were by no means limited to the primitive seat of the affection, the abdomen. Pain was the most remarkable of them, and is treated of in so many separate sections, as it occurred in the genitals, extremities, loins, thorax, and head. That in the genitals (usually supposed to exist in the testicle,) was, in the majority of cases, placed in the spermatic cord. It occurred in the fourth part of the patients, and was in many instances attended with retraction of the testis, but never with redness or tumefaction of that organ. The character of the lumbar pain seemed favorable to the theory of Astruc, as to the seat of the disease being in the medulla spinalis. It was increased by pressure, and, radiating from the spine, passed round to the epigastrium, sternum, umbilicus, and walls of the pelvis, exactly as that symptomatic of a lesion of the medulla spinalis.

Many authors have asserted that the pain in lead colic is subject to nocturnal exacerbation. Stoll goes so far as to consider this peculiarity more marked even than in syphilis. On this M. Grisolle remarks, "I am inclined to think this an exaggerated statement; for, of eighteen patients carefully questioned on this point, ten only suffered an exacerbation of pain by night." (P. 44.) We regret that we have not room for more detailed notice of this part of the treatise.

The lesions of innervation observed by M. Grisolle were not confined to these. The most important affections were epilepsy, paralysis with coma, and general trembling. The intellectual faculties were more or less disordered in two cases: in one there was a state bordering on dementia, in the other the symptoms resembled those of delirium tremens. Epilepsy occurred twice only. In both cases it came on suddenly, and

the proper symptoms of colic were very slight: both patients died within thirty-six hours from the moment of the attack. In these instances the author does not attribute the convulsions "to sympathy, nor to a reaction of the nervous centres produced by the intensity of the pain, but considers it necessary to admit an immediate action of the lead on the cerebro-spinal system." (P. 46.) We are fully disposed to agree with M. Grisolle in his mode of explaining these nervous phenomena. It seems, indeed, impossible to suppose them, as some authors have done, a sympathetic effect of the abdominal affection, when we recollect that they supervene occasionally before the colic itself, or persist frequently after its total cessation.

The nature of lead colic is a point which has given rise to much warm dispute. Four different opinions have found abundance of supporters. In giving a sketch of them, we shall endeavour to settle, at the same time, the question of the "anatomical character" of the disease.

1st. The disciples of Broussais have, of course, not failed to discover in the few cases where the use of the scalpel was possible, the omnipresent gastro-enteritis; broadly asserting (*Ann. de Méd. Phys.* 9.) that, in every instance in which examination of individuals affected with lead colic had been made, the mucous membrane of the intestinal canal was found inflamed. Yet not a single author, unprejudiced by systems, has found the least sign of inflammation in the part to which we have alluded. Our author's cases add to the number already laid before the profession by Andral, Louis, Laennec, Martinet, Ruz, &c., in which the most accurate search failed to detect inflammatory changes. In M. Grisolle's two cases, there was neither "anormal coloration, softening, or thickening of the tissues composing the digestive tube," (p. 52;) although in these cases there was an additional cause, according to the Broussaian theory, for the production of enteritis,—the use of drastic purgatives. Death had occurred in the two cases from epilepsy; a third case, of fatal termination from cholera, was, of course, unfit to aid in solving the question. "If," observes M. Grisolle, "lead, in the molecular state, produced even slight inflammation by contact with the digestive mucous membrane, it appears to me that the symptoms of colic should be more rapid in their occurrence and intense in their degree, when the metal is introduced directly into the alimentary canal. Now, the exact contrary is the truth: those who respire emanations from lead contract the colic more easily than those who swallow the metal; which, too, when given in large doses, induces the symptoms, not of saturnine colic, but of poisoning; that is, of a violent inflammation of the mucous membrane of the digestive tube." (P. 52.)

2dly. Coarctation of the intestine has been laid down by some writers as the efficient cause of lead colic. Our author justly rejects this, as constituting a peculiar character of the disease. Irregular contraction of the intestine is a phenomenon observed after death from a variety of affections, and cannot be considered otherwise than as an accidental circumstance in colitis pictonum, when it occurs. In six cases examined by Andral, there was no diminution of the caliber of the intestine. We must not omit that, in one of four cases in which M. Grisolle attempted to introduce an œsophagus tube into the colon per anum, he was able to pass only eight inches; but it is impossible to assert that its stoppage

was not due to a collection of hardened fæces. In the other cases it penetrated to the end, and without the least difficulty.

3dly. The opinion which places the seat of lead colic in the medulla is supported by hypothesis alone. In one of his epileptic cases, M. Grisolle found a general diminution of consistence in the medulla; the brain was similarly affected in both subjects. But these cases can in no wise throw a light on the pathological anatomy of the colic itself. They corroborate, as far as regards saturnine epilepsy, the details of cerebral lesion given by Miguel, Laennec, and Caseaux. But, in a very large majority of cases of epilepsy due to the poison of lead, no such lesions have been found.

4thly. The opinion that the disease is of a purely nervous nature is adopted by M. Grisolle. "The absence of fever, the violence of the pain, its relief by pressure, and special character of returning by paroxysms; its occasional radiation in the known course of certain nerves; the white flat tongue, obstinate constipation, scalding urine; the interruption of the symptoms, which sometimes lasts for several days, suffice to prove its nervous nature." (P. 54.) This opinion is borne out by "the total absence of any appreciable lesion in the intestinal canal, and the almost constantly happy effects of narcotics and irritants." (P. 55.) M. Grisolle does not attempt to decide whether the disease be seated in the filaments of the cerebral nerves or of the sympathetic. "However this may be," he adds, "the spinal marrow would appear, in severe cases, to share the poisonous influence of the lead, whether the metal exercises a direct modification on it, or through the extensive communications which exist between the ganglia and rachidian nerves."

M. Grisolle presents us with nothing new on the prognosis. An interesting calculation is given by Professor Bouillaud, from which it appears that, in 3,569 cases of lead-colic, death occurred *ninety-five* times, or a little less than once in thirty cases. All these deaths were the effect, not of the colic itself, but of various complications, particularly affecting the nervous system. The patients who furnished materials for the above calculation were, almost without exception, treated at La Charité.

The treatment of the affection is subdivided by the author into prophylactic and curative. The use of certain hygienic precautions in the mode of construction of the manufactories, and the conduct of the workmen, constitute, according to M. Grisolle, the only true prophylaxis of lead colic. In this he is supported not only by the favorable results obtained from such precautions, but by the complete inefficacy of certain medications, the prophylactic virtues of which have been extolled from time to time. The essential conditions for attaining the greatest possible healthiness in the establishments is, that they should be built on an elevated situation or on the banks of a river; that there should be a continual current of air through them; and that the necessary machines be covered as much as possible, so as to prevent the dust from reaching the workman.

With respect to the labourers themselves, the evil influence of spirituous excess points out the necessity of abstinence: the day's work should never be commenced on an empty stomach; for it appears that those whose habit it was to do so "were seized with colic after *forty-eight* days' work, on an average; while those who followed the contrary plan

remained unaffected *sixty-five* days." (P. 63.) The presence of white or red lead in the rugæ and depressions of the skin indicates the necessity of using the warm bath occasionally. We may remark, that M. Gendrin has somewhere attributed to the oxide and carbonate of lead, thus combined with the epidermis, the various effects of the metal distinct from the abdominal affection. Unquestionably, as M. Grisolle believes, such an opinion must be erroneous; "not that we are authorized to deny cutaneous absorption, but because the presence of molecules of lead on the skin is a *constant* phenomenon, in my opinion, inasmuch as I never looked for it without succeeding in my search." (P. 64.) Our experience coincides with that of M. Grisolle on this point: in upwards of *thirty* cases, we have invariably found the metallic incrustation simply with the naked eye, and without the help of any chemical reagent. In order effectually to remove it, M. Grisolle recommends the use of the sulphur bath, followed by a simple warm bath, to detach the recently formed sulphuret.

Our readers are probably aware that in 1832, M. Gendrin, to whom we have just alluded, presented a memoir to the Institute, undertaking to prove the preventive powers of sulphuric acid against the development of lead colic. The results obtained by our author on this question (who has put to the test the accuracy of M. Gendrin's allegation,) are as follows:*

"Nineteen labourers, who had used the sulphuric tisane, were obliged to give up their work, after an average stay of fifty days; whereas, twenty-five others, who employed no prophylactic measures, did not contract the disease until after ninety days." (P. 65.)

These calculations are made from all the patients, taken without distinction; no matter from what manufactory they came.

"Next comparing amongst themselves the labourers of the same establishment, I found, for that of Pecq, that those who drank no sulphuric acid fell ill after forty-four days, while those who made use of it resisted forty-nine days. At Clichy, seventy-six days was the mean stay of those who used none of the tisane, fifty that of those who employed the prophylactic treatment of M. Gendrin." (P. 65.)

From these facts our author deduces the uselessness of sulphuric acid as a prophylactic. Our limited experience in this matter accords with his. We have observed in the few cases where the workmen had been prevailed on to use enough to give it a fair trial, that it was wholly wanting in the miraculous virtues attributed to it by M. Gendrin. It has even produced slight diarrhœa, loss of appetite, and gastric derangement in many cases after a few days' use. If the testimony of four cases may be relied on, nitric acid is equally powerless in fulfilling the same indication; our author's experience of it does not extend further.

We have already alluded to the multiplicity of modes of treatment of lead colic that claim for themselves the first rank in point of efficiency. The strong desire to be useful has in some cases blinded medical innovators as to the value of their favorite treatment; in others attachment to particular systems has been the cause of error. Nothing is more easy

* The sulphuric tisane was composed of a drachm and a half of sulphuric acid at 66° to three litres of water, and several ounces of moist sugar. It was supplied *ad libitum* to the workmen.

than to persuade oneself of the superior powers of any given treatment. Cases of mild and severe character, those treated at their outset or at their decline, need only to be jumbled together, and favorable results must necessarily be obtained. But those who seek truth, and not specifics, avoid this unfair method of proceeding. A single case will illustrate our assertion. Our readers, no doubt, recollect the praises bestowed on the alkaline principle of the willow, salicine: it was announced by more than one as a sovereign remedy for intermittents, and, from its cheapness, recommended to our sympathies as the poor man's cure. Yet, when tried by an accurate observer, its reputation was proved to be wholly unmerited. Professor Chomel (the observer to whom we refer,) made his experiments on those patients only whose affection was not fast tending to a natural recovery, and in whom it was not of that mild character which allows of a favorable termination without any treatment at all.

The method of treatment most frequently employed in the cases of M. Grisolle, whose details on this part of the subject exhibits the most praiseworthy accuracy, was that by purgatives and opiates combined. We shall do our author the justice to give, in his own words, the summary of his observations.

"The following enema was administered, either alone or along with opium, to sixteen of my patients:—Dec. Sennæ, ʒj.; Sodæ Sulph., Mel. Mercurial. Ann. āā ʒij.; Aquæ, q.s. Its exhibition was uniformly followed, within from three to twelve hours, by a remarkable degree of relief. Of these sixteen patients, seven were affected with violent colic; altogether each of them took during the treatment, which lasted, on an average, five days and a quarter, six enemata and five grains of opium. I dated the recovery from the moment the abdominal pains ceased, the alvine evacuations were restored, the appetite returned, and digestion was easy. The other nine individuals were affected with less severe colic than the preceding; they recovered, on an average, in four days and a quarter, after having had four purgative enemata and three grains of opium. In half the cases just analysed, I noted vomiting; but this symptom ceased on the supervention of the alvine evacuations. The cessation of pain must not be ascribed to the opium alone; for it was not administered to six of the above patients, and in every case it was not given until the constipation was overcome, and considerable amelioration was obtained in other respects. When the colic was of the severe kind, the first enema rarely produced much effect. It was occasionally necessary to administer several, from three to five, for example, one after another, to overcome the constipation when very obstinate. The fæces first voided were either dry and black, or yellow and of slight consistence." . . . "Six patients were treated at the same time with purgatives by the mouth and in enemata. This last method produced the most abundant evacuations and rapid relief. Recovery took place in this series in seven days and a quarter. The purgatives used were three in number: Castor-oil, ordinarily combined with croton-oil, in the dose of from one to three drops. The latter never produced superpurgation; sometimes, indeed, it had no effect at all. The 'huile d'épurgé' was also administered in drachm doses, and almost always produced vomiting, with or without purging." (P. 69.)

"It became necessary to give up the use of purgatives in four cases, from their want of success, and on account of the supervention of febrile symptoms. Venesection was performed in two of these cases. The blood was very rich in both instances, and buffed in one of them. The serosity, small in quantity, was treated by the reagents of the salts of lead, without presenting either black coloration or precipitate." (P. 71.)

M. Grisolle's experience is unfavorable to the use of opiates alone, as recommended by Stoll; and to the modes of treatment by tobacco, sulphuric acid, and antiphlogistics: on the latter, particularly, he has given some very interesting information.

ART. V.

What Asylums were, are, and ought to be: being the Substance of five Lectures delivered before the Managers of the Montrose Royal Lunatic Asylum. By W. A. F. BROWNE, Surgeon, Medical Superintendent of the Montrose Asylum, &c.—*Edinburgh, 1837.* 8vo. pp. 231.

It must, we think, be considered a fortunate circumstance for any asylum when a book of this import is published by its superintendent. The reflections in which it must have originated cannot but have reacted favorably on the mind, and have preserved it from falling into common systems of treatment,—the offspring of indolence, inhumanity, or impatience. Of all situations, that of the superintendent of lunatics calls for the most vigilant industry, the most unwearied patience, and a humanity most proof against disappointments. No one who is practically acquainted with the character of lunatics, with their cunning, their obstinacy, and their perverseness, can be surprised to find how many keepers of lunatic-houses at length desist from curative efforts, medicinal or moral, and become content to keep their patients well fed and out of the way of accidents. But as the undoubted consequence of this quiescent method is, that many lunatics are deprived of the chances of cure, and many continue to be unjustly confined for life, the merit of a superintendent is very great whom all the troubles of an asylum cannot divert from the great duty of providing for the cure of the greatest possible number of his patients, and for the comfort of all. Regardless of their insensibility and their ingratitude, such a man is merciful to all who are placed under his control, and imitates, at humble distance, the just equanimity of a higher Power, on which he and they alike depend.

The title of Mr. Browne's first Lecture is, *What is Insanity?* and of the second, *What are the Statistics of Insanity?* The third, fourth, and fifth refer to the questions of what asylums *were, are, and ought to be.*

Unfortunately for Mr. Browne, our critical composure is disturbed at the outset, by the impossibility of digesting two diametrically opposite assertions: "I have no claim to originality," says Mr. Browne, in the second page of his preface; but, in the first page of his book, and the very first sentence, he says, "The pages which I am about to submit to you, and subsequently to the public, possess one quality which many regard as a merit, but which I am inclined to think is a misfortune. It is that of originality." Never did author commence by placing his readers in such a dilemma; from which we extricate ourselves by believing the assertion in the preface, as of later date, and made with more deliberation; to which we are furthermore inclined, as it may relieve the author from the oppressive sense of that originality which he regards as a misfortune, and the straining after which is a calamity from which he cannot always be said to escape. This admission by no means abates from his merit in bringing forward known facts in a new light, and enforcing valuable truths anew, for an important and a philanthropic purpose.

Mr. Browne appears to us to confound two things essentially different, in his chapter on the nature of Insanity. Abjuring all attempts to define insanity, which he considers to be only so many attempts "to discover one form of words expressive of the nature of a hundred different things,"

he seems to look upon such definitions as having for their great object the pointing out of the limits which make confinement or restraint necessary. It is, however, one thing to define the meaning of unsound mind, and another to point out the cases which require confinement or restraint. Mr. Browne's professed views have enabled him to pass over, unnoticed, any definitions of others; and yet, when attempting a division of the species of insanity, after quoting Arnold's and Heinroth's, he gives, as "the best," his own division: that division actually includes a definition, which he appears to consider his own, and which he repeats at least eleven times in his first chapter. We think he must have noticed something very much like this definition, and also an approach to exact rules for applying it to practical purposes, among the recent works to which he makes brief and occasional reference. We trust we can make this matter clear, with perfect fairness to Mr. Browne, by quoting his own

"ARRANGEMENT.

"I. IDIOCY. Non-development of faculties.

1. Gradation. Non-development of all the powers.
2. — External senses developed.
3. — A propensity or affection developed.
4. — An intellectual power developed.

"II. FATUITY. Obliteration of faculties.

1. Partial.
2. Complete.

"III. MONOMANIA. Derangement of one or more faculties.

Section 1.

1. Satyriasis.
2. Homicidal and destructive.
3. Proud.
4. Vain.
5. Timid.
6. Cunning and suspicious.
7. Religious and superstitious.
8. Desponding and suicidal.
9. Imaginative.
10. Avaricious.
11. Benevolent or affectionate.

Section 11.

12. Incapability of perceiving relations of ideas.
13. Incapability of perceiving relations of external things.
14. Incapability of perceiving qualities of external objects.

"IV. MANIA. Derangement of all the faculties.

1. Mania with increased activity.
2. Mania with diminished activity." (P. 12.)

On inspecting this list, it appears evident that Section II. of Monomania applies to every case in Section I.; that it is their *definition*, and also that of all the varieties of Idiocy, Fatuity, and Mania. In all, the relations of things or of ideas are incorrectly perceived. This the author himself says over and over again, when separately describing the eleven varieties of Section I.; and he might certainly have said it all through. The conclusion virtually admitted, without notice or mention, is, that insanity consists in such a disturbance of any faculty as leads to an incapacity of perceiving the just relations of things or ideas. But

how are the just relations of things or ideas perceived? By comparing one thing with another, or one idea with another. Mr. Browne will, therefore, we are sure, pardon us if we remind him that this is the definition of insanity of a previous author; the principal object of whose work was no other than to show that many who come under this definition were still not subjects for confinement; but solely those whose comparing powers were injured on such points that they could not be at liberty with safety to themselves or others, or to the property of themselves or others, or to the feelings of themselves or others. Both the definition and the rule are adopted by Mr. Browne; but he is oblivious when he regards them as new. We are not the less glad to see the definition appear in any shape, for we believe it to be true; and we are even more glad to see the rule enforced, because it alone is consistent with humanity, or even with justice.

Readers acquainted with the writings of the phrenologists will see that Section 1. of the arrangement we have quoted nearly follows the phrenological arrangement of the faculties. Mr. Browne acknowledges his belief that by no other system can insanity be understood, described, or treated. We admire the boldness with which he avows his attachment to a system which is too often mentioned merely for the purposes of ridicule; and we do not hesitate to say, that we think the diversities of the mind, sound or unsound, are only explicable on the first principles of those who consider the brain as a congeries of organs. In another respect, however, we cannot at all agree with the author. Smitten, we fear, with the presumptuous philosophy of the French physiologists, he is dissatisfied with any theory of insanity which does not refer every case to lesion of the structure of the brain, and account for the degree of the malady by the degree of palpable lesion. In the majority of fatal cases, we certainly should expect something more than functional disorder; structural lesion, the result of long-continued disease; but, as Mr. Browne admits that the brain may be irritated by the disorder of other organs, we think he must also admit that its mere functional disturbance may occasionally induce violent symptoms of disordered mind. When physiologists know something of the mode in which the brain acts in health, pathologists may be allowed to speak positively of its unhealthy states; but not before. When it is proved that the nervous system has no actions which the eye cannot follow, no conditions which are not appreciable by sight or touch, we may expect to find every symptom explained by hardness or softness, by redness or paleness, by hypertrophy or wasting, by extravasation or ulceration; but not before. The assertion of invariable palpable lesion of structure, wherever there is lesion of function, is no more, at present, than the jargon of a school which dreads the admission of anything that is not physical, and, on the ground of this miserable and narrow view, aspires to the dignity of a superior philosophy. The best excuse for the English students who bring it over from Paris, where it is loftily declaimed at the bedsides of the dying, amidst the acclamations of much-delighted pupils, is, that they only think it very fine because they do not comprehend it, whilst it has the advantage of discrediting the existence of many more things which they comprehend quite as little. We are far from ranking Mr. Browne in such a class: he evidently possesses far more experience and

more knowledge. We can only ascribe his adoption of the French pathology to inadvertence.

Mr. Browne notices several interesting facts in his chapter on the Statistics of Insanity. The proportion of the insane to the sane, throughout Europe, is said to be 1 in 1000; in Wales, it is said to be 1 in 800; in Scotland, 1 in 574; but in America, 1 in 262. Of 472 cases given by Esquirol, only 13 arose from excess of study, 100 were ascribed to an excess in some propensity or other, and 90 to ill-regulated sentiments. In 1000 cases cited by Georget, 470 were ascribed to irregular morals, 106 to drunkenness, 20 to an ill-conducted education, and 25 to mental labour. One-half of the cases of insanity, Mr. Browne well observes, arise out of crimes, folly, and ignorance. If we add to this consideration that of the number of crimes which flow from ignorance, we cannot but deduce a very important conclusion from such a review; and if, as there appears much reason to believe, insanity prevails more in this age of intellectual activity and increasing comfort, its prevalence shows that mankind, in their advance towards worldly possessions and personal enjoyments, have not yet learned the highest wisdom, that greatest art of life, the management of the "restless mind." We believe Mr. Browne takes a correct view of this matter when he states that "the professions which are most intimately connected with temporal and selfish interests, and the dispositions which are vicious or lead to vice, are precisely those upon which the infliction falls most heavily." (P. 56.) As regards mental occupation, we do not think he is justified in attributing, as we understand him to do, as disturbing an influence to the exact sciences, so called, as to pursuits which excite the imagination. Led away, we cannot but think, by the desire of seeming very original before the trustees of the Montrose Asylum, he accuses Dr. Conolly of having "made the startling assertion that, among the educated classes of patients admitted into Bicêtre, no instances of insane geometricians, physicians, naturalists, or chemists are to be found, while priests, poets, painters, and musicians occur in great numbers." This he considers to be quite erroneous, and opposed to Esquirol's observations in his private asylum, appropriated to the wealthier and educated classes; whereas the Bicêtre is the asylum of the destitute. In M. Esquirol's asylum, it is observed, there are neither priests nor poets, but two engineers, four physicians, four chemists, &c. &c. But Mr. Browne should rather appeal to the registers of the Bicêtre themselves, for priests and poets are not unfrequently found to be poor. He has also somewhat perverted Dr. Conolly's meaning, which is best shown by his own words in the passage which we take to be the one referred to in Mr. Browne's work.

"The registers of the Bicêtre, for a series of years, show that even when madness affects those who belong to the educated classes, it is chiefly seen in those whose education has been imperfect or irregular, and very rarely indeed in those whose minds have been fully, equally, and systematically exercised. Priests, artists, painters, sculptors, poets, and musicians, whose professions so often appear marked in that register, are often persons of very limited or exclusive education; their faculties have been unequally exercised; they have commonly given themselves up too much to imagination, and have neglected comparison, and have not habitually exercised the judgment. Even of this class, it is to be remembered that it is commonly those of the lowest order of the class, in point of talent, who become thus affected; whilst, of naturalists, physicians, chemists, geometricians, it is said not one instance occurs in these registers." (*Indications, &c.*, p. 92.)

Mr. Browne says this is accounted for by the absence of naturalists, physicians, chemists, and geometricians from the Bicêtre, in consequence of their being in comfortable circumstances; but, in the next page, he himself asserts that poets and priests are more frequently attacked by insanity than either physicians or naturalists, which is nearly all that was asserted in the assertion which he considers so startling; and he ascribes this also to the excitement of the imagination, which was all that was contended for. (P. 58.) He even quotes a table of the professions of 500 patients at Charenton, among whom are only six physicians and two chemists. There are, to be sure, no poets, and only six priests.

In another respect we think Mr. Browne incorrect: he seems to consider the agricultural poor nearly exempt from insanity. "Poverty," he says, "enjoins a compulsory temperance; it shuts out the longings of ambition; it acquaints with the realities of life, and excludes the effects of sentimentalism," &c. &c.; all which is undeniable: but when he goes on to say, that "the agricultural population, which presents poverty in its most attractive forms, and enjoys its best privileges, is to a great degree exempt from insanity," we think he is altogether mistaken. Our own opportunities of acquaintance with poor agricultural labourers, so far from having impressed us with the idea of their poverty exhibiting itself in attractive forms, leads us to consider a large proportion of them as among the most abject and the most wronged of mankind. Toiling all the day, they do not earn sufficient to enjoy decent food, clothing, or bed; and, if their children were not gratuitously clothed and instructed, many of them would be but naked savages. Of the diseases treated in country dispensaries, we have found about two-thirds to be some of the forms of dyspepsia, the product, we believe, of bad food, misery, and care, and exposure to weather. Among such people, we should expect no immunity from insanity, and we find none. Not a village without its mad people. Not a common asylum throughout the country without its complement of wretched labourers and labourers' wives, scarcely in the rank of civilized creatures. Such impressions, we know, are not to be taken in evidence as calculations; but even calculations are not wanting. Dr. Prichard, in his valuable Treatise on Insanity, makes the following observations:

"Some curious facts develop themselves in regard to the comparative frequency of madness and idiotism in different ranks of the community. Of the 14,000 insane calculated to exist in England, or of the 12,547 ascertained, not fewer than 11,000 are paupers, maintained principally at the expense of parishes. A most remarkable difference is found in the proportional number of lunatics in agricultural and in manufacturing districts. Previous to enquiry, we should conjecture that the causes of insanity would have more influence, and the disease be more prevalent, in a manufacturing, than in an agricultural population; but the contrary is the fact. Thus, in twelve counties in England, of which the inhabitants are chiefly employed in agriculture, the entire population being 2,012,979, the insane amount to 2526, giving about 1 lunatic to 820; while in twelve counties where the majority of the inhabitants are otherwise employed, including Cornwall, where a great number are miners, the entire population being 4,493,194, the insane amount to 3910, or nearly as 1 to 1200. In Scotland and in most of the Welsh counties the population is chiefly agricultural, and this may, perhaps, account for the greater proportion of lunatics in the population of those parts of the island." (*Dr. Prichard's Treatise*, p. 334.)

Dr. Prichard thinks it probable that the labouring of women in the

field during pregnancy, may, as suggested by Sir A. Halliday, be one cause of this disproportion; and that hard labour and low diet may have some influence. It is to be observed that Mr. Browne quotes Halliday, Esquirol, and Duncan, in support of his very opposite opinions concerning this question. The chapter on Statistics contains several curious particulars, worthy of perusal, and concludes, not very appropriately, but by a defective arrangement very prevalent in these lectures, with remarks on the employment of lunatics as a means of cure. The remarks themselves are excellent.

In Lecture III. we reach the real commencement of the book, according to its title; the heading of this chapter being *What Asylums were*. And truly a very instructive chapter it is. It should be carefully read by all visiting magistrates, especially, and visiting physicians appointed at quarter-sessions; whose duties are generally ill performed; partly because they do not know their own powers or the exact nature of their office, and partly from local influences. We well know that asylums are not now what they were: but they were what they were for want of superintendence; and to whatever points superintendence cannot reach, human nature being the same, it is always to be feared corruption will yet extend, and negligence. There are mercenary establishments in every county. Nothing is so easy as entrance there; nothing so tardy and difficult as deliverance.

The shocking details given by Mr. Browne are quoted from authentic documents, from the Reports of Parliamentary Committees chiefly; and, bad as they are, we think that worse might be adduced. Their force is somewhat weakened by a kind of challenging and declamatory style, arising out of a redundancy of Scottish energy, and which characterizes Mr. Browne's book; but the facts are sufficiently horrible. The author sums them up as follows:

"Let us pass a few minutes in an asylum as formerly regulated, and from the impression made by so brief a visit, let us judge of the effects which years, or a lifetime spent amid such scenes, was calculated to produce. The building was gloomy, placed in some low confined situation, without windows to the front, every chink barred and grated—a perfect goal. As you enter, the creak of bolts, and the clank of chains are scarcely distinguishable amid the wild chorus of shrieks and sobs which issue from every apartment. The passages are narrow, dark, damp, exhale a noxious effluvia, and are provided with a door at every two or three yards. Your conductor has the head and visage of a Charib; carries, fit accompaniment, a whip and a bunch of keys, and speaks in harsh monosyllables. The first common room you examine, measuring twelve feet long, by seven wide, with a window which does not open, is perhaps for females. Ten of them, with no other covering than a rag round the waist, are chained to the wall, loathsome and hideous; but, when addressed, evidently retaining some of the intelligence, and much of the feeling which in other days ennobled their nature. In shame or sorrow, one of them perhaps utters a cry; a blow which brings the blood from the temple, the tear from the eye, an additional chain, a gag, an indecent or contemptuous expression, produces silence. And if you ask where these creatures sleep, you are led to a kennel eight feet square, with an unglazed air-hole eight inches in diameter; in this, you are told five women sleep. The floor is covered, the walls bedaubed with filth and excrement; no bedding but wet decayed straw is allowed, and the stench is so insupportable, that you turn away and hasten from the scene. Each of the sombre colours of this picture is a fact. And those facts are but a fraction of the evils which have been brought home to asylums as they were." (P. 132.)

In proceeding to speak of *What Asylums are*, which forms the subject of the Fourth lecture, there are some pages of a verbose declamation which should surely have been retrenched before publication. "The cry of the lunatic uttered in the exuberance of his own self-inflicted anguish," and similar passages of oratory, whatever may have been thought of them at Montrose, go far to throw ridicule over Mr. Browne's best intentions. His style is remarkably round-a-bout. Meaning to express that the evils of the past and present condition of the insane may have been exaggerated, he says, "exaggerations have unquestionably crept in, and become amalgamated with the rigid bare truth of many of the statements which have been advanced, both as to the past and present condition of the insane. But, has the apocryphal no parallel in the accredited history? Ignorance—ignorance alike of all that could and of all that did befall within an asylum lent its aid." Again, intending to say that a progressive improvement is taking place in asylums, he says, "universally a decided change is contemplated and desired, and the din of preparation is heard wherever isolation is attempted."

In these, as in many other instances, Mr. Browne's style becomes ineffective by aiming at the forcible; and his lectures acquire a kind of colouring, as if intended to astonish more than to teach. We should not mention this fault, if whole pages were not sometimes disfigured by it, and often much to the obscuring of their sense and meaning. Mr. Browne quotes from our first Number the account of Pinel's glorious emancipation of the enchained lunatics at the Bicêtre in 1792; from which event we believe he correctly dates the beginning of the improved treatment of insane persons which characterizes modern times. This simple and impressive fact he, as usual, somewhat overloads with eloquence. "From darkness," (he exclaims, meaning, we conjecture, medical men and legislators,) "they passed into light,—from savage ferocity into Christian benevolence. These terms are energetic;" &c. The truth is, these terms are not energetic, and that is Mr. Browne's continual mistake.

But of these trifling blemishes we entirely lose sight when we read Mr. Browne's fearless exposure of the still existing evils of asylums.

"There is no classification, no employment, no exercise. If you pass through an establishment, all may be tranquil, orderly, and humane; but the inmates are lethargically slumbering on chairs, or endeavouring to devise occupation by tormenting their fellows, or circumventing their keeper. Men have been proved to have remained seated in the same spot, during the day, for a dozen years, without an attempt being made to rouse their muscular or mental energies. This species of negligence is often presented in a more hideous form. Patients who at first are perfectly able to walk, are allowed to remain in bed; their limbs waste, contract, are partially anchylosed, and they ultimately become unable to rise." (P. 144.)

We know this to be a correct picture, not of the best managed private asylums, but of the worst managed asylums of England in the present day. We know, too, that into such asylums nervous patients may be sent any day, with a regular certificate, or without one; without one properly signed, or with one signed by medical men called in merely to sign it. When once so imprisoned, all appeals for a time are vain. If the visiting physician is written to, he declines to act; if the magistrates, they make a pompous, formal, useless enquiry; if the Lord Chancellor,

he has no control in the case; if the metropolitan commissioners, they order an investigation, which the magistrates can render as insignificant and useless as their first enquiry.

"Where pecculation no longer exists, and where the lunatic is comfortably lodged and sufficiently clothed, there is still great inattention to the mode in which the building where he sleeps is heated. In winter he is compelled to pass, as if in imitation of a Russian bath, from the temperature of a crowded and probably over-heated common-hall, to that of a damp cell which has been cooled down by the indispensable process of ventilation to the freezing point." (P. 144.)

"In 1820, the Commissioners found a patient in a private institution, alone in an out-house, without a fire,—the visit was paid in winter—the windows were broken, probably by his own act. He was without shoes, but was in other respects sufficiently clothed. After much prevarication and deception, it was proved that this patient did not sleep in the apartment said to be his, but in a miserable room up a private stair, concealed by a door, which was discovered with considerable difficulty. It was a single room, small and offensive, containing only a wet and dirty piece of sacking filled with straw, with one rug and a blanket. For this treatment the patient paid 50*l.* per annum." (P. 145.)*

"Coercion is employed unnecessarily. Either from the savage philosophy of terrifying into obedience, and, it is to be presumed, into the possession of reason, or from the despicable economy of employing a small corps of keepers; chains, muffs, manacles, are in many places the substitutes for mildness and prudence, or suitable attendance. Not only the violent and the destructive but the perverse, even the restless and noisy maniac must be secured." (P. 146.)

"It appears that in one instance three keepers were expected to guide, govern, and soothe 250 patients. In another asylum, 164 patients were intrusted to two keepers. In a third, each servant was appointed to take charge of fifty patients. The proportion usually is one keeper for thirty lunatics. This means that one man or woman is to attend to all the wants and wishes, regulate the employments and amusements, counsel, tranquillize, walk and converse with, feed, clothe, and put to bed thirty persons, every one of whom displays a different form of insanity, is furious or fatuous, malicious or melancholy." (P. 147.)

We need not quote any of Mr. Browne's remarks on the inefficiency of the generality of attendants to do all that the peculiar state of many lunatics requires, in all stages of excitement, depression, and convalescence; nor on the imperfect classification of patients. These subjects have often, and powerfully, been brought before the attention of the public. As nothing is more to be expected than that the proprietors of lunatic asylums will accuse Mr. Browne of making exaggerated statements, we regret that he has quoted (p. 161) the erroneous opinion of some physician whom he does not name, that the indecent language used by lunatic females even of the higher classes is learned in asylums. There can be no doubt that it was learned long before, in youth, in childhood, at school, or from servants; and remaining unexpressed in a state of sanity, breaks forth in the violence of morbid excitement. It is, at the same time, a shocking revelation of the female mind, polluted early, and only glossed over by the refinement of artificial life.

Several useful observations are scattered through this lecture, as indeed through all; but whilst some refer to asylums as they are, some refer to them as they were, and some to what they should be. Instances of cruelty under the old system, and observations on the indiscriminate diet

* Report, Pauper Lunatics in Middlesex, p. 158, 1827.

in existing asylums, are mingled together without arrangement, and the lecture concludes with a proposition to make all asylums Public Asylums, which Mr. Browne speaks of as original, although he can scarcely be ignorant of its having been proposed seven years ago by another writer.

The concluding chapter of the work is entitled *What Asylums ought to be*; and is introduced by one of Mr. Browne's strangest sentences. "A perfect asylum may appear to be a Utopia; 'a sight to dream of, not to see.' It would be miserable policy to gratify the ambition of the heart so far, or to pall the keen appetite for doing good by admitting that any attempt had succeeded in placing such retreats in complete accordance with the necessities of the diseased mind." After two readings, one may understand this, and then learn no more than the whole of the preceding lecture was intended to teach us. Mr. Browne then draws, with good discrimination, the moral and intellectual character of a physician fitted to take charge of the insane. He points out the requisite qualities of a site for an asylum, often, he justly says, too much neglected; and insists strongly and properly on more attention being paid than is common to the size and ventilation of the sleeping apartments, and to night-classification, an ample supply of baths, &c. His observations on warming asylums; on the clothing of the patients; on planting and diversifying the airing grounds; on the institution of gardens, a farm, and workshops; are well deserving of attention. He urges the propriety of remunerating the lunatic for his labour, or of letting the remuneration accumulate for him on his release; a suggestion of which the value will be appreciated by all who remember M. Esquirol's observation on the frequent relapses caused in patients by finding, on going out of the asylum, that their affairs have gone to ruin. We would also particularly recommend the observations on the mode of classifying lunatics so that the peculiarities of one may be salutary antagonists to the peculiarities of another; although we can gather that the author's opinion is, what our own assuredly has always been, that, as much as possible, every convalescent lunatic, if not every lunatic, should be kept from lunatic society. His remarks on permitting lunatics to partake, as far as their condition permits, of the consolation of attending the public services of religion, are marked by good sense and good feeling; and we entirely concur in his judicious observations on the artifices of cure which have occasionally been successful with hypochondriacal patients. We should say, with Esquirol, that such artifices should only be a last resource. With respect to secret methods of cure, like all other systematic frauds, they can only be spoken of with abhorrence. Alluding to an account given in the Medical Gazette of a ball at the Salpêtrière in May 1835, Mr. Browne says that something of the kind has taken place every week, for some time past, in the establishment under his care.

The asylum in the neighbourhood of Paris under the care of Drs. Falret and Voisin, that of M. Esquirol at Ivry, and the Retreat at Hartford, U. S. are spoken of, we believe correctly, as combining nearly every possible advantage of such institutions. In the report of the latter, it is stated that of twenty-three recent cases admitted in one year, twenty-one recovered. That at Sonnenstein in Saxony is also referred to, of which an account has been given in Dr. Burrows's Commentaries, and in Mr. Lee's work on the Medical Schools of the Continent. An account of

the asylums of Naples is taken from Willis's *Pencilings by the Way*. Mr. Browne concludes his last lecture, and we shall conclude our notice of his book, by the following idea of an asylum as it ought to be, which we merely abbreviate by the omission of a few lines.

"Conceive a spacious building resembling the palace of a peer, airy, and elevated, and elegant, surrounded by extensive and swelling grounds and gardens. The interior is fitted up with galleries, and workshops, and music-rooms. The sun and the air are allowed to enter at every window; the view of the shrubberies and fields, and groups of labourers, is unobstructed by shutters or bars; all is clean, quiet, and attractive. The inmates all seem to be actuated by the common impulse of enjoyment, all are busy, and delighted by being so. The house and all around appears a hive of industry. When you pass the lodge, it is as if you had entered the precincts of some vast emporium of manufacture; labour is divided, so that it may be easy and well performed, and so apportioned that it may suit the tastes and powers of each labourer. You meet the gardener, the common agriculturist, the mower, the weeder, all intent on their several occupations, and loud in their merriment. The flowers are tended, and trained, and watered by one; the humbler task of preparing the vegetables for the table is committed to another. Some of the inhabitants act as domestic servants, some as artisans, some rise to the rank of overseers. The bakehouse, the laundry, the kitchen, are all well supplied with indefatigable workers. In one part of the edifice are companies of straw-plaiters, basket-makers, knitters, spinners, among the women; in another, weavers, tailors, saddlers, and shoemakers, among the men. For those who are ignorant of these gentle crafts, but are strong and steady, there are loads to carry, water to draw, wood to cut, and for those who are both ignorant and weakly, there is oakum to tease and yarn to wind. There is in this community no compulsion, no chains, no whips, no corporal chastisement, simply because these are proved to be less effectual means of carrying any point than persuasion, emulation, and the desire of obtaining gratification. But there are gradations of employment. You may visit rooms where there are ladies reading, or at the harp or piano, or flowering muslin, or engaged in some of those thousand ornamental productions in which female taste and ingenuity are displayed. You will encounter them going to church or to market, or returning from walking, riding, and driving in the country. You will see them ministering at the bedside of some sick companion. Another wing contains those gentlemen who can engage in intellectual pursuits, or in the amusements and accomplishments of the station to which they belong. The billiard room will, in all probability, present an animated scene. Adjoining apartments are used as news-rooms; the politicians will be there. You will pass those who are fond of reading, drawing, music, scattered through handsome suits of rooms, furnished chastely, but beautifully, and looking down upon such fair and fertile scenes as harmonize with the tranquillity which reigns within, and tend to conjure up images of beauty and serenity in the mind which are akin to happiness. But these persons have pursuits, their time is not wholly occupied in the agreeable trifling of conning a debate, or gaining so many points. One acts as an amanuensis, another is engaged in landscape painting, a third devolves to himself a course of historical reading, and submits to examination on the subject of his studies; a fourth seeks consolation from binding the books which he does not read. In short, all are so busy as to overlook, or all are so contented as to forget their misery.

"Such is a faithful picture of what may be seen in many institutions, and of what might be seen in all, were asylums conducted as they ought to be." (P. 229.)

This description, be it remembered, is not that of a theorist, or of an enthusiast, but of a practical man, long accustomed to the management of lunatics, and at the head of an establishment which we doubt not possesses, under his humane and skilful care, all the advantages which he has described, and one to which he has not alluded, that of having so kind and intelligent a superintendent.

ART. VI.

1. *Handbuch der Physiologie des Menschen, für vorlesungen.* Von DR. JOHANNES MÜLLER, Professor der Anatomie und Physiologie an der Universität in Berlin, &c. &c. Erster Band. Coblenz, 1835.—Zweiten Bandes, 1ste Abtheilung. Coblenz, 1837.
Manual of Human Physiology. By DR. JOHANNES MÜLLER, Professor of Anatomy and Physiology in the University of Berlin, &c. &c. Vol. I. Coblenz, 1835. Royal 8vo. pp. 856.—Vol. II. 1st Part. Coblenz, 1837. Pp. 247.
 2. *Elements of Physiology.* By J. MÜLLER, M.D. &c. *Translated from the German, with Notes,* by WILLIAM BALY, M.R.C.S. &c. *Illustrated with Steel Plates and Wood-Engravings.* Part I.—London, 1837. 8vo. pp. 428.
 3. *Die Erscheinungen und Gesetze des lebenden menschlichen Körpers im gesunden und kranken Zustande.* Von DR. FR. ARNOLD und DR. J. W. ARNOLD, Professoren an der Hochschule in Zurich. Ersten Bandes, 1er Theil. Zurich, 1836. 8vo. pp. 388. — 2er Theil. Zurich, 1837. Pp. 460.—Zweiten Bandes, 1er Theil. Zurich, 1836. Pp. 253.—2er Theil. Zurich, 1837.
The Phenomena and Laws of the living Human Body, in Health and in Disease. By DR. FR. ARNOLD and DR. J. W. ARNOLD, Professors in the Academy at Zurich. 1st Vol. 1st Part. Zurich, 1836. 8vo. pp. 388.—2d Part. Zurich, 1837. Pp. 460.—2d Vol. 1st Part. Zurich, 1836. Pp. 253.—2d Part. Zurich, 1837.
 4. *Rudiments of Physiology, in three Parts.* Part I. *On Organism.* Part II. *On Life as manifested in Irritation.* Part III. *On Life as manifested in Sensation and in Thought.* By JOHN FLETCHER, M.D., F.R.C.S.E., Lecturer on Physiology and on Medical Jurisprudence.—Edinburgh, 1835, 1836, 1837. 8vo. pp. 155, 146, 144.
 5. *Outlines of Human Physiology.* By HERBERT MAYO, F.R.S. &c. *Fourth Edition.*—London, 1837. 8vo. pp. 534.
 6. *Human Physiology.* By ROBLEY DUNGLISON, M.D., Professor of Materia Medica in the University of Maryland, &c. *Second Edition.*—Philadelphia, 1836. Two Vols. 8vo. pp. 546, 566.
- I. THE author of the first of the works before us, though only in the meridian of life, has long achieved a celebrity which extends far beyond the limits of his own country; the just reward of his valuable contribution to every one of those portions of natural science upon which physiology is founded, and from which it derives the materials of further advancement and perfection. His earlier works are preserved principally in the national periodical publications; as the journals of Meckel, Tiedemann, and Ammon. They are papers of singular value and interest on subjects of minute anatomy in all the classes, with the physiological associations, and on the process of development. Whilst still at Bonn, he published, as an independent work, first, his Treatise on the Physiology of Sight, in which he takes up the subjective phenomena of vision, or (if it be now necessary to explain the term,) those changes in

the living body which are intermediate between the refraction of light in the eye and the mental perception. Afterwards he gave to the world another work of surpassing excellence, his treatise *De penitiori structurâ glandularum*. In this great work,—from the injection of the ducts of glands, and of the vessels which supply these ducts,—from the mode in which the glands are first formed by continual subdivision, and, as it were, germination of the duct, (which begins like a cæcal production or tubular growth from the general mucous cavity, and shoots into the plastic matter of the germ; differently indeed in different glands, so as in some cases to form merely tubules with closed cords, in others tubules which expand into vesicles,) he proves beyond the possibility of doubt, with respect to all glands, that, however various the form of the subdivisions may be which the branching ducts assume, yet the object and purpose is the same in all. It is, by means of the convoluted and subdivided ducts, to secure a large surface in a small space for the disposition of capillary vessels. The internal surface of the duct, so disposed, is the secreting surface. The capillary vessels which supply that surface nowhere terminate in the ducts by open mouths. When most minute, the ducts are always still much larger than the vessels which Haller and others supposed were their beginnings; and the peculiar product of any gland is secreted from the internal surface of its ducts in a way exactly analogous to the corresponding function of any portion of the intestinal tube.

These various works, of which the results are now generally adopted in the publications of later systematic writers, have signalized their author as an accomplished anatomist, a practical microscopic observer, a good animal chemist, an honest experimenter, a learned commentator upon the entire literature of each subject he undertakes, a sound and philosophic reasoner.

Qualities so distinguished naturally pointed out the subject of them as destined for the highest situation, in which honours and his peculiar means of usefulness might be combined; and, according to the equitable practice of the Prussian government, whose decisions in the adjudication of medical distinction almost universally respond to the general voice of the scientific medical portion of its public, Müller now occupies at Berlin the chair of the celebrated Rudolphi. Honours, however, do not appear to damp his energies. We still find him unceasingly engaged in his labours; still distinguished by the same ardent zeal and inflexible perseverance; still producing either original works or continuing the works of his predecessors. His *Archiv. für Physiologie* is a continuation of Meckel's Journal; in which, not to mention his numerous original communications, he gives, at the end of each year, a comprehensive account and estimate of the value of the contributions to anatomy and physiology throughout Europe within the period: whilst, as larger original works, published separately, we may instance his anatomy of the *Myxinoïdæ* and the allied species, and his *System of Physiology*.

The last of these works, coming from a person so peculiarly accomplished for the task, has naturally excited a great expectation. We believe that, as far as it has proceeded, (for it is yet unfinished,) it has not disappointed those of the author's countrymen, who were disposed to be satisfied with the completion of what he undertook to perform. But

it is not unreasonable to suppose that this work, if generally known in this country, would excite a different kind and degree of interest from that which has attended it in Germany; for there it is but one of many comprehensive productions which an ardent competition to advance the knowledge of vital processes has brought forth within a limited period; whilst the sobriety of philosophic speculation which its author evinces may scarcely, perhaps, be estimated at its just value by those who are familiar with the dazzle and the delight which poetry or mysticism in the garb of science may afford. Here, on the other hand, where two or three good compendiums of the science serve us for a period of twenty years, and these, however they illustrate the talent and the knowledge of their authors, from their design not full, and from the national taste not speculative,—here, where nevertheless there are countless numbers anxious to participate in the honours of advancing a science which formerly received so much from our countrymen, and still continues to receive not a little,—a work like Müller's would be received as a precious gift, comprising, as it does, all the important facts of the science; reducing them to their just value for the confirmation or the improvement of theory; holding prominently forward the lights which organic chemistry and comparative anatomy and physiology have afforded to the physiology of man; and clearly and constantly directing attention to the nature of those difficulties of processes yet unsolved, and to their bearings upon received opinions.

Since the preceding sentences, and indeed since the whole of the present article was written, Mr. Baly's version of a part of Müller's treatise has been published; and, after having compared a considerable portion of the translation with the original, we are bound to express our unqualified approbation of the manner in which Mr. Baly has executed his task. The translation is not only faithful and accurate, but elegant, and reads throughout (with very few exceptions,) like an original English work. We consider the English student, and indeed the whole profession, under great obligations to Mr. Baly for the very important addition which he has made to the medical literature of this country; and we recommend his work, in the strongest terms, to all our readers who do not possess the original treatise.

Although we regard Müller's work as by far the best of its class, still Germany is rich in productions of this general character, though with individual modifications. Such are the *Physiology of Rudolphi*, the delightful *Biologie* of G. R. Treviranus, the *Physiology of Tiedemann*, and that of Arnold. The first two of the four last named are broken off in the midst; the one by death, the other by the altered purpose of its author, who, finding that, in a long work interrupted by the vicissitudes of stormy times, the rapid growth of the science had rendered the early volumes old before the later could be brought forth, resolved to give the results of his labours in another form and in shorter commentaries. Of the other works, we trust the authors will be spared to complete their laudable undertakings. But, besides those already mentioned, many other systematic works on physiology have issued from the German press within a limited period: works of a loftier pretension and a much wilder flight, not always so well sustained as that more even course within sight of the world's realities to which we have alluded; yet still, in some

instances, uniting the rare powers of high genius with the intelligence and the talent of patient research. We purpose, in the pages which follow, to take a short view of the course which physiological science has run in Germany since Haller's time; which may, in a few words, give some idea of its fortunes in the different schools of that country.

During the latter half of the last century, Haller's doctrines, founded on direct experiment, were sifted by his opponents and extended by his followers. The several modes in which the various organic tissues manifest their vital reactions were carefully investigated, and conclusions similar to those which Haller had established concerning two of these, the muscular and nervous, were extended to all. Each different organ and each different tissue was shown to have its own reactive energy, its own peculiar mode of life. Of these different energies the control and direction was assigned to a higher power in the living body, rather to satisfy, as it seems, the reason of man, which strives after unity of cause, than as the result of abstraction or higher generalization. This controlling power was differently assigned by different teachers. By some it was given to the nervous system; hence the theory of "nervous energy" as the cause of the peculiar "perceptivity" of the different tissues; whilst, according to Blumenbach, the various tissues and organs come into being with their several powers by the "*Nisus formativus*," or "*Bildungstrieb*." Of these two theories we may observe, that the first, instead of ascending in the chain of causation, assigns a partial effect of life, or, in higher creatures, a condition of its manifestations, as the proximate cause of life; whilst the other appears to assign a process for a cause.

Reil, who entertained the same idea of life,—viz. that of many manifestations conspiring to form unity,—revived, with great power of genius, the corpuscular theory of Descartes to explain its proximate cause. This cause is, according to him, the peculiar mixture and form of matter. The powers of organic matter depend upon elective attraction, upon its capacity to assume a special form in virtue of a crystallization which is not merely symmetrical, but (*zweckmassig*) appropriately adapted to an end. "The matter," says he, "which animal substances attract from without is attracted according to a determinate law, in which is contained the reason of its peculiar form. So the germ of a crystal of common salt attracts the component part which may be yet wanting to complete the perfect cube by a determinate law, in which lies the cause of that peculiar form." There are many passages in Reil's work which show him to have been no bigoted adherent to his own theory, to have been aware of its difficulties,—nay, of its imperfections. "We cannot define," says he, "what the power is which characterizes the matter of animals and plants, until chemistry is able to tell us more certainly what are the primary substances of which they are composed;"* and, as if uncertain of his own principle, he asks "are the phenomena of life the resultant of the sum of all the properties which are met with in organic matter? or, is there some single matter on which alone the phenomena depend?"† "To me it appears not improbable that, besides the matter which we perceive with our senses and can treat chemically, there may be yet some finer substance in animal bodies, of an unknown nature, which, being

* Reil's *Archiv. für die Physiologie*, vol. i. p. 48.

† Ibid. p. 30.

commixed with that which is visible, may render it complete,—may, according to the measure and the mode of the addition, ennoble that which is coarse, and may render it capable of acting as the sufficient cause of animal phenomena, in all their determinate modifications.” Thus does Reil elude the very principle of his system. In what, we may ask, does his “finer matter” differ from any imaginary Archæus? In explaining the facts of any one particular science, there seems no philosophy in determining to borrow from some other science, whose facts are not strictly analogous, the causes upon the assumption of which that other has been constructed. More especially is this indefensible when that other science, so far from being complete, is still obliged to have recourse to hypotheses to combine its phenomena. If that, which is a legitimate end and object of science, shall ever be realized,—the reduction of organic force to those general forces which animate and rule the universe,—it will then, as we believe, appear, not that ordinary physical or chemical powers characterize the class, but that limiting conditions have come into view, and are clearly recognized and accurately defined, and that, under the operation of these, the higher power is restricted to specific exhibitions of energy so very inferior. What these conditions may be has not yet been even guessed at.

But, though his theory was imperfect, as we have seen, yet the genius and talent evinced by Reil, in contributing to establish by reason and by induction the important truth that peculiar organic matter and peculiar vital properties are always in strict relation to each other, must rank him amongst those who have signally contributed to the advancement of physiological science. He constantly insists, as indeed was essential to his system, that organic power is only seen in connexion with organic matter. Irritability is not something infused into muscle, nor sensibility something superadded to nerve. In virtue of the power, whatever it may be, which brought these forms of matter into existence, these forms have, with their existence, their properties also. Matter and power are only known in mutual connexion; the existence of matter is action, its peculiar existence its peculiar mode of action. Every organ, therefore, bears within itself the cause of its own phenomena; by means of its own powers it is nourished, grows, performs its office in the animal economy; and, though its connexion with the rest of the body, as part of a whole, must necessarily be maintained, as a condition of the maintenance of its powers, yet this connexion is but a condition, and not a cause of those powers. “The animal body,” says Reil,* “may be considered as a great republic consisting of many parts, each part indeed preserving a certain relation to all the others, and each contributing to the preservation of the whole; yet each part acts by its own peculiar powers, and possesses its own peculiar perfections.” Muscular fibre is irritable because it is muscular fibre, not because it derives power from the nerves; though connexion with the nerves be one of the conditions of the maintenance of that power. In this same spirit, when speaking of the growth of bone, Müller expresses himself as follows:

“It is entirely erroneous to suppose that one organized part is the organ of nutrition for another organized part; as, for instance, that the substance of bone is

* Archiv. i. 105.

formed by the periosteum, the bone nourished by the periosteum. The substance of bone, inasmuch as it is organized, must itself assimilate. Unorganized parts alone, those which have no vessels, as hair, nails, teeth, the crystalline lens, are produced by an organized matrix and maintained by the apposition of new matter. I consider the opinion that bony matter is produced by the periosteal membrane as a piece of barbarism unworthy the present state of physiology." (*Phys. i.*, 361.)

Many of the leaders of scientific research who succeeded Haller, were so closely engaged in establishing the nature and the varieties of vital power as exhibited in the different organs and tissues, that they seem to have paid but little attention comparatively to the circumstances which are indispensable conditions of its exhibition.* Men were recalled however from this exclusive view of the subject, by a theory (if it deserves the name) more partial and more exclusive than any which had yet been heard of,—but having another direction. Brown's "theory of excitement," to which we allude, was far more favorably received in Germany than elsewhere; for there it effected a signal revolution in medicine. Its adoption as a medical theory has been accounted for by the fact that on its first announcement the doctrines of Galen had not been entirely expelled from the country; and because a system remarkable, like that of Brown, for its simplicity and its slender requirements of labour or of learning, must needs recommend itself by contrast to many who were weary of the cumbrous apparatus of Galen, his humoral theory with its primary and secondary qualities. To the faults of Brown's system it is scarcely now worth while to allude: its occult quality "excitability" whose nature and laws are not even hinted at, its neglect of all distinction between the phenomena of life as apparent in the different systems, its reference of the blood to the class of mere stimuli, as if the animal fluids were not vital, its attributing the same value and effect to every stimulus, every one of them being assigned as a cause through whose efficacy the excitability, or vital power, is used up, whilst there is no cause assigned by which its losses may be repaired. These capital defects, with the theory itself, may be forgotten. The good which arose from its temporary adoption by numbers has been permanent. It recalled attention to the external conditions of life, to the effect of warmth, of air, of blood, of the secretions, of food, upon its manifestations. It was seen more clearly that unless such conditions be fitly ensured, however exact may be the form and composition of parts which organic power has brought into being, organic action is suppressed. "This inactive state," says Müller, "of the organic powers subsisting in the fecundated germ of the egg which has not been brooded, or in the seed of a plant before it has sprouted, must be distinguished from death. Yet it is not life, but specific capacity of life. Life itself, the manifestation of organic power, begins under the action of certain conditions,"† and can be maintained only whilst they are present.

The presence, then, of these necessary conditions of life is attended with those changes of the material composition of organized bodies, which are the essence of vital phenomena. The conditions themselves are, in

* This does not apply to Blumenbach, nor to Reil: they were amongst the first who recalled attention to the conditions of life.

† *Phys. i.*, 28.

fact, the supply of new matter which combines with the various organs whilst some of their previous components are removed.

"The warmth, the air, the food, the animal juices necessary for the conversion of that food into blood, the blood itself, the imponderable matter which seems to be supplied to parts by nerves: all these things are the necessary materials for the production of those successive changes in organic matter which are the consequence and the continuation of life. Though some of these conditions be but ordinary forms of matter, binary combinations, yet their effects on the living organism are seen in very different productions. These are never binary combinations: such are only observable in substances which are undergoing separation, or have been separated, for removal. The carbonic acid, a binary compound, escapes from the blood in respiration; whilst the effect of the oxygen which in part unites with the blood is seen on its becoming engaged in those multiple combinations which are peculiar to that fluid."

"The vital stimuli must not be confounded with other stimuli which, though they have an effect upon the living body, do not add to its power, contribute nothing to the formation of organic matter. All that stimulates, all that produces change in the composition of living tissues is not therefore vital stimulus. A mechanical stimulus may modify the condition of a sensitive membrane, may call forth a manifestation of life, viz. sensation; yet it does not vivify, it gives no augmentation of organic force. But food is not merely a stimulus of organic tissues; it is itself capable of life: it is a stimulus which vitalizes and becomes itself vital. A healthy man may bear to be deprived of it scarcely more than a week: animals which stand high in the scale, not many weeks: amphibia, on the contrary, especially serpents and tortoises, much longer. Water, whether it enter into organic combinations, as water, or separated into its elements, is also required in its uncombined state for the manifestation of life: for animal parts which are not softened by water are not capable of life. Atmospheric air is so indispensably necessary for vital phenomena, that they cannot be maintained for many instants in the higher animals without respiration, without those changes of the blood connected with respiration, and without the influence of blood thus changed upon the organs. Privation of food may be borne for some considerable time: in amphibia, for instance, the blood may receive no new nutrient particles to present to the organs for a very long time. But those other changes which the blood brings about in the organs in virtue of the respiratory process cannot be dispensed with long even by them: by men only for a few seconds. Warmth also, especially important in the beginning of life, when the body is not in a condition to produce any, and in a general way indispensable for all organic things, whether plants or animals, would seem to enter into the composition of organic beings. For organic processes require a determinate temperature in every animal and every plant; and we know that in the binary combinations of ordinary chemistry, which cannot be effected without a certain temperature, a determinate quantity of heat is absorbed. Under the influence then of these conditions, food, water, air, warmth, organic being is spontaneously developed from its germ; the organic matter which is already present is constantly decomposed, and the vital phenomena themselves are phenomena of increasing combination of new matter and decomposition of old, or, in other words, of change in organic matter. Whether electricity be necessary for the development of life, is as yet quite undetermined." (*Müller*, p. 30-31.)

The degree in which life is dependent upon these different vital stimuli, comparatively, has been investigated by Spalanzani, by Schröder van der Kolk, by Le Gallois, by Edwards, and others.

Thus have we seen that the investigation of the vital properties of tissue led the followers of Haller to conclude that there are in the same body different vital powers, and that they imagined some paramount force besides, capable of controlling them all. The examination of the conditions of life corrected what was illogical in this conclusion. For out of the examination, extended to all periods of the life of plants and

animals, arose a knowledge of the process of development. This knowledge, though not perfect, is sufficiently complete to prove that the same essential power, which is connected with the matter of the germ at the time of its production by the parent plant or animal, evolves from each individual germ, by the conversion of matter which is present with the conditions of life, all those organs which belong to the purposes of its future existence. The germ is *potentially* that which the future body becomes *actually*. The vital energies of the organs are the development of the essential power of the germ.

We now turn to that which is more particularly understood as the German school of physiology, when views are alluded to which have been cherished to a greater extent, or at least more generally, in that country than in others. Germany has always, since she had a literature, been the nursing mother of philosophy; and philosophy has reacted there, as elsewhere, upon every branch of science. For what is philosophy in its true sense, the sense in which Bacon understood the word, but the science which endeavours to ascertain the principles of human knowledge, the *scientia scientiarum*? The system, therefore, from which these principles are sought must inevitably give the character and direction to the pursuit of every particular branch of science. The pursuit will be partial and the attainment unsatisfactory, until that first philosophy has established infallibly its own principles. Such systems being founded, all of them, in the nature of man's intellect, will be more purely speculative or more purely practical, according as the greater authority is attributed to his thinking or his active powers, his reason or his senses. But the true system must have both these characters, for the mind must exercise all its powers. Hence the great results in every science have been obtained by men who have laboured in this spirit. They are the genuine eclectics, who having gained their facts from experience, have attained to principles by meditation properly applied to facts, and have soberly and laboriously verified or corrected their principles by new practical applications of them. The earliest schools of philosophy in Greece, the Italic and the Ionic, took the different directions above alluded to. Pythagoras, the founder of the Italian school, sought in the reason of things, in the harmony of the universe, the cause of their existence. Thales, the founder of the Ionian school, sought it in the things themselves. The opinions of Pythagoras were expanded, by the noble conceptions of Plato, into the doctrines of the first academy; those of Thales by Aristotle, into the doctrines of the Péripatetic school. Plato asserted the doctrine of innate ideas, of universal notions, or forms of knowledge, impressed upon the mind of man by the supreme author of all things, of which the prototypes are in him, and appealed to them as the only source of all our knowledge of what is true, or good, or lovely; the fountain, from which must be drawn all certain principles of science, of morals, and of art. Aristotle descended from this lofty ground: he rejected innate ideas; the eternity of matter became a dogma of his school; and whilst all knowledge was declared to consist in the knowledge of causation alone, the appeal was made to the senses. To one or other of these classes of opinion, to that where reason or that where experience holds the highest place, may be traced most of the controversies which have since agitated philosophers. After the revival of letters, Descartes and Leibnitz sup-

ported the doctrine of innate ideas, which was rejected by Bacon and by Locke. But whilst Locke repudiated the doctrine of Descartes, he was far from enjoining such a reliance upon experience as admits nothing beyond sense. He did not deny that "the mind has some intellectual capacities, some powers accompanied with various ultimate laws of belief, for which no experience can account."* Locke's system was misinterpreted by Condillac, and was for a time received from him by his countrymen, in a form which led to the grossest materialism. According to Condillac, ideas are only "sensations transformed;" the faculty of feeling comprises all the other faculties of the mind. He sets up his human statue, allows it every mode of sensation, thus supplies it with notions of all external things; but forgets that reason and conscience are not thus imparted. The effect on physiological disquisition was soon felt. For, not to speak of De la Mettrie and the author of the *Système de la Nature*, if such men as Cabanis could assert it to be "too obvious to require proof that physical sensibility is the source of all the ideas and of all the customs which constitute the moral definition of man," need we wonder at the revolting absurdity of St. Lambert, who proposed, as the formal definition of man, "an organized sensible mass that receives intelligence from all which surrounds it, and from its wants?" Leibnitz, on the other hand, modified the doctrine of innate ideas, so as, in part, to meet the objections of Locke; and after him the more ideal form of philosophy has passed through various stages of development in Germany, in the periods of Wolf, of Kant, of Fichte, of Schelling and his successors; and is still passing on, we trust, towards that adult and perfect form when it may be able to hold in its grasp, to embody and to vitalize all the results of experience and analysis, whether directed to matter or to mind.

The critical philosophy of Kant is an enquiry into the nature and the limits of the human faculties. Hume's scepticism was one cause of the enquiry, Berkeley's idealism another. If philosophy be a possible science, it must be able to reconcile such opposite results, and to shew in what they are opposed. What we perceive by our senses are not the things themselves; Kant endeavours to show that man is able *à priori* to deduce those laws of his reason in virtue of which nature stands related to him, and has for him a reality. He distinguishes between reason and understanding—the one transcendental, its conclusions necessary and general (like those of geometry); the other, judging by the senses, experimental, precarious. Ideas are the causal chain of the one, experience of the other. He applied his principles to organic bodies, gave his celebrated definition of an organism, and shewed that in each definite organism the final cause is the idea through which it is a part of the universal organism. The dark language of Kant's school was introduced into medicine and physical science by neophytes who seemed, with few exceptions, scarcely to know the sense in which the master had used it. The firm ground of experience and all the holds of practical knowledge appeared to be relinquished for the wild ocean of *a priori* dreams and distorted speculations.

Of the opinions of Fichte we need say little: he drew more deeply

* Dug. Stewart's *Philosophical Essays*.

than Kant had done the line of demarcation between the objects of perception and the perceiving mind, between object and subject. His principles have been criticised as being mere logical laws; forms of thought without substance; and therefore incapable of affording any knowledge of what either subject or object is in itself.

Schelling, as well as Fichte, proceeded from the critical school. His philosophy has had a general and a remarkable effect, having extended itself widely amongst jurists, philologists, physicians. His vivid imagination, his extensive acquaintance with nature, his great knowledge of the history and the philosophy of antiquity, enabled him, in appearance, to supply the deficiency which had been noted in Kant's system, to discover the uniting link, the common principle of its theoretical and its practical part. It is not the place here to enter into the particulars of Schelling's argument. He attempts the solution of the same problem, how it should come to pass that an external world exists for us: but far from separating subject and object, as Fichte had done, he shews that they are necessarily united. From consideration of the powers of nature and the powers of mind, he concludes that the laws of nature must be capable of proof immediately in consciousness as laws of consciousness, and conversely the laws of consciousness as laws of nature. Hence nature and consciousness are identified in the *absolute*, the infinite mind. The essence of the infinite is the absolute identity of subject and object. Schelling opposes absolute knowledge by means of *ideas*, (that intellectual intuition in which subject and object are one,) to the lower knowledge obtained by judgment through the senses. It seeks to know the essence and the form of things by ideas of reason, and considers being and knowing identical. It is transcendental idealism, and deduces all knowledge, not from nature and not from reason, but from that which identifies the two—the godhead,—and seeks to show the constant parallelism of mind and of nature.*

The chief propositions of the system, as applied to nature, are: that there is only one identical essence; that in respect of this there is only a quantitative difference; that the One Absolute reveals himself in the eternal production of finite things which are forms of his essence; that nature is not dead, but living, divine; that science follows this revelation as steps in the development of the absolute essence, and is an image of the universe whilst she unfolds the ideas of things.

It is no part of our business to show how arbitrary are some of the principles upon which this philosophy is founded, nor to point out its character of pantheism, of fatalism, of necessity. That has been done by others, and attempts have been made to correct these defects, whilst preserving its principle of identity. In fact, but for the marked influence which it has had upon physiology, and that a favorable one, we should not have alluded to it at all, but should have left it to the critics of cosmogonies in general, with those of Hesiod and Dante, and others. But since we hope to bring before our readers, as occasion may serve, several distinguished works by disciples of this school which presuppose a general knowledge of its tenets, and use its language, we have thought it convenient and right once for all to state what it is. When applied to nature

* Tennemann's *Grundriss der Geschichte der Philosophie*; p. 411, et seq.

in general, it is the theory of development in the largest sense: and, when properly understood, asserts the necessity of the closest observation of every part of the process, as preliminary to that intellectual intuition of the idea which connects the whole. As applied to living bodies, to particular living bodies, to particular organs, it is still the same. It enjoins the closest, the calmest, the clearest observance of process, all for the same end, to gain the ruling idea. The process is still found to be essentially the same: evolution of the absolute, conversion of the identical (in difference) into those opposites (forces) which are the visible qualities of things.*

Steffens applied to inorganic nature, and Doellinger and Oken to organic, the speculations which Schelling had himself chiefly illustrated by chemistry.

In connexion with this part of our subject we shall introduce a pleasing passage from one of a course of popular lectures on Psychology, delivered by Carus at Vienna. It is an exposition of the metamorphoses of plants: of the *idea* received by botanists from Goethe: and may serve two purposes: one to explain, by an instance, something of the language of Schelling's school; the other to shew in what light comparative anatomy, or morphology, is considered by it.

"Ordinarily, it is thought that we begin early enough if we trace the history of a plant from the seed. But the fact is here lost sight of that the seed is already a membered whole, and very far removed from that simplicity which gives the primordial phenomenon. That simplest form is to be sought in the first tenderest germ of the seed itself, within the flower of the parent, amongst the higher plants. Here in all respects the appearance announces itself as most simple; it presents itself in the most *indifferent* form, as globule; in the most *indifferent* consistence, as fluid; composed of the most *indifferent* element, as water. In a word, whether we contemplate a plant as the oak of a hundred years, or as the towering shadowy palm, or as a slender grass, or as the simplest thread of conferva, its first most simple form was a microscopic drop of fluid. This *unity* being given, as vital activity proceeds an internal membering of this drop arises, we observe how first the *opposition* between external and internal forms, husk and kernel, how in the kernel itself the opposition between mass that *affords* and mass that *receives nutriment* announces the future vegetable form; and so the seed, at length matured, is excluded from the parent body." . . .
 "Should now those circumstances occur under which its slumbering life must awake to new activity, then begins a new and very remarkable series of internal membering and parting. The germ from which the young plant is to proceed parts into plumule and root; the husks of the seed burst asunder, the nutrient mass heaped within forms and unfolds itself into the first compact root, or germ-leaves (cotyledons) of the young plant; and in the same exact proportion it shoots upwards joyously to the *light* by its green stem, and downwards into the *darkness* of the earth for nutrient juices by its root. But new divisions are still in progress: each new bud repeats in metaphor the seed, and the future metamorphoses are only distinguished from that original one in this, that the character of the latter becomes more delicate in them. Already the upper stalk-leaves assume a softer form, often variously divided, often, towards the flowering, discarding their simple green and dressed in various colours. In the calyx the form of the leaves is still further changed (sepals), they have a higher meaning, they enclose the flowers; until at length having reached their highest stage they unfold

* Even those who most resolutely resisted all connexion with the philosophical school, as Rudolphi and Tiedemann, conducted their labours in the spirit which it inculcated; witness their rich contributions to comparative anatomy, and the extensive research which they carry throughout animated nature when considering any physiological question. The age of rude empiricism has passed, as far as Germany is concerned.

into *opposites* in the petals and stamens. But this separation of petals and stamens in the flower is not enough: the relation of sex is indicated in the stamens themselves; and in the midst of opposites, thus carried to the highest degree yet still included in unity, comes forth the germ of a new seed. So that where one ring of vegetable life is completed a new ring begins, and links itself to the one chain which is extended through all vegetable life.

"From this example we learn the nature of an organic whole, its development from within outwards. We learn further from this, the 'genetic,' mode of considering nature, how many apparently different parts of plants are in fact only metamorphoses of one and the same fundamental form. And whilst we perceive that the transformations may be retarded or may be precipitated or may revert, the idea of unity in variety is but the more strongly infixed in us. We see, for instance, if it be the rule that leaves transform themselves into sepals, sepals into petals, petals into stamens: stamens may return again to petals, as in double flowers, as deviations from the rule: or petals to sepals, as on the points of the petals of the rose new sepals are frequently seen to shoot."*

Thus, however various may be the external appearance of a plant, the simple relation of stalk to leaf is intrinsically the whole plant. It can but metamorphose the same parts, and all the metamorphoses occur in succession in the same individual, being not transformations of organs but of the plant itself, viz. stalk and leaf. Hence there can be, as Müller has elsewhere observed,† no comparative anatomy of plants. The anatomy of one plant exhibits the transformations of the whole vegetable kingdom. In animals, on the contrary, the metamorphoses whether of body or of organs do not occur in the same individual. They are found in permanent forms scattered throughout the whole animal kingdom. Comparative anatomy, in determining these differences of form and of organs, does not rest satisfied with the result of its comparisons, which are so far only an aggregation of isolated facts. It endeavours to know the *process* of change; it endeavours, by looking through the animal kingdom, to gain the idea which directs the process, and so, intellectually, to be able to repeat it.

But to return from this digression. It may be said, and has been said, that the philosophy of nature commenced in fanciful dreams and arbitrary assumptions. And certain it is that it has failed to realize its magnificent anticipations. But the physiology which was connected with it did not always continue to soar in those lofty regions of unsubstantial speculation, from which it was directed to take its first flight. By an analysis of facts, it enquired into that mutual and intimate connexion of all things which had been asserted on *a priori* reasoning; the foundations of the science were thus largely extended. It investigated the relations which had been asserted to exist amongst living things; a higher zoology was the result, together with most valuable contributions to comparative anatomy. It fully tested the fundamental philosophical principle of unity in the midst of diversity; the history of development of the animal kingdom in general, of separate animals, of separate organs, was in consequence, so greatly enriched in substance and improved in form that a large share of the merit of its production appears to be due to the Germans. That was not merely a fanciful system to which such men as Doellinger, Meckel, Oken, Carus adhered. In their hands physiology

* Carus, *Vorlesungen über Psychologie*; p. 15.

† *Vergleichende Physiologie des Gesichtsinnes*; p. 28, 29.

assumed a determinate tendency, and its huge mass of facts became animated by a connecting idea.

Seeing that such have been the results obtained in Germany, we naturally ask—what is in fact the peculiarity of their system? and, if it be peculiar, in what respect it differs from those views which conducted Harvey, and Cuvier, and Bell, to their great discoveries? We do this the more willingly, because, though strongly averse to those crude hypotheses which are often advanced under the name of theory, we are not disposed to agree to the proposition so emphatically asserted by many; that physiology, being a physical science, is to be advanced by experiment and observation alone. It may safely be denied, we believe, that this is true of any physical science. Certainly it is not true of astronomy, for instance, the only perfect one of them all. For if it be asserted that this science is founded solely in the ascertained facts of mechanics and of the observation of the heavens, we may ask how it happened that the first law of motion was ever believed to be true, when it is found to be contradicted by every appeal that can be made to experience. We may ask, if observation be all in all, how it happened that Kepler, who knew the true motions of the planets, and announced those general laws respecting them which are, in fact, all that is necessary in order to determine the proximate cause of those motions, how he yet failed to assign a true cause? So far was he from the mark, that he believed the planets to be urged in their elliptic curves by forces which act in the direction of those curves. Yet Newton arrived at the true cause, and asserted its law by a series of inductions which were founded on Kepler's results alone. He arrived at it, however, by doing something which Kepler omitted: in German phrase, he discovered the "idea," the idea appropriate to those valuable facts which Kepler had ascertained after labours and disappointments innumerable, the idea which enabled him to produce them all in thought. He saw the principles included in the facts. *If* equal areas be described, according to Kepler's ascertained law, in equal times, then Newton saw that the body *must* be drawn towards the centre. *If* the orbit be an ellipse, then Newton saw that the force, drawing towards the focus, *must* diminish as the squares of the distances increase. *If* the squares of the periodic times of the planets be, as Kepler shewed, proportional to the cubes of their mean distances from the sun in the focus of all their elliptic orbits, then Newton saw that these bodies *must* gravitate towards the sun in the ratio of their masses directly, and the squares of their mean distances inversely. The assumptions are all matters of observation; and of these assumptions Newton's principle is the necessary result. But it required the mind of Newton to discover the ideas which suited the facts of the case, and to connect the two by evidence of the clearest reason.*

It is plain that, in this case, the most accurate observation of facts, the comparison of them in every possible relation did not of themselves lead to theory; to the comprehensive principle which, being once clearly seen, is capable of being expanded into all the details of the phenomena, and of confirming its own truth by shewing that phenomena which at first sight appeared subversive of the principle, are in fact its necessary con-

* La Place, Exposition du Systeme du Monde; p. 428.

sequences, "regular then most when most irregular they seem." The history of this science instructs us further that, even where general laws have been stated in their simplest terms, labours and difficulties different in kind but scarcely less formidable, yet remain: viz. the inductions of verification. With these difficulties astronomy has maintained a victorious struggle from Newton's time to the present. But what, let us ask, might be expected to be the result of such a struggle with respect to other sciences whose facts are more complicated? If in chemistry, if in physiology, the law of molecular motions apparently so anomalous were assigned, can we hope that we should be able to effect the useful and practical application of it, the deduction of all the particulars from the theory?

Whilst, therefore, it cannot be doubted that laws exist by whose operation organized bodies with their several members are evolved, for constancy implies law; yet, since they have not been discovered, equivalents or substitutes have been sought for them.

Such equivalents are found in observation of the results of these laws; or, of the processes which they are actually effecting; in observation directed to ascertain not the connexion of *cause* and *effect*, but of *means* and *end*.

"In considering the intellectual processes by which Harvey's discoveries were made," says Mr. Whewell in his admirable work just published, "it is impossible not to notice that the recognition of a creative purpose, which, as we have said, appears in all sound physiological reasonings, prevails eminently here. 'I remember,' says Boyle, 'that when I asked our famous Harvey what were the things that induced him to think of a circulation of the blood, he answered me, that when he took notice that the valves in the veins of so many parts of the body were so placed, that they gave a free passage to the blood towards the heart, but opposed the passage of the venal blood the contrary way; he was incited to imagine that so provident a cause as nature had not placed so many valves without design; and no design seemed more probable than that the blood should be sent through the arteries, and return through the veins, whose valves did not oppose its course that way.' We may notice, that this discovery implied the usual conditions, distinct general notions, careful observation of many facts, and the mental act of bringing together these elements of truth. Harvey must have possessed clear views of the motions and pressures of a fluid circulating in ramifying tubes, to enable him to see how the position of valves, the pulsation of the heart, the effects of ligatures, of bleeding, and of other circumstances, ought to manifest themselves in order to confirm his view. That he referred to a multiplied and varied experience for the evidence that it was confirmed, we have already said. Like all the best philosophers of his time, he insists rigidly upon the necessity of such experience. In every science, he says, be it what it will, a diligent observation is requisite, and sense itself must be frequently consulted. We must not rely upon other men's experience, but our own, without which no man is a proper disciple of any part of natural knowledge. And by publishing his experiments, he trusts, (he adds,) that he has enabled his reader to be an equitable umpire between Aristotle and Galen; or rather (he might have said,) to see how, in the promotion of science, sense and reason, observation and invention, have a mutual need of each other." (Vol. iii., p. 401-2.)

Again, in Cuvier's theory of the "conditions of existence," there is found such a substitute as that above alluded to. This theory we are glad to instance, because it embodies many of the views which have been considered peculiar to the philosophical school. Though the law of organic formation cannot be stated, yet it is found to be a result of the law not only that certain organs coexist, but that any modification of one of them

necessitates corresponding modifications in all the others. "Animals with hoofs must be herbivorous, they have no means of seizing their prey. Having no other use for their anterior extremities than that of support, their shoulders require a less vigorous organization; hence the absence of the clavicle and of the acromion, and the narrowness of the scapula. Not requiring to perform rotation of the forearm, their radius will be united to the ulna, or, at least, articulated by ginglymus, not by arthrodia, to the humerus, &c." These views seem tolerably evident when once stated. Yet to embody them in a theory it required two things: one, to know the general character of all the groups and the details which modify their anatomy throughout the whole animal kingdom; the other, to gain the idea which subordinates these.* That idea is teleological: the organs are seen to have a purpose; it is seen that they conspire, in their several purposes, to effect a determinate useful result; it is seen that the various organs are associated for those ends by which the animal fulfils the purpose of its being. When this idea is verified in the details, it is found that there is *not merely a general but a specific consistence in the form and combination of organs; there is found to be a constant harmony between organs apparently the most remote; the altered form of one is invariably attended with a corresponding alteration in the others*; a fragment of bone, a process or a surface has a determinate character; an articular surface may indicate, to those who can read the enigma, the entire form and habits of the creature to which it belonged.†

By means like these connecting bands have been discovered between the limited intellect of man and the complicated results of vital forces, and which have proved their value almost as effectually as if, having assigned the highest physical cause, he was able to deduce those results as its necessary consequence. Neither, as has been seen, are the views suggested by the consideration of final causes, and those peculiar to the philosophical school of physiology opposed to each other like the poles of a magnet. On the contrary, they run parallel. But the ken of the former does not reach so far, neither does it penetrate so deeply beneath the surface. The teleological school, (say the philosophers,) is satisfied with partial views, with a certain seeming fitness on the surface of things; with evidence of artist-like design, with the belief that one thing is made for another, or for its own preservation. There is, however, (they add,) not merely this external fitness, but also an inward essential connexion of all things, pervading all and uniting all, both internally and externally. Nature is a system of connected parts; an admirable unity of innumerable members; the collection of these forms the organism which comprises them all. Man himself is such a member; an isolated member he is apt to think himself; but, in fact, a member whose existence is bound up with that of all the others in a totality in which all are included.‡

We now leave these considerations: in fact, as far as Müller's work is concerned, we are not required to recur to them. For it is practical: it is conducted in the true spirit of "thinking experience."

"I have made it, in all instances, my object to investigate the difficulties of the

* Whewell's History of Inductive Science; vol. iii., 476. Compare.

† Cuvier; Ossements Fossiles; vol. i., 49.

‡ These statements are collected from Valentin's new work, "Handbuch der Entwickelungs Geschichte der Menschen."

subject, and to analyze the more important facts in such a way as to lead to the solution of physiological problems; even the more abstruse, if our general knowledge of the principle of life were more complete. We deceive not ourselves with general formulæ of life, and so forth. Still the exact natural philosopher, who does not employ the term 'life' for every species of activity, is not so blind as not to perceive that every where in nature there is action. This every one knows. Activity is everywhere: even the rest of material particles is effected by their mutual attractions. Had custom originally applied the term 'life' to the activity of the system of the universe, I should have used it in this sense; but custom has limited it to the activity of organic beings. He who chooses to call the heavenly bodies 'organisms' may do so; in my youth I thought it quite right to do so. The consideration, however, that the differences of these extraordinary and ordinary organisms are greater than their resemblances, determined me to drop a designation so agreeable to some enquirers into nature." (*Müller, Pref. to Part II. Vol. I.*)

II. The second work announced at the head of this article, comprising the Physiology of Man in health and in disease, is executed by two brothers, professors at Zurich; the Physiology of Health by Dr. F. ARNOLD, well known for many works, particularly by his excellent treatise "*Der Kopftheil des Vegetativen Nerven-Systems*," which he published when a teacher at Heidelberg; the Physiology of Disease, by Dr. J. W. ARNOLD. It is with the former portion only of this work that we are at present concerned. The author premises a view of general Physiology, as introductory to the exposition of the special vital processes. In this he expounds not only the general phenomena and laws of the living body, but also its relations to external nature, and those conditions of its existence which are founded in its organization. Such a prefatory work he considers to be indispensable, if an author would avoid the inconveniences which appear inseparable from the plan of those who postpone the general physiology to the special; such, for instance, as the necessity of continually assuming as known that which is afterwards to be taught, or of inverting the nature, order, and connexion of vital processes; as is the case when the doctrine of the formation and motion of the blood precedes that of the chyle, when the doctrine of digestion follows that of nutrition and secretion, &c. In the general physiology the fluids, the tissues, the systems of the body, their properties and composition, are considered by our author, as far as this is necessary for the full comprehension of the special processes of life. He, moreover, in this part of his work, compares the organization, the corporeal and the mental faculties of man and animals, and of different races of men, since no aspect of human nature ought to be neglected by the physiologist. To this mode of treating the subject, it may perhaps be objected, that it forces into physiology many subjects which belong more properly to the province of natural history on the one hand, and of minute anatomy on the other. They who thus object will probably prefer the more concise and very elegant prolegomena of Müller, as sufficiently preparatory for a well educated student of medicine, whilst the special details are not so extensively anticipated.

Arnold, with the greater part of modern physiologists, considers life as the "aggregate of the phenomena presented by those natural bodies which have within themselves, under a certain form of being, the proximate cause of those phenomena; and which indicate, in all their processes, self-subsistence and fitness for an end." "These bodies," he says, "are

called *living*, or *organic*, which are not merely subject to the ordinary laws of nature, but in which a special force operates, which in this respect is called *vital force*. Dead, or inorganic bodies, on the other hand, are all those whose manifestations and changes may be referred to the ordinary natural agents, called physical forces." "The form in which the life of an individual manifests itself is called 'organism:' its peculiar constitution 'organization.' The organism is a whole, consisting of parts necessarily and intimately bound together, and acting towards an end, viz. for the purpose of its own perfection and conservation." Again, "organic bodies exist not merely by and through themselves, but their life also depends upon a constant mutual action between them and the external world. For from it they incessantly receive matters, and to it they return matters, and stand in a reciprocal relation to the various forces of the universe which is indispensable to their continuance." (*Introduction*, § 2, 3, 4.)

The author's general enunciation of his opinions respecting the nature and properties of living bodies we present to our readers in full. They are characteristic of the school whence they originate, and illustrate some of our preceding remarks.

"Those phenomena presented by organic bodies, in the reception, the assimilation, and the rejection of matters, in respiration, in the motion of fluids, in nutrition, inasmuch as they possess the common character of being manifested by motions, justify the assumption of a single cause, or a single force capable of producing in the different parts the same, or at least corresponding phenomena. But these essentially differ from the effects of chemical or mechanical forces; nay, are manifested in opposition to them. The force, then, which is assumed as their cause, must be peculiar. It may be designated from the phenomena, the motor force (*vis motoria*); or, since it is apparent in all organized beings, organic force (*vis organica*); or, since it is the source of the chief processes in plants, vegetative force (*vis vegetativa*.) According as its manifestations are accompanied by change in space *without* contraction or *with* contractions, it has been considered under two different appellations, viz. *plastic force* and *contractile force*. And these again according to the variety of their effects have received different names and been considered as special forces. The first of them as *assimilative, absorbent, secreting, nutrient* force; the other as *muscular* and *contractile* force. Besides the phenomena due to these various forces, which have their common character in one and the same cause, there are other phenomena manifested in psychical activities. These appear later than the corporeal: they are peculiar to animals. In the lower orders of being they appear as sensations and desires, are gradually developed into perceptions and resolves; on these are founded the phenomena of thought and the accomplishment of purpose. Next come the processes which are accompanied by fancy and memory.

"All the phenomena of psychical life, (like those of corporeal,) admit of reference to one force, as the cause of the processes of the understanding and the will; and which many (Descartes, van Helmont, Stahl, Whytt, Bianchi, &c.) have designated as the one fundamental force of animal life. It is ordinarily called, from the system through which it operates, *nervous force*; but, from its essence, it may be more conveniently denominated *psychical force*." . . . "The aggregate of manifestations and effects of organic force is called *body*; the complex totality of the phenomena of nervous or psychical force, *soul*. The two are necessarily and intimately bound together in man, and cannot be separated even in thought; for they mutually determine and cause each other. They are powerfully and multifariously interwoven. Psychical life is related not merely to the understanding and the will, but to the corporeal processes also. And these in turn, but more especially the total amount of formative activity, have an effective reaction upon it. Body and soul together make up a whole which is exhibited to us in its highest perfection in the human organism.

"Since corporeal and psychical life are, in man and animals, thus essentially united and mutually dependent, a remote cause of life is assumed, which calls forth the organism in form, and originates its activities—the *vital force*, (*vis vitalis* of Blumenbach, Barthez, Sprengel.) This force it is which produces from fluids the primitive organic form, the globule or vesicle, and causes these phenomena of inherent activity which display themselves in the elementary form as effects of an internal cause and of fitness for an end, (*Zweckmassigkeit*.)"

"Since man exists in a world which supplies the materials of that constant interchange by which he repairs the powers which have been exhausted in vital processes, he must possess, in his various parts, the property of being susceptible of external influences, that he may act in consequence of the affection received from them. This susceptibility (*facultas percipiendi*,) exists in every part; and, inasmuch as it is a common property, corresponds to the vital force, of which it is a necessary predicate, being displayed in various degrees in the various structures of the body. External influences which determine the living body to action are called *stimuli*. The property of the organism, in virtue of which it is capable of reaction or of vital manifestations on the application of stimuli, is called *excitability*, (*incitabilitas*.) The condition of the body, and of its parts, thus induced, is called *excitement*, (*incitatio, reactio*.)

"Susceptibility (*perceptivitas*,) is present wherever there is vital force. As that force is displayed in two different directions, body and mind, so also are there two directions of susceptibility. As a property of an organism by which it perceives stimuli which determine it to reactions, consisting in changes in space, it is present wherever there is organic force. It is then called *irritability*. As a property of the human body by which it perceives external impressions, through which sensations are produced in the organs and structures of the body, it is confined to animal organisms, is a predicate of psychical force, and is called *sensibility*." (§ 301 et seq.)

In those which Arnold gives as the general results of observation and reflection directed to vital processes, we find too much of the language of ordinary physics; and too little of accuracy in the proof that all the various so-called "forces" are but specific expressions of one and the same fundamental force. There is, in short, the same defect here as that which we alluded to in the system of Borden, Barthez, and others. Besides this, to speak of "a force of secretion," "of absorption," &c. may lead the inexperienced to suppose that these are real entities; that they explain the facts which are referred to their agency, without reference to the structures in which the phenomena occur. But this cannot be the intention of Arnold; and, if not, the term "property" might in all cases, except that of the highest cause, be substituted more conveniently for that of "force." But even this exception leads us again to the point from which the opinions of physiologists diverge so widely, according to the philosophical notions which they may have imbibed. With one set, force and matter coincide, force is a property of matter; with another set, matter is either a production of force, or force is something super-added to matter. They are conceived of as distinct things: force as an entity which does not occupy space; vital force as the architect and preserver of the body.

III. This question leads us to notice the third of the works which we have placed at the head of this article,—that of Dr. FLETCHER, which he modestly terms "Rudiments of Physiology." It is the work of one who was enriched with much learning, and practised in original thought upon the philosophy as well as the phenomena and laws of life. The third part, at least as far as the composition goes, may be considered as the

work of Dr. LEWINS, who has very lately given it to the world from notes of his deceased friend. We are led to notice this work in connexion with German physiology, because its author was well read in the works of German physiologists, and ably defends many of their tenets; and because we have thought that, to point out this character in a work of so much general talent, might be acceptable to many of our readers. We cannot pass over Dr. Fletcher's excellent chapter "on the Aggregation of Organized Beings," so rich in anatomical details, so cautious in generalizations, so just, in most instances, in its criticisms upon several subjects of much interest and difficulty,—without pausing to regret that one so admirably adapted to the office of a teacher of physiology should have been removed by an early death from his high undertaking.

With respect to the fundamental question to which we have alluded, Dr. Fletcher is altogether opposed to the admission of a "vital principle," in the sense in which it is understood in the works of Harvey, Stahl, Hunter, Whytt, Sauvages, Bordeu, Bell, Prout, Kirby, and others,—viz. an elementary substance, which, entering into certain aggregations of matter, becomes the cause, not only of the peculiar form of bodies, but of their physical properties and of all their phenomena. This notion, derived from the earliest times, recommended itself to men's minds when they observed the mysterious change which all living bodies undergo in death; a change so remarkable, that ordinary language ascribes it still to the extinction of the "vital spark," the abstraction of some finer spirit, or "breath of life;" and still more was it recommended by their persuasion that they knew the essence and nature of those grosser forms of matter which surrounded them, and from which living bodies appeared so greatly to differ. A stone was with them essentially "inert;" if it moved, some external force must act upon it: bodies, therefore, which moved spontaneously must possess within them some cause of their motions distinct from their matter. In later times, says Dr. Fletcher, it is not by such reasoning that a substantial vital principle can be defended; for it is known that properties essentially different result from differences in the composition, aggregation, and substance of different forms of matter.

"If we reflect on vital actions, anomalous as they appear, what do we find in them, in fact, but certain movements of either particles or masses of matter, not certainly identical with, but still very analogous to those which, in unorganized matter, we call chemical and mechanical; and which we are contented to ascribe, not to any substantial principle of action, but to certain properties and powers resident in these matters, the reciprocal action of which gives rise to what are called attraction and repulsion? And why need we hesitate to admit that similar, though not the same, properties and powers may, in organized beings, be competent, while they are in mutual co-operation, to effect those actions in which life consists, and which, of course, terminate on the cessation of this co-operation, as the ingredients of a chemical compound cease to be agitated when their affinities are satisfied, and a watch stops when either the susceptibility of motion in its wheels is destroyed, or the requisite power ceases to operate upon them? It is true a living body appears to require no such additions of new ingredients as a chemical compound, and no such frequent winding up as a watch, to avoid falling into the soon-established repose of the particles or masses of inorganized matter. But we must keep in mind that, in the latter case, while the properties and powers of the substances in co-operation are soon satisfied and exhausted, there is no inherent renewal of these substances, and with them of those properties and powers to renew their proper actions; whereas, in the former,

it is the specific end of some of these actions to give rise continually to new aggregations of matter, distinguished by the same properties, and acted on by the same powers, as the old which have disappeared, so that the conditions of continual action are never for an instant suspended. It is not, then, that there is in living beings no addition of new ingredients, and no winding up, but that this addition and this winding up are incessant: and all that death implies, therefore, is a cessation of these as a necessary condition of life, in its character of a substantial vital principle on which such action depended.

"But is not, it may be asked, design, such as can be attributed only to some such vital principle within them, manifested in the heaving of the chest, the contractions of the heart, and the other sensible motions of various parts, as subservient to the several functions of organized beings; and still more in the various molecular actions, whereby, with undeviating accuracy, every particle of the machine is removed and deposited precisely at the time and in the quantity that is required, not only to form these several parts,—the eye or ear, for example, those finished examples of exquisitely adapted workmanship,—but subsequently to maintain each in a state of integrity, ever varying, yet ever the same? Undoubtedly; design deep and wonderful; . . . but this design is that of the great First Cause of all things, who has adapted in every case the physical causes, the immediate means, to the end to be fulfilled: not that of this miserable means, which acts and can act only in blind obedience to the laws imposed upon it." (Part II. a. p. 26, 28.)

After enumerating various instances in the inorganic kingdom which seem to him to be no less obvious proofs of the operation of final causes than those witnessed in organized matter, Dr. Fletcher proceeds:

"There is nothing, then, in reality more singular in the actions of organized beings than in those of unorganized matters,—nothing more indicative of design,—nothing more obscure,—nothing which stands more in need of a substantial principle, or resident entity, to account for it. We may, indeed, if we please, in conformity with the views of many philosophers, not only ancient but modern, call these actions of inorganic matters, chemical and mechanical, their life; but then, to be consistent, we must either allow, as the ancients did, a vital principle to inorganic matters, in common with organized beings, or we must refuse to the latter a principle of which the former are almost universally regarded in the present day as destitute. It is not contended that we understand the nature of any of these actions; it is not contended that we advance one step towards explaining them by ascribing them to the agents in question. But it is contended that we know the nature of vital as well as of either chemical or mechanical action; and that, if we are satisfied to attribute the two latter to the agents above specified, as is almost universally done, we cannot, without great inconsistency, refuse to ascribe the former, which are in every respect so analogous to them, to similar agents, instead of to an imaginary substantial principle, of the operation of which there is no more proof or probability in the one case than in the other. Of the immediate nature of physical causes in general we know absolutely nothing, since we are capable of recognizing their existence only by their effects. . . In like manner, we know not how or why a certain aggregation of matter called organized should be capable, when acted on by certain appropriate powers, of manifesting the phenomena of life. But we know that it does so; that, the more perfect is the organism, the more remarkable are these phenomena, and that any change in the former produces a corresponding change in the latter: and what other proof can we require or possess that organized matter is, *qua* organized, irritable or endowed with vitality, and that it is not upon any substantial principle of life that these phenomena depend?" (Ib. a. p. 29, 30.)

Thus irritation, or life in its simplest form, according to Dr. Fletcher, consists in the perception, by organized beings, of certain stimuli acting upon them, otherwise than either strictly mechanically, or strictly chemically. Sensation, also, according to him, is a perception also; but it differs not only in degree, but also in kind. The being manifesting sen-

sation, not only perceives, but perceives that it is perceiving; in conformity with the view of Glisson. In the case of irritation, the susceptibility is called irritability, and the power which calls it into action may be of various kinds. In the case of sensation, on the contrary, the susceptibility consists in a new property called sensibility, and the power by which it is called into action is always irritation. By this is understood that, when an agent acts upon a sensible part, it does not immediately act upon the sensibility of that part, and thus excite sensation; but upon its irritability, exciting in the first instance irritation, which irritation becomes now a new power, and operates upon sensibility so as to produce sensation.

We purposely avoid entering upon the all-important question of the *immortality* of any portion of man's nature, however designated, as not necessarily connected with the question of life regarded as the subject of physiological enquiry. It is, however, but justice to Dr. Fletcher to state that he is a very strong advocate for the immortality of the *soul*, which he considers as something quite distinct from *mind*.

It is amusing to see how extremes meet. One party of physiologists, the various classes of Stahl's followers, assert that life and soul are but different names for the same essence; an essence superadded to body, which evinces itself as formative power in building up and sustaining the body, and as psychical power in those animals in which the brain is developed; whilst, in the highest of these, this psychical power displays itself as self-consciousness,—i. e. without any intermedium of matter. To this party they who hold Dr. Fletcher's opinions are, in appearance, altogether opposed. They justly say, that we know nothing of essences as distinct from matter; that force is only a name for the supposed cause of phenomena which affect our senses; that for every phenomenon there must be a material substratum, otherwise it could not affect our senses. But they overlook the fact that we have consciousness of much that cannot, according to this interpretation, be called a phenomenon at all, since it affects no external sense,—viz. the operation of our own minds. According to the Stahlians, the soul acts upon matter in every part of the body, and in the brain stands forth as the highest flower of animality, in its own unmixed character, resting upon body on the one side, and free from it on the other. With the second party mind is in body also, but only as the complicated function of its highest structure, the brain. According to the apparent admission of both, mind does act upon matter, and matter does act upon mind; whilst they can never agree to what extent this mutual action may occur, nor in what we should understand by matter, and what by mind. Dr. Fletcher asserts, as we have seen, the existence of a higher principle than mind, the immortal principle; but with what part of the body it is connected is not very apparent. It can scarcely, in his system, have any locality; for, if it have, it must appear as an organ or as a faculty, and in either case be transitory and perishable. Until this new principle is introduced, it is not quite clear that there is any great difference between Dr. Fletcher and Stahl; nor does there seem any necessity to superadd to the mind of man this distinct principle in order to secure to him an eternal existence, which is supposed to be denied to brutes. The Creator of souls may give to some that imperishable nature which he has denied to others.

But we are here touching, with Dr. Fletcher, upon subjects quite beyond our enquiry. Even in objects of legitimate physiological research, we are soon taught how incapable we are of comprising many of those infinite relations of which its ordinary phenomena are the results,—as secretion, nutrition, &c. Yet these remarkable processes are effected by secondary means, the Creator working by laws impressed upon matter; and it is still the same in the yet more astonishing phenomena of sensation and of thought. And, if the process still be the same in the creation of such intelligence as the human soul, we can have no conception, the most distant, of that infinite wisdom which is able to impose laws whose operations are necessary, and which yet leave the will free and the conscience responsible.

Were we disposed to enter into a minute criticism of Dr. Fletcher's work, (which we are only prevented from doing by the length which this article has already reached, and the ground we have yet to pass over,) we should feel it necessary to point to much that we regard as fallacious, as well as to express our approbation of much that is sound and valuable. On a few topics of importance we shall record our dissent from him, in order that the many into whose hands his work will probably fall may be led to consider how far the weight of his arguments and the ingenuity of his reasoning establish the positions he endeavours to prove. These are sometimes invalidated by the inaccuracy of the facts he has assumed as their foundation: thus, in the comparative classification of plants and animals, which he has introduced into his first chapter, the Cryptogamic tribes are placed opposite to the Zoophytes and Mollusca; the Endogens are regarded as corresponding with Fishes, and the Exogens with the higher Vertebrata; the sub-kingdom Articulata being left without any analogous division in vegetables. Now, if any correspondence exist between the principal divisions of the two kingdoms, we cannot help recognizing that of the Endogens and Articulata as more evident than can be discerned between any other groups; and this analogy was pointed out by Desfontaines soon after his grand discovery of the essential difference in the structure of the Exogens and Endogens. A few pages subsequently we find it stated that no remains of Coniferæ, or other dicotyledonous plants, are found beneath the tertiary strata; although the fact is now quite notorious that nearly the whole of the coal formation is to be regarded as made up of the partially decomposed trunks of the pines and their allies. These inaccuracies, which are among the first that strike us on opening the volume, are, we regret to say, by no means solitary; and it is manifest that, however acute may be the reasoning founded upon fallacious statements, the results cannot be looked on but with suspicion. Whilst, therefore, we express our approbation of such chapters as that on the Nature of Life, where the clearness of the author's mind is peculiarly well displayed in grappling with the abstract conceptions which the subject necessarily involves, we must put our readers on their guard against similar displays of apparently conclusive reasoning built upon unsound premises. Thus, immediately after the vital properties have been proved to be endowments of organized matter, we find the author denying such vital properties to the chyle and blood, forgetting that both these fluids exhibit more or less of an organized structure; their globules (par-

ticularly those of the latter,) presenting an arrangement of their particles fully as complex as that of the simpler solid tissues.*

Still more erroneous appears to us the line of argument in the succeeding chapter, on the seat of Irritability. Believing this property to be confined to the ganglionic nerves, the author assumes, as the basis of his demonstration, that this system is universally distributed, not only through all those parts of animals in which it is known to exist, but even in those lowest organisms in which no nervous fibrils can be traced, as well as throughout the vegetable kingdom; his argument for this assumption being simply, that the contrary cannot be proved. Hence he infers that, as irritability and a nervous system universally coexist, the former is a property of the latter; and, returning to the point in the circle from which he set out, he asserts that vegetables possess a nervous system, *because they manifest irritability*. Now, if we are to assume every thing in anatomy and physiology of which the contrary cannot be proved, we must bid adieu to all hope of elevating them to the rank of exact sciences; and we can scarcely imagine a mode of proceeding more adapted than this to destroy what degree of certainty they have already attained.

From these animadversions we pass with pleasure to the chapters on the Stimuli to Irritability, which contain very much that is valuable, arranged in a clear and well-digested form; and here, as in other parts of the work, the amusing character of the notes relieves the scientific dryness of the text; although it is, perhaps, our duty to criticise these also, as occasionally bordering on that grossness of idea which cannot but offend the truly refined mind.

In the section on Sympathy and Passion, or Instinct, many interesting facts are well grouped together; and we fully agree with Dr. F. in considering the actions excited by these stimuli as of an involuntary character, although we differ from him as to the degree in which the mind participates in their performance: to this subject, however, we shall return in our next Number, when it is our intention to review the whole of the recent discussions on sympathetic and excited actions. We are soon obliged, however, to restrain the expression of approbation; for the following section is devoted to the demonstration of the conveyance of Sympathy, Passion, and Instinct by the Respiratory System of Nerves. We doubt not that these nerves form a part of the general system by which the movements thus produced are excited; but the portion of the instinctive motions which they produce is, to our apprehension, a very small one; and we can see no ground whatever for attributing the *organic sympathies* (which Dr. F. has peculiarly well stated in a collec-

* Dr. F. thinks it superfluous to speak of the vegetable fluids in connexion with this question, (II. a. p. 42:) we, on the contrary, think that they have an important connexion with it, as no general statement can be correct which will not include all the cases to which it applies. We may refer our readers to the curious observations of Amici on the formative power of the elaborated sap of the vine, (*Ann. des Sci. Nat.* t. xxi.) and to those of Mr. Knight on the Vegetable Fluids. We may also take this opportunity of mentioning a fact to which we do not remember to have seen anything analogous stated elsewhere: a patient, supposed to be labouring under endocarditis, having been bled, the coagulum (formed in a common bleeding-dish,) exhibited at its edges a number of little wart-like processes, exactly resembling in character the softer granulations which are seen on the cardiac valves after death from this disease.

tive form,) to any but the ganglionic system. The universality of the distribution of the respiratory nerves, which is our author's main argument, is a mere assumption, and is only presumed to exist from the connexions between the pneumogastric and the sympathetic: we are here, therefore, again called upon to protest against such an unstable foundation being employed as the basis of a train of reasoning, the ingenuity and connectedness of which might well deceive the unwary into a belief in its solidity.—We shall here take our leave of this work; and in doing so shall again express our regret that its highly-gifted author did not live to bring it to a termination, and that the cultivators of the science of physiology have been deprived of the aid of one so peculiarly fitted, by his extensive information and acuteness of intellect, to promote its interests.

IV. The progress of our review now brings us to a work planned and executed in a very different style, which we regard as peculiarly characteristic of the English School of Physiology; but, before proceeding to a detailed examination into its merits, we shall offer a few remarks on the progress of the science in this country since the time of Haller, and point out what may be considered the principal advantages and disadvantages of the method in which it is usually cultivated in Britain. Although Glisson had, long anteriorly to Haller, propounded in a general form doctrines similar to those which Haller particularly applied to the muscular structure, and which, as we have already shown, his successors extended to other systems, the little influence they had acquired in the country where they originated is shown by the reluctance evinced by the British physiologists to give up the favorite tenet of the then predominant school, that all the vital properties of the tissues are referrible to the influence of the nervous system. The keen discussions carried on by Whytt and Porterfield with Haller and his followers, doubtless, contributed to the advancement of theoretical physiology in this country; although the effect of the writings and instructions of the first-named author is almost as evident in the more recent productions of our speculatists, as is that of the sounder and more philosophical doctrines of Haller on the works of the German school, or at least of that portion which has not been mystified by the abstractions of transcendentalism. There are few English physiologists, even of the present time, who do not in their writings acknowledge, or even defend, the notion of a distinct controlling agency in every living system, although every writer has his favorite designation for this fiction of the imagination. We can imagine nothing more calculated than this doctrine to check the real progress of physiological enquiry; since, to refer every inexplicable process in the living system to the operation of a Vital Principle is about as conclusive a method of saving the trouble of further investigation as that of Molière's candidate, who, to the "*Quapropter opium facit dormire?*" of the examiner, replies with his "*Quia habet vim dormitivam.*" The influence of the doctrines of Whytt was aided and strengthened by the investigations of Hunter; and fully conceding, as we do, the great merit of the latter in establishing the operation of vital properties in the various processes of the animal economy, both healthy and diseased, it is a

source of regret that his name has assisted to keep alive, especially among his friends and pupils, a system which we are convinced he would, if now alive, do his utmost to overthrow.

It has been a necessary consequence of the prevalence of the doctrine of a vital principle, that little pains have been taken by English physiologists to ascertain the elementary vital properties of the tissues and the laws of their operation; a study which, on the continent, has been so fruitful and important in its results. Even the brilliant theory of Brown excited but temporary attention in this country, and was forgotten almost as rapidly as it was brought into notice; whilst, as we have seen, it assisted greatly in the revolution in speculative physiology which was taking place in Germany, by directing attention to the external agencies on which vital actions are as much dependent as on the vital properties of the organism. Still, however, our countrymen have not been idle; and their labours, directed in a different channel, have been eminently successful in the departments chosen for research. The British School of Physiology is essentially a practical one; the object which it has kept steadily in view has been the discovery and establishment of facts; and it has been particularly advantageous for its progress that it has been generally unincumbered with those premature theories and fanciful speculations which have frequently exercised an influence over continental physiologists, of which they were themselves unconscious. Although, as we formerly observed, the number of labourers in this department of medical science has hitherto been small in Britain, compared with that which is to be found amongst the continental faculty, this very circumstance perhaps increases the value of the results obtained by the former; since it is to be supposed that none embrace the pursuit who are not conscious of its importance, and determined to advance its true interests. Moreover, there is in this country a very strong and, we think, a very proper feeling against that indiscriminate employment of experiment which characterizes some of the continental schools; and the objection applies not only to the cruelty, but to the worthlessness of the practice. Hence the results of experiments made with caution, and intended more as a verification of opinions founded upon other grounds, than as a *means* of discovery, are evidently more trustworthy than those of trials made at hazard, with no definite end but the expectation of some striking result. Whilst, therefore, speculative physiology may certainly be regarded as less advanced in Britain than on the continent, we cannot but feel that the progress which the science has made in this country may be considered as strictly inductive, and that the materials which have here been collected are worthy of being placed amongst the most valuable and important which any country has contributed. We cannot forget that it was Hunter who first gave prominent expression to the fact that the truths of physiology are to be sought, not in one or two systems alone, but in every variety of state of each living organism, whether animal or vegetable; and the influence of his life and labours has, we are certain, done far more than counterbalance the injurious effects of some of his doctrines, by the earnestness with which he inculcated and practised that freedom and extent of enquiry against which no erroneous dogmas, however supported by authority or sanctioned by time, can long maintain their ground. The work of Dr. Fletcher affords a notable instance of

the baneful influence of preformed theories in warping or distorting facts. Wherever the author has employed his acute powers in reasoning from the known to the unknown, his conclusions are stable and definite; but, as soon as he admits the influence of a favorite theory, his ingenuity is misemployed in torturing the evidence upon which it is to be established.

Notwithstanding, however, the great advantage which has accrued to the English School of Physiology from its attachment to facts and disregard of hypotheses, we cannot but think that there is rather too much caution exercised regarding the admission of some of those generalizations which have been fairly established by observation and experiment. The work of Dr. Roget was the first in which the ascertained laws of development were placed before the British public; and we have heard the author severely criticised by men of some eminence in our profession, even for the very moderate, and we should have thought unobjectionable form in which these doctrines were put forth. That of Dr. Fletcher has taken a still bolder step, yet one fully justified, in our opinion, by the mass of facts he has accumulated in its defence; and we look forwards to the time when the doctrines to which we have alluded shall not only be received upon sufferance, but confirmed and extended by the labours of our countrymen. They certainly stand upon a much surer foundation than the shadowy *Archæus* which has so long held dominion amongst us. We should be sorry to see less eagerness in our schools for the observation and collection of facts; and an exclusive devotion to this object is certainly less practically injurious than a complete reliance upon theory. It is well that there should be in physiological science, as elsewhere, a conservative section, who may restrain the movement party from advancing with unsafe rapidity; and it has always appeared to us a very beautiful adaptation of the varieties in the constitution of the human mind to the external world, that there should be this balancing of tendencies. Nevertheless, we should like to see more regard paid in this country to physiology as a *science*,—that is to say, as an expression of general principles, and not a mere collection of facts.

The favorable reception which the former editions of Mr. MAYO's Outlines have experienced from the medical public might appear to render superfluous the expression of our opinion regarding the general merits of the work, since these have already been pronounced upon by a tribunal to which ours must be regarded as subordinate. We cannot avoid, however, pointing out the features which peculiarly distinguish it from other elementary treatises on the same subject; and it is then our intention to notice the principal improvements which have been made in the present edition, and to advert to those of which we consider it still susceptible: and, if it should appear to our readers that we bestow a disproportionate degree of attention on the last-named topic, we hope it will be believed that, far from wishing to depreciate in this manner the estimation in which these Outlines are deservedly held, we are only anxious that the work should be rendered still more worthy of general regard.

The main purpose of the work is to afford to the student of physiology an outline view of the facts at present recognized in the science; and, keeping this end in sight, its author has in most cases studiously avoided entering on the discussion of disputed questions, and has exercised a due degree of caution in the admission of those generalizations (many of

them more specious than solid,) which some of the continental writers on this subject are so fond of putting forward. The facts are in most instances well selected and arranged, and detailed in clear and concise language; so that, without exhibiting the comprehensive learning of Bostock, or the profound talents of Alison, Mr. Mayo has presented to the English student a work free alike from the embarrassing richness of the "System" of the former writer, and from the somewhat abstract refinements which perplex the ordinary reader of the "Outlines" of the latter. Hence we should say that, as an elementary treatise, we regard Mr. Mayo's as the most practically useful; whilst the more advanced student may receive a large increase to his knowledge of the history and facts of his science from Dr. Bostock's work, and of its principles from that of Dr. Alison. When the science of physiology shall have been so far simplified as to render a knowledge of the structure and functions of any particular being a process of simple deduction, then, we feel convinced, the acquirement of its *principles* should be the first object; but we doubt if this can, with most minds, be attempted in its present condition. Having already quoted the opinions of authors of the German School on the most essential doctrine of physiology, that of a Vital Principle, we shall bring into contrast with them a brief extract from Mr. Mayo's first chapter, which contains the clearest statement we have hitherto met with of the true signification of the words *law*, *property*, and *principle*.

"The word *law* serves to express the conditions essential to a change; the word *property* attributes to a substance the power (capability) of producing a change under ascertained conditions; while the word *principle*, characteristic of a less advanced state of science, has been generally employed (as the final letters of the alphabet are used by algebraists,) to denote an unknown element, which, when thus expressed, is more conveniently analyzed. . . . Hitherto the laws of life, or the properties of living matter, have not been determined with precision; and physiologists have shown themselves reluctant to disuse the vague term 'a principle of life,' to which an imaginary and delusive meaning is attached by many, who forget that terms of this nature have no real value, except as generalized expressions of facts." (P. 2.)

With the second chapter, on the Blood, we are sorry to feel called upon to express considerable dissatisfaction. Although much addition has recently been made to it, the author has neglected many observations of superior importance, which ought unquestionably to have been introduced. No notice is taken of M. Lecanu's analysis of the blood, or of his interesting enquiries into the varieties it presents in the proportions of its solid and fluid parts, according to age, sex, temperament, previous depletion, and various constitutional states. The observations of Hewson on the form of the globules are also omitted; and we are the more surprised at the neglect of this most accurate authority, because his statements, though denied by Messrs. Hodgkin and Lister, have been confirmed by MM. Prevost and Dumas, and still more recently by Professor Müller and M. Milne Edwards; the two latter of these observers having arrived at the same conclusions independently of each other. But the greatest omission is that of the view of the constitution of the blood in the living state, which originated, we believe, with Hewson, and which, after steadily gaining ground in this country, has been proved to demonstration by Müller. Although Mr. Mayo has entered pretty fully

on the causes which influence the rapidity of the coagulation, he has neglected one remarkable and important fact, that it is most rapid where the proportion of serum to crassamentum is largest, and especially where this arises from previous loss of blood, which bears strongly upon the *questio vexata* of the vitality of the blood. Indeed, we think that he has given his opinion rather hastily, when he says that the phenomenon of coagulation may be regarded as the result of the chemical composition of the blood, since it has led him to pass by the very interesting connexion which it has been shown to have with the rigidity of muscles after death, as well as many other facts and arguments which have been advanced in support of the contrary position.

To the chapter on Muscular Action we do not perceive that any addition has been made, nor perhaps is much required; though a notice of Nysten's experiments on the irritability of various muscles after death, as tested by the effects of galvanism, might have been advantageously introduced. The classification of the muscles according to the nature of the stimulus by which they are excited, is, we believe, peculiarly due to Mr. Mayo, and probably leads, more than any other, to a just appreciation of their characters. The primary divisions of voluntary and involuntary muscles are formed by the possession or want of sensibility to the stimulus of mechanical irritation, conveyed through the nerve distributed to each. The strictly involuntary muscles contract only by the application of a stimulus to themselves, and their action is usually unattended with consciousness, and cannot even be controlled by the will. The class of voluntary muscles, on the other hand, contains not only those which respond to mental influence, but those which perform many automatic movements without the aid of that influence, and even in opposition to it. Mr. Mayo appears to have some difficulty in subdividing this class according to the actions of the muscles, since they are all alike called into play by nervous influence; and he does not seem to conceive it possible that such influence can be transmitted without an effort of the will. On this subject we may refer to some observations in our last volume, (p. 421,) which appear to us to set the involuntary action of the reputed voluntary muscles in a clear light: this is not to be confounded, however, with that of the purely involuntary muscles, since the former still depends upon the stimulus of innervation, whilst the latter cannot be excited by it.

In his chapter on the Mechanism of the Circulation, we are sorry that Mr. Mayo has not more fully availed himself of the many valuable results which have been laboriously and carefully attained since the publication of the former edition of his work. Thus, when speaking of the action of the mitral valve, he states that, during the systole, the direction of the chordæ tendineæ is oblique, and not in the axis of the ventricle; so that the two opposite edges of the valve are brought into contact by their tension. Now, the Dublin committee, appointed by the British Association, directed special attention to this point, and came to the conclusion (in which they were partly confirmed by the Report of the London Committee,) that the action of the chordæ tendineæ does not produce closure of the valve, that being effected by the pressure of the reflux blood at the moment of systole; but that it draws the flaps of the valve into the most advantageous position for being thus pressed together.

Another remarkable instance of Mr. Mayo's complete neglect of these valuable Reports occurs soon afterwards. "The first sound of the heart is probably caused by the flapping-to of the mitral valve, and the protracted vibration of the tendinous cords; the second, by the flapping-to of the sigmoidal valves. It was found by Dr. Hope, that the preventing either valve from closing, by wires passed into the heart, put an end to or impaired the sound attributed to the shutting of that valve." In opposition to this statement, we might bring forward the numerous experiments of the London Committee, which appear to prove that the auriculo-ventricular valves are incapable of producing a sound analogous to the flapping-to of the sigmoid valves; and that, if their action be prevented, the first sound is scarcely diminished. With respect to the relative value of these statements, our readers must use their own judgment; but we cannot help being surprised that the results of investigations, so important in their character, and so fairly and ably conducted, should have been neglected in a treatise like the present.

Without stopping to discuss Mr. Mayo's theory of Inflammation, the objections to which have elsewhere been fully stated,* we must remark, that the author has left entirely out of consideration all those phenomena which satisfy us that the circulation of blood in the living system is not altogether dependent upon the impulse of the heart and the contraction of the arteries. It is, perhaps, desirable in an elementary work to avoid entering largely into the discussion of controverted questions; but the leading facts on each side should be fairly stated in such a form as to encourage further enquiry on the part of the student. In this particular instance, too, the enquiry into the nature of the capillary circulation, the causes by which it is maintained, and the influence it undergoes from external objects, may be regarded as a question of the highest practical importance; and we are therefore the more surprised that it has been here overlooked.

Little alteration has been made in the chapter on Pulmonary Circulation, but one of the additions to it is entitled to notice. "It is evident that, when we view the carbonic acid of expiration as directly thrown off from the blood, it must be classed as a secretion. If so, it forms a remarkable instance of a secretion that can be made to take place after death." (P. 64.) With the view expressed in the first sentence we fully coincide. The structure of the lungs, when regarded in its general conditions, is precisely analogous to that of the acknowledged excretory organs; nor can any reason be assigned why the exhalation of carbonic acid from the lungs should not be placed on the same footing with the transpiration of aqueous fluid from the skin. If regarded in the light of a secretion, we certainly have it in our power to trace much more clearly than in any other case the chemical changes essentially concerned in it; but does not the rapid advance of organic chemistry encourage the hope that we shall be able, ere long, to obtain as full comprehension of other processes of the animal economy as we now possess of the function of respiration? In considering the organic changes which take place after systemic, or (as Dr. Prichard suggests) *somatic* death, we must bear in mind that none of these can be much prolonged unless the means are supplied

* Supplement to Alison's Physiology, pp. 19, 20.

by the continuance of the circulation, which is the common bond between them all. Some also require the aid of the organs of animal life; but it may, we think, be stated as a general fact, that even when sensation and volition have ceased their manifestations, and nervous agency is totally extinct, each one of the organic functions may continue as long as vital properties remain in the tissues which perform it, and the necessary means are supplied. Thus, the process of respiration may undoubtedly be performed after death, without artificial aid; for the heat frequently returns to the body, and the livid hue on the faces of those who have died from asphyxia often gives place to a vermilion tint: there can be little doubt, therefore, that a similar change is taking place in the capillaries of the lungs, if oxygen be in contact with them. But, in order that these processes may *continue* to be performed, there must not only be a movement of blood in the vessels, but also a renewal of oxygen in the lungs; and hence the artificial assistance sometimes given by the inflation and compression of the thorax cannot be regarded as *producing* the changes which essentially constitute the function of respiration, but only as enabling them to be prolonged.

Amongst other omissions that we feel called upon to notice is one which is calculated to give to the student a very erroneous view of one of the most important practical questions in physiology: we mean the cause of death by asphyxia. Mr. Mayo repeats in the present volume the statement which we find in the former edition, that life is destroyed by the continued suspension of the energy of the brain, occasioned by the circulation of venous blood; and that the heart's actions are checked by the loss of muscular irritability, from the same cause. Had the opinions of Drs. Williams and Kay been mere hypotheses, they might fairly have been excluded from a work which professes to give a view of that only which may be regarded as ascertained in the science; but, as these opinions are supported by numerous well-devised and carefully conducted experiments, which no one has attempted to invalidate, they should, we think, take the rank to which, as legitimate inductions, they are entitled. That the circulation of venous blood in the brain does not immediately produce cessation of vital actions, has been fully proved by Dr. Kay; and that some other cause than the want of the stimulus of arterial blood is concerned in extinguishing the irritability of the heart, appears probable from the experiments of the same author, and also by those of Dr. J. Reid,* who has shown that, when the action of the right ventricle ceases in cases of venous congestion, it may often be restored by such a slight abstraction of blood from the cavity itself or the veins leading to it, as will relax the over-distended fibre; a fact, the practical importance of which it is needless for us to point out. And, finally, we regard it as almost positively established that the chemical changes for which the pulmonary circulation is adapted, are essential to the continuance of the flow of blood through the capillaries of the lungs; and that the cessation of the respiratory movements only acts secondarily upon the circulation, by impeding these changes. We do not call upon Mr. Mayo, however, to exchange his opinion for ours; but simply to give that attention to the evidence on the subject which it certainly merits.

* Edinburgh Med. and Surg. Journal, vol. xlv. p. 387.

The sections devoted to Secretion and Animal Heat claim but little remark from us, since they present a very fair outline of the present state of our knowledge on these topics. We may remark, however, that many facts, besides the *one* mentioned by Mr. M., confirm the idea that the ingredients of the excretions pre-exist in the blood; and that there is much evidence, not adduced by the author, to prove that animal heat is principally maintained by the chemical changes constantly taking place in the tissues. We are ourselves inclined to the belief that the influence of the nervous system upon the temperature of the body is only secondarily exercised by the control which it certainly possesses over these molecular changes; and this belief derives confirmation from the remarkable facts with which vegetable physiology supplies us, and to which we have alluded in a former article. We cannot leave the subject of secretion, however, without expressing our great satisfaction with the very clear and concise analysis which Mr. Mayo has given of Mr. Kiernan's discoveries in the anatomy of the liver.

On the chapter on the Functions of the Stomach we have only to observe, that no allusion has been made to Brâchet's experiments on the influence of the par vagum on chymification. We do not refer to them as having solved this difficult problem, but as offering one out of many possible explanations; and the circumstance of the Monthyon prize having been adjudged to their author may be regarded as entitling them to some notice. If Brâchet's experiments be correct, there can be little doubt that the function of the par vagum is, on the one hand, to convey to the sensorium the impression that produces desire of food; and, on the other, to produce those motions of the parietes of the stomach which are necessary to the first process of digestion. It is fair to state, however, that some of the experiments we have ourselves witnessed do not altogether confirm those of Brâchet; but any one who is acquainted with the complicated distribution of this nerve must perceive the difficulty which is created by its numerous anastomoses, in arriving at any certain results. If the nerve be divided between the superior and inferior laryngeal branches, their connexion will, to a certain extent, keep up the nervous current; and, if the trunks only are divided where they accompany the œsophagus, the plexus formed upon that tube is quite sufficient for the temporary supply of nervous agency to the functions of the stomach.

We are sorry to be obliged to continue our notice of omissions by pointing out one that relates to a question in physiological anatomy, which is still exciting the highest interest; namely, the origin of the lacteals and lymphatics, and the nature of their communication with the veins. On the former point, Mr. Mayo cites the old observation of Cruikshank, and omits to mention that almost every modern physiologist has now abandoned the idea that the absorbents commence by open mouths on the villous coat of the intestines; even Magendie, who long retained this opinion, having now become convinced of the contrary. The question of the communication of the circulating and absorbent systems is, we admit, still open to controversy. Whatever weight be attached to the experiments of Fohmann and Lippi on the affirmative side, at least equal credit is due to those of Rossi and Panizza on the negative; and these should scarcely have been omitted by Mr. Mayo.

We are glad to be able again to exchange the tone of censure for that

of commendation in regard to the addition, which next presents itself, of an excellent outline of M. Breschet's observations on the structure of the skin; and still more respecting the chapter on the Nervous System, which now comprehends an extended view of its anatomy, completed by the introduction of the latest discoveries, with much that is interesting and useful regarding its functions. It will be seen, from our former statements, that we differ widely from Mr. Mayo respecting the nature of instinctive actions: he considers them as the result of unavoidable impulses acting through volition; whilst we regard them as independent of volition, and produced by the direct influence of external impressions operating through the cerebro-spinal axis, and the circle of nerves proceeding from it. Experiment and observation alike prove that the hemispheric ganglia are not necessary to the performance of the instinctive actions immediately concerned in the maintenance of life; and it is difficult to conceive that sensation and volition can reside in the spinal cord without any other mental faculties. Whatever view we take of this question, however, we fully agree with Mr. Mayo in the belief that almost all animals enjoy, in a greater or less degree, the possession of various mental faculties, analogous to those of man, and superadded to their instinctive powers. These mental faculties appear to us to give the capability of education; and their extent in different tribes of animals would seem to bear some general relation with the development of the hemispheric ganglia, or the parts corresponding to them in the Invertebrata. The instincts themselves can scarcely be modified, except by the controlling power of the will, or by some alteration in the organization of the being, adapting it to a change of circumstances; and this modification is transmissible to the offspring, as Mr. Lyell has beautifully pointed out, only when it is connected with the maintenance of the existence of the animal in its new situation. We could have wished to enlarge more upon this portion of Mr. Mayo's work, unquestionably the most valuable; but our limits compel us to confine ourselves to his pathological doctrines (now introduced for the first time in these "Outlines,") on which, as they have not been previously noticed in our Review, we shall offer a few observations. These we shall introduce by reminding our readers of the anatomy of the medulla oblongata, in which, as is well known, some important discoveries have recently been made.

According to Mr. Mayo's description, "The greater part of each half of the medulla oblongata,—that is to say, all but the corpus restiforme and the anterior pyramid,—is continuous with a tract, which ascends behind the pons Varolii to the cerebral hemisphere of the same side; and therefore places that cerebral hemisphere in mechanical communication with the nerves of sense and motion of the same side of the body." Mr. Solly, however, seems to regard this tract as commencing with the *sensory* column only of the spinal marrow, and as terminating in the thalamus of the same side.* With regard to the corpora restiformia, Mr. Mayo, after alluding to Mr. Solly's discovery of their communication with the anterior column, concludes that, by their means, "each hemisphere of the cerebellum is placed in mechanical communication

* Solly on the Brain, p. 226.

with the origins of the nerves, both motor and sentient, of its own side of the frame." If Mr. Solly's idea, that part of the cerebellic fibres proceeding from the motor column enter into its decussation,* prove correct, it is evident that each hemisphere of the cerebellum will then be connected with both sides of the spinal cord. Mr. Mayo then proceeds to describe the decussation of the fibres of the corpora pyramidalia. He first shows that "the anterior pyramid of one side is placed in mechanical relation with the corresponding hemisphere of the brain;" and then states that "a thin layer of the outer filaments seems to spread, some to the corpus restiforme, some to the anterior surface of the same half of the spinal marrow, but by far the greater part bend over to the opposite half of the spinal marrow, dipping towards its centre, into which they plunge in such a manner as to place themselves in mechanical relation with the anterior and posterior parts of the opposite half of the cord."

Here we again find Mr. Solly differing from Mr. Mayo; the former author regarding the corpora pyramidalia as exclusively belonging to the motor tract, which commences in the corpus striatum, and terminates in the anterior column of the spinal cord. We mention these varieties in the opinions of anatomists who have paid great attention to the subject, as proving that we are not yet in a condition to draw any very definite conclusions from pathological phenomena with regard to the influence of cerebral lesions on the functions of the nervous system; a fact which is acknowledged by the most eminent authorities. Mr. Mayo has, however, advanced a theory on the subject (explained at greater length in his "Outlines of Pathology,") which he considers as giving a satisfactory explanation of the manner in which the decussation of the anterior pyramids contributes to the production of paralysis on the side of the body opposite to that in which the cerebral lesion exists.

"When a cerebral lesion produces palsy, it cannot be supposed to act by interrupting the customary supply of cerebral power to the spinal marrow: for, if it did, the removal of one hemisphere of the brain, or its lateral compression in an animal (from which a large part of the cranium has been removed,) should produce hemiplegia; and an acephalous infant, or an animal with the cerebrum and cerebellum removed, should be completely palsied; which is contrary to fact. Another consequence of this mode of influence, if it existed, would be, that, in palsy strokes, the parts the most remote from the brain would have their supply of nervous energy soonest cut off, which again is contrary to fact; inasmuch as the arm is, in almost every case, struck first, and more severely than the leg, and, when recovery takes place, is slowest in being restored." (P. 222.)

We must say that Mr. M.'s reasoning appears to us somewhat inconclusive. We all know that there are many motions to which the influence of the brain is not essential; these being performed alike in the state of apoplexy, in which volition is entirely suspended or destroyed, in acephalous infants, and in animals from which the brain has been removed. But these movements require only the existence of the spinal cord, and the completeness of the circle of sentient and motor nerves connecting it with the part. Hence there is no more reason why one side of the body should be paralysed by the removal of one hemisphere, than that the whole should be similarly affected by the removal of the entire brain. The fact mentioned in the last sentence of the preceding quotation is

* Op. cit. p. 155.

indisputably correct, but we do not see that it warrants the inference drawn from it. Mr. Mayo probably agrees with us in believing that the motive influence affecting each portion of the body originates in a distinct part of the cerebral mass, although pathologists have hitherto failed in distinctly tracing the connexion: if this be the case, it is very easy to understand that the portion from which the voluntary motion of the upper extremity originates may be more liable to hemorrhage and other lesions than that connected with the lower; and that, being usually more severely affected, it shall be slowest in recovery. It is highly probable, too, that the superior extremity has more extensive connexions with the brain than the inferior; since its actions are more frequently associated with mental operations, both in the needful occupations of barbarian industry and in the finer arts and accomplishments of civilized life. But Mr. M. takes a very different view, and infers, from the facts already adduced, that the cause of the paralysis is "an actively depressing influence propagated from the diseased brain to the nuclei of the segments of the medulla oblongata and spinal cord."* We have already endeavoured to show that there is no necessity for such an assumption, and we shall now point out what appears to us its inconsistency with known facts. If such an "actively depressing influence" be the cause of the paralysis, it is difficult to conceive how it can be propagated to the lower part of the spinal cord without involving the upper, unless that distinctness in the connexions of the nerves with the cerebellum be admitted, which we have shown to render unnecessary Mr. M.'s conclusions. The author admits, as an objection to his theory, that, in hemiplegia from cerebral disease, the leg is sometimes affected alone or in a greater degree than the arm; and allows that, if this occurrence were frequent, it would invalidate his hypothesis. We cannot see why a single decided case should not have the same weight as a hundred, when directly opposed to an hypothesis; but, in the present instance, Mr. Mayo has a mode of escape which would probably not suggest itself to many of our readers; "Occurring as it does very rarely, it admits of many explanations, one of which is the following. Suppose the spinal cord to be predisposed to paraplegia, then the hemiplegic shock supervening and being slight, the phenomenon to be accounted for would naturally ensue." Moreover, it seems to us that the depressing influence ought to annihilate all capability of producing motion where the paralysis is complete,—that is to say, where the power of the will is entirely destroyed; but we have sufficient evidence that this is not the case in the motions which may be excited by internal and external stimuli, in apoplectic and hemiplegic patients. The impulse, according to Mr. Mayo, is received by the spinal cord *en masse*, as it were, and the extent of its propagation is determined altogether by distance. A shock is given to the nervous fluid, and the undulatory

* This idea is not so novel as Mr. Mayo appears to suppose. We extract the following passage from an article on the Physiology of the Brain, contributed by Dr. Gordon to the Edinburgh Review: "The insensibility which follows a division of a nerve, or of the spinal cord, or a destruction of the brain, admits of an equally probable explanation, on the supposition, either that the brain is constantly supplying something to the different parts of the body which enables them to feel, or simply that some injurious effect is propagated downwards along the nerves from the parts which are injured above." —Vol. xxiv. p. 452.

disturbance, though violent in the nearer parts, becomes fainter and fainter till its waves are lost in the more distant. The spinal cord is treated as if it had no distinction of parts; no relation but that of proximity or remoteness to the nerves connected with it. But, if Mr. M. had said that the shock was communicated through the spinal cord in tracks corresponding to the nerves affected, we should not have called his statement in question; for it cannot be doubted that lesions of the brain frequently exert something more than a privative influence,—as in the circumscribed spasmodic and convulsive affections,—whether we call it shock, irritation, sympathy, disturbing or depressing influence, or any term expressing a relation of cause and effect between the central mischief and the peripheral disorder.

We cannot regard Mr. M. as fortunate in his attempt to explain the fact that, in hemiplegia, the face is paralyzed on the same side with the body, although there has been a decussation in the spinal column between the origins of their nerves. This is certainly an occurrence not to have been anticipated, and of a very puzzling character: it does not appear to us more extraordinary, however, than the fact to which Mr. Mayo has not thought proper to allude, that hemiplegia undoubtedly results, in many cases, from cerebral lesion of the same side;* nor do we think that Mr. M.'s theory gives us any assistance. But our readers shall form their own opinion of the value of his explanation. After pointing out that the origin of the portio dura of the seventh pair is above the decussation of the pyramids and just below the pons Varolii, he continues, "Is it surprising that the palsy shock, falling at a point so near this, and in continuity with it by reflected extension of nervous fibrils, should propagate its depressing influence upwards, and paralyze the seventh, the eighth, the ninth, the fifth nerves, one or all?"

We confess we do not understand the drift of this argument. The motor and sensory columns of the cerebro-spinal axis are usually regarded as passing through the pons varolii, (which is formed by the expansion around them of the crura cerebelli,) then proceeding under the designation of the crura cerebri to their termination in the corpora striata and thalami nervorum opticorum. One would naturally suppose, therefore, that a palsy shock originating in the cerebrum would be transmitted downwards along this line, paralyzing in its course the facial nerves arising from it, and would then cross with the decussation of the columns to the opposite side. But Mr. Mayo seems to suppose that the palsy shock is communicated to the cerebro-spinal axis about the origin of the seventh nerve, and then proceeds upwards by "reflected extension" to the facial, and downwards to the spinal nerves. Whence he supposes the shock to originate, he has not clearly expressed, and we cannot therefore be answerable for our want of comprehension. "But how is the fact to be accounted for," he continues, "that lesion of one hemisphere of the cerebellum produces palsy of the opposite side?" Here we encounter the same variety of phenomena as results from lesion of the cerebrum, and to which Mr. M. has not alluded. Although we should expect that as the communication of the cerebellum with the spinal cord is direct, the paralysis should be direct also, it is almost always on the side oppo-

* Andral, Cours de Pathologie interne. Tom.iii. pp.75-79.

site to that of the lesion; but that it is not universally so, is stated by Andral* and other pathologists. Mr. Mayo's explanation of the crossing of the palsy stroke, does not appear to us more felicitous than his previous attempts at elucidating this most intricate subject.

"A most complex proximity and intertexture exists between the transverse fibres of the pons varolii and those of the anterior pyramid in their progress through it; a closeness of interlacing nearly resembling that in the spinal cord at the mixture of the filaments of the pyramid with it. Is it too bold an hypothesis to suppose that where there is lesion of the cerebellum, the shock which it originates is capable of affecting through that closeness of interlacing the fibres of the pyramid of the same side, which being thus struck, carrying the palsy influence downwards to the point of decussation?"

On this we have only to remark, that Mr. M. does not assert that there is any continuity of the filaments of the pyramids and *crura cerebelli*, but merely an interlacement; and if simple contiguity is sufficient for the propagation of a palsy stroke, we may bid adieu to all attempts to throw light by anatomical research on the laws of paralytic diseases. With every respect, therefore, for Mr. M.'s ingenuity and pathological knowledge, we cannot see reason for adopting his theory, since it does not seem to us to assist in the comprehension of the complex mass of phenomena which are constantly being presented to our notice. We think that our anatomical knowledge must be much extended and refined before we can pretend to make it a ground of inference; and that, especially, we must gain much additional information on the functions of the cerebrum and cerebellum, and on the minutiae of the connexions of these organs with the spinal column, and the origins of the nerves. And, in the mean time, we are not ashamed to confess our inability to account for phenomena, which the highest authorities in pathological science have deemed it impossible to explain.

We must now quit this topic, and pass on to the chapter on the External Senses, into which a very important and interesting discussion on the nerves of taste has been introduced. Mr. Mayo still retains, in opposition to Panizza, the opinion that the glosso-pharyngeal nerve is not that which ministers to this sense; but that it principally depends on the third branch of the fifth pair. The arguments drawn from the experiments of Panizza are certainly much weakened, if not entirely set aside by Mr. M.'s counter-statements; and he is, in our apprehension, the most correct in his view of the especial seat of the impression. It is well known that Panizza imagines the sense of taste to be most acute where the filaments of the glosso-pharyngeal nerve are most abundant, viz. towards the base of the tongue. Mr. Mayo, on the other hand, maintains that the sense is most acute at the tip and edges of the tongue, a fact we should suppose conformable to general experience; and hence he infers, that as the branches of the glosso-pharyngeal do not reach the tip of the tongue, the fifth must be the nerve which conveys the impression from those parts. From various experiments, he concludes that the former nerve does not act upon the muscles of the tongue; and from some pathological observations, he thinks it probable that it conveys the vague impressions of contact and nausea which are produced on touching the base of the tongue. The question is one which is fully open to fur-

* Op. cit., pp. 84, 85.

ther investigation, especially since the experiments of Mr. Broughton and Dr. M. Hall, communicated to the meeting of the British Association at Bristol, completely accord with those of Panizza, (several facts drawn from comparative anatomy seeming to afford confirmation to their views;) whilst those of Dr. J. Reid laid before the last assembly of the same body, (in which our personal knowledge of that gentleman's skill and accuracy leads us to place great confidence,) go to prove that it is a nerve of common sensation, and that it is by the impressions conveyed by it that the motor nerves of the muscles of deglutition, (principally the pharyngeal branches of the par vagum,) are called into action.

In the other sections of the same chapter which have been added in the present edition, the reader will find much that is interesting on various modifications of common sensation. The sense of effort must certainly be admitted as one of these; but we can scarcely say the same of the sense of motion, which seems but a variety of the former, occasioned by the temporary resisting action of the muscles when the motion which the body is sustaining is altered either in rapidity or direction. In the section devoted to some of the uses of sensation, which is well calculated to bring the student's knowledge into exercise, we find one passage which strikes us as somewhat infelicitous.

"The idea of *outness*, on which our belief in an external world, in place of a world of dreams, is mainly founded, is not excited by every sense. Smelling, tasting, hearing, do not appear to me to excite it; nor motion necessarily: but it is irresistibly conveyed to our minds by sensations of touch, resistance, and vision. In the last instance alone the fact may appear questionable; yet it is sufficiently proved by the remarkable expression of Cheselden's patient, who, when first restored to sight, drew back, thinking that the objects which he saw touched his eyes." (P. 323.)

Now without stopping to criticise the word *outness*, we may ask whether the sensations of touch and vision are not as often and as fully experienced during sleep as those of smell, taste, and hearing; and how, therefore, our belief in the reality of an external world can depend peculiarly upon them? Cheselden's patient had probably not been disturbed with any metaphysical doubts on this knotty question; and nothing could be more natural than that the impressions derived through his new sense, should be at first associated with those by which most of his knowledge of external objects had been previously acquired. An interesting case occurs to us which we shall mention, although not altogether in point. Some years ago, we saw the operation for congenital cataract performed upon an intelligent boy of between four and five years old. For some time after the pupil had completely cleared, he found his way about his father's house and other places to which he had previously been accustomed, more by touch than by sight; but in a new situation the latter sense was of material assistance to him.

Passing by the division of the work appropriated to Voluntary Motion, which contains few additions of importance, we proceed to that which embraces some of the most interesting questions in physiology, namely, those connected with the function of reproduction. Few can be ignorant of the nature of the beautiful researches in embryology, which have recently occupied the attention of our continental brethren, but which, with a very few honorable exceptions, have received no advancement in this country. The tendency of these researches has not only been the

collection of facts, but the establishment of certain general laws derived by induction from them, and opening to us views of the highest interest. In the former edition of Mr. Mayo's work, we were much pleased to observe the attention which he had evidently given to the subject, and the lucid manner in which the materials drawn from numerous sources had been digested and arranged. These excellencies, which it possesses in common with several other parts of the treatise, are not diminished in the chapter as it stands at present, since it has undergone but little alteration; but the progress of this department of science, in the interval, fairly required from Mr. Mayo the recasting of the whole of the section appropriated to it. The extent of the subject forbids us from entering into those details which we have thought it desirable to introduce on other topics; we may, however, specify some of the deficiencies which most forcibly strike us.

It appears to be now established as a general fact, that the fundamental or essential structure of the ovum is the same, not only among vertebrata, but in most, probably all, of the classes of the invertebrata; and that the peculiarities which afterwards characterize the different tribes arise only in the progress of development. The order in which they arise necessarily results from the passage of a more general type into a more special one; thus, in the development of any ovum, the characters of the sub-kingdom are first superadded to those of the animal; the characters of the class appear after those of the sub-kingdom; and so with those of the order, family, genus, species, and sex. The same law of development, that of a special form being elaborated out of one more general, holds good with regard to the evolution of each individual organ; and as this law may also be easily shown to govern the gradual development of each system in ascending the animal scale, it follows that certain transitory resemblances will be presented by each system of the higher animals during its progressive evolution, with the permanent forms of the same system in the inferior classes. Now this being one of the highest generalizations which we can expect to attain on this subject, and being in our opinion firmly established, it would have given us much pleasure had we found that Mr. Mayo had arranged his abundant materials in such a manner as to illustrate and confirm it. With regard to the first point, the essential correspondence in the structure of the ova in all classes of animals, he is altogether silent; he does not even mention the names of Purkinje, Valentin, Wagner, Coste, and Delpsch, to whom (especially to the first three) we are indebted for this beautiful generalization; and we find also that he retains the description of the ovulum of mammalia given by Von Bär, notwithstanding that the researches of Purkinje, Bernhardt, and Valentin, have distinctly proved that the ovulum instead of being analogous to the germinal vesicle of birds, as Von Bär supposed, really corresponds with their whole ovum, and contains a yolk and germinal vesicle.

We must close these remarks by noticing an interesting observation recently made by Mr. Mayo on the character of the globules in pus.

"I have ascertained that the particles of pus bear the following relation to those of the blood. They are exactly of the same shape; that is to say, they resemble in form silkworms' eggs, being circular flattened discs with rounded edges, having a shallow circular depression on each flat surface. They were previously known to be circular

and to be larger than blood-particles. Measuring them, I found their diameter to be $\frac{1}{3000}$ of an inch; while, in the same micrometer, the particles of blood appear to be $\frac{1}{3000}$ of an inch in diameter. Particles of pus are therefore, without doubt, particles of blood slightly changed." (P. 475.)

In the foregoing criticisms, it has been our principal object to direct the attention of our readers to the chief novelties which either are or ought to have been introduced into the present edition of Mr. Mayo's work. We are far from wishing to diminish the estimation in which it is universally held; and we should have taken more notice of the many excellencies by which it is distinguished, did we not suppose them to be already well known to our readers. Indeed, it wants but to be continued with the same zeal and industry with which it was first compiled, to be rendered one of the best elementary works on Physiology, not only in our own but in any language.

V. The last work on our list is, we believe, the only systematic treatise on Physiology that has in recent times appeared on the other side of the Atlantic; and we hail its publication as the harbinger of yet more important contributions to this branch of medical literature from the same quarter. Dr. Dunglison has aimed at nothing more than the selection and arrangement of the facts dispersed through the works of the numerous authors who have written on various branches of physiological science; and he appears to have performed his task with much judgment, his production being distinguished by the same lucidity of thought and clearness of expression, which characterized those which we have formerly noticed with approbation. The student who consults it must not expect, therefore, to meet with comprehensive views or ingenious theories; but he will find it a storehouse of details carefully selected, and judiciously arranged. In this respect it may be considered intermediate between the works of Dr. Bostock and Mr. Mayo; being free from the perplexing profuseness of the one, and having the advantage over the occasional meagreness of the other. The judgment of the author is frequently shown to considerable advantage, in the concise summaries which he appends to the conflicting statements of different observers; the want of which is the principal fault we have to find with the learned system of Dr. Bostock. Were we inclined to criticise the plan of Dr. Dunglison's treatise, we should suggest that the anatomical descriptions which are to be found in every elementary work on the structure of the human body, are somewhat superfluous and add unnecessarily to the bulk of the volumes; although we must admit, that no work on physiology can be complete or even comprehensible which does not embrace many structural details not usually dwelt on with sufficient minuteness in anatomical works, as well as much illustration drawn from comparative anatomy; and for a treatise like Dr. D.'s, which seems intended to be accessible to the unprofessional reader, there is, perhaps, an advantage in combining a general description of the arrangement of the organs with the account of their functional manifestations. If Dr. D. had substituted for these descriptions, however, more copious references to the organization of the other tribes of animals, we cannot but think that he might have rendered his work more generally useful to all classes of his readers. In a treatise on human physiology which considers man merely as an *animal*, it is perhaps cor-

rect to commence with the description of his most purely animal organs and functions, which is the course adopted in the present work ; but when man is regarded as having an *organic* life distinct from his animal life, and supplying the latter with the conditions of its existence, we cannot but think that it is the most philosophical, as well as the most convenient arrangement, to begin with the consideration of those functions which are common to all classes of organized beings. This, however, is a matter of opinion; and almost every systematic writer has shewn that he differs in this matter from his predecessors.

It is obvious, from what we have stated of the plan of Dr. Dunglison's treatise, that the details of its execution afford little food for criticism; and we shall therefore content ourselves with transferring to our pages some of the few novelties most likely to be interesting to the English reader, and making a few extracts which may convey an idea of the general style of the work. The following remarkable case of the effect of the inhalation of nitrous oxide gas is related by Professor Silliman.

"The subject of the case was a man of mature age, and of grave and respectable character. For nearly two years previous to his taking the gas, his health had been very delicate, and his mind frequently gloomy and depressed. This was particularly the case for a day preceding the inhalation, and his general health was such, that he was obliged to almost wholly discontinue his studies, and was about to invoke medical aid. In this state of bodily and mental debility, he inspired about three quarts of nitrous oxide. The consequences were, an astonishing invigoration of the whole system, and the most exquisite perceptions of delight. These were manifested by an uncommon disposition for pleasantry and mirth, and by extraordinary muscular power. The effects of the gas were felt undiminished for at least thirty hours, and, in a greater or less degree, for more than a week. But the most remarkable effect was on the organs of taste. Before taking the gas, he had no peculiar choice of food, but after this, he manifested a taste for sweets only, and for several days ate nothing but sweet cake. This singular taste was, indeed, carried to such an excess, that he used sugar and molasses, not only upon his bread and butter, and lighter food, but upon his meat and vegetables. This he continued to do until the time when Professor Silliman wrote, eight weeks after the inhalation, when he was still found pouring molasses over beef, fish, poultry, potatoes, cabbage, or whatever animal or vegetable food was placed before him. His health and spirits were good, and he attributed the restoration of his strength and mental energy to the influence of the nitrous oxide." (P. 119.)

In the chapter on the physiology of the circulation, we find the following interesting fact regarding the irritability of the heart, which is recorded by Dr. Mitchell of Philadelphia.

"In 1823, being engaged in dissecting a sturgeon—*Acipenser brevirostrum*? its heart was taken out and laid on the ground, and, after a time, having ceased to beat, was inflated with the breath, for the purpose of drying it. Hung up in this state, it began again to move, and continued for ten hours to pulsate regularly, though more and more slowly; and, when last observed in motion, the auricles had become so dry as to rustle when they contracted and dilated. He subsequently repeated the experiment with the heart of a *Testudo serpentaria* or *snapper*, and found it to beat well under the influence of oxygen, hydrogen, carbonic acid, and nitrogen, thrown into it in succession. Water also stimulated it, perhaps more strongly, but made its substance look pale and hydropic, and, in one minute, destroyed action beyond recovery." (P. 148.)

We do not recollect to have seen any case recorded of a similar nature to that contained in the following extract, which we transfer to our pages as being alike interesting to the physiologist and pathologist.

"3. The *excretory ducts*, or *biliary ducts*. These are presumed to arise from acini, communicating, according to some, with the extremities of the vena portæ; according to others, with the radicles of the hepatic artery; whilst others have considered, that the radicles of the hepatic ducts have blind extremities, and that the capillary blood-vessels, which secrete the bile, ramify on them. This last arrangement of the biliary apparatus in the liver was well shown in an interesting pathological case, which fell under the care of Professor Hall, in the Baltimore Infirmary, during the last spring, (1835) and which was examined after death, by Professor Geddings, in the author's presence. The particulars of the case are detailed in the 9th No. of the 'North American Archives of Medical and Surgical Science,' with some interesting remarks by Professor Geddings. In this case, in consequence of cancerous matter obstructing the ductus communis choledochus, the whole excretory apparatus of the liver was enormously distended; the common duct was dilated to the size of the middle finger: at the point where the two branches that form the hepatic duct emerge from the gland, they were large enough to receive the tip of the middle finger; and as they were proportionately dilated to their radicles, in the intimate tissue of the liver, their termination in a blind extremity was clearly exhibited. These blind extremities were closely clustered together, and the ducts, proceeding from them, were seen to converge, and to terminate in the main trunk for the corresponding lobe." (P. 252.)

For the same reason we quote the following case, although we doubt whether the details warrant the inference expressed in the introduction.

"The following case, with which the author has been obligingly favoured by his friend, Dr. Wright, has an instructive bearing upon the subject [nutrition of the fetus.] The condition of the placenta was such as to lead that intelligent observer to conclude, that any circulation between the mother and the fœtus, through the placenta, was impracticable.

"On the 6th of December, 1833, I was requested to visit Mrs. T—, a young woman of large form, good constitution, and generally excellent health. She had been married about fifteen months, and I was now called to attend her first labour. She had felt occasional labour-pains through the day, and was delivered of a fine, vigorous, female infant, in about four hours from the time of my call. The labour was, in all respects, natural, and as easy as is common, or consistent, with a first parturition. After the birth of the child, an hour, perhaps, was passed in waiting for secondary pains to effect the expulsion or favour the removal of the placenta, but no movement of this kind having then occurred, a gentle examination was made to ascertain whether that body might be easily and properly taken away. The vagina contained nothing more than the funis, the outlet of the uterus was open, soft, and extensible. The cord was gently followed into the uterine cavity, and the cake found near its fundus, retaining a close connexion with the uterus. The placental mass was large and firm, presenting to the touch a peculiar feeling, as of a dense sponge, full of coarse, granular, or gravelly particles. Deeming it now proper to relieve the patient fully, a cautious effort was made to detach the placenta from the uterus, in order to its manual extraction. In pursuing this design, it was found, that the adhering surface of the former consisted of a uniform calcareous lamina, or plate, rough to the finger, and exciting such a sensation or feeling as would be caused by a sheet of coarse sand-paper. When the mass was detached, and brought away, the laminar surface just referred to was found to be a calcareous plate, uniformly covering the whole of the attached portion of the cake, the entire surface of the utero-placental connexion. The calcareous matter, thus distributed, was thin and readily friable; but, as before remarked, it appeared to constitute a uniform superficial covering. The correspondent uterine surface, the part from which the placenta had been separated, felt rough, but comparatively soft, imparting nothing distinctly of the calcareous or gritty feel. Out of the body, the placenta felt heavy, and eminently rough throughout. When compressed, or rubbed together, the large amount of nodular or granular matter, dispersed through its substance, was not only manifest to the touch, but a very audible crepitation or grating sound could be thus elicited from any and every part of the mass.

"In this uncommon instance of placental degeneracy, both the mother and child were perfectly healthy and well. The latter, indeed, was remarkable for its fine size, perfect nutrition, and vigour. From the condition of the cake, and the character of its adhesion to the uterus, I apprehended a more than ordinary liability to secondary affection, in the form of puerperal fever; and, whether influenced or not, by the circumstances detailed, secondary fever did ensue on the third day from delivery, attended by the usual signs of puerperal hysteritis, which affection, however, was happily subdued by general and topical bleeding, calomel, &c." (*Note*, p. 409.)

In this very remarkable case, it is certainly a matter of surprise that the fœtus should have attained its healthy size and vigour, which is contrary to the usual result of placental diseases; but it is to be regretted, that Dr. Wright did not ascertain the permeability of the vessels by injection, as the deposit (which, we believe, has been equalled in extent in no recorded instance,*) might have been confined to their interspaces without materially obstructing them.

In closing our notice of Dr. Dunglison's work, we would suggest to its learned author one addition which would much enhance its value; viz. that of references to the authorities from which the facts are derived, especially those which are not usually consulted by the student. We deem it the duty of every one who has traversed such an extensive labyrinth as that which our author has so diligently explored, to leave a sufficient number of direction-posts for the guidance of those that may come after, especially where those hidden and devious paths are to be indicated which might escape the notice of an ordinary treatise.

ART. VII.

A Practical Essay on the History and Treatment of Beriberi, and Observations on some Forms of Rheumatism prevailing in India. By Assistant-Surgeon JOHN GRANT MALCOLMSON, Madras Medical Establishment.—*Madras*, 1835. 8vo. pp. 343, 98.

To these essays was adjudged the prize offered by the Presidency of Fort St. George, of five hundred rupees, or of a gold medal of that value, for each of the best dissertations on the subjects mentioned in the title-page. They are described by the Medical Board of that Presidency as being not less remarkable for laborious research than for original and comprehensive views, and they have very properly been published by order of government.

I. BERIBERI. We are conscious that the title of the first essay is little calculated to win the attention of the European reader; but, although its immediate subject is an endemic of regions so remote as Ceylon and the south-eastern coast of Hindostan, it is only necessary to state that the investigation of this disease has developed facts which have an important practical bearing on numerous intricate affections of the spinal cord, in order to ensure for Mr. Malcolmson's volume that atten-

* It is to be lamented that Dr. Simpson's valuable monograph on Diseases of the Placenta, (*Ed. Med. and Surg. Journ.*, vol. xlv.) has not yet been completed. Few subjects connected with fœtal development are so deserving of study as the influence of morbid states of the placenta.

tion from the profession in all countries to which its merits justly entitle it.

We shall not enter into the philological disquisition on the meaning of the word *Beriberi*. It is sufficient to state that this term is almost universally adopted by Europeans in India to designate a peculiar affection of the lower extremities, chest, and other parts, and which constitutes a very fatal endemic in that country. On the more important points of its prevalence and fatality, we learn that, of eighty-eight deaths which occurred among a body of native troops in 1827, fifty-two were owing to this disease, or five-eighths of the whole mortality, and five times more than the deaths from cholera or fever; and, during another period, whilst twenty deaths occurred from *beriberi*, there were but twelve from fever and four from dysentery. The deaths among cases *returned* as *beriberi* are one in four; but neither the total nor proportionate mortality is easily ascertained, an adherence to the names of Cullen's Nosology causing certain medical officers to designate the disease dropsy or rheumatism, and men who are discharged as cured occasionally dying in villages or garrison hospitals.

Though these circumstances tend to veil the real amount of fatality, enough is shown to prove the disease to be extremely destructive in the districts where it prevails; and Mr. Malcolmson applies himself with praiseworthy diligence to an enquiry into its causes. Like too many investigations of the same nature, however, this terminates unsatisfactorily, and the author candidly admits that, whilst he has demonstrated the inaccuracy of the causes assigned by preceding authorities, he is able to establish nothing in their room. The district in which the endemic chiefly prevails on the continent of India is about four hundred miles of the south-east coast of Hindostan, extending from Ganjam on the north to Masulipatam on the south. This tract is bounded landward by deeply-wooded hills, which are described as being the most destructive to health of any in India, except in the height of the hot weather. These hills approach sometimes within twenty miles of the shore, but in general are upwards of thirty or forty from it; and near Masulipatam, where the disease is comparatively mild, they are at a much greater distance. A description is given, and apparently a very accurate one, of the surface of this district, and likewise of its meteorology; and, from a consideration of all the circumstances, the author deduces the following corollaries:

"There certainly appears to be some connexion between the moisture of the surrounding country and the prevalence of *beriberi*; but much more complete and accurate series of observations are necessary to show to what extent this acts, in how far it is necessarily present, and by combination with what other influences it produces effects so different from those of similar or greater degrees of the same cause." (*Essay on Beriberi*, p. 37.)

—"Beriberi prevails in certain districts within forty miles of the sea, and at no great distance from mountainous forest tracts, and in which the rains commence early, are of considerable violence, and the face of the country much flooded, affecting chiefly adults and strangers from other districts and other parts of the same, who have resided from eight to twenty months in the place. This law will, I have no doubt, require to be greatly modified as our information is extended, but it will be useful at present in directing our enquiries." (*Ibid.*, p. 38.)

The influence of the ordinary causes of fever,—of sea-air and moisture,

—of the water employed as drink, and of the grain used as food, is passed under consideration in the enquiry, but without any decisive conclusion being attained. An intimation is given, however, that the deteriorated quality of the rice and other grains produced in this district may be instrumental in engendering the disease. The author quotes Professor Christison, to show that the diseases of grain which have given rise to epidemics in Europe have been caused by a damp, warm atmosphere, to which a low, moist soil and surrounding woods have contributed. Such influences are in operation in the places where the disease prevails, and, if taken in connexion with the injunction of native practitioners to patients labouring under beriberi to restrict themselves to a diet of wheat, (which is generally brought from the interior,) afford encouragement to the careful observation of the causes to which they seem to point. With respect to contagion, it is not generally supposed to be a cause, but attention is called to the circumstance that, in many instances, members of the same family have fallen victims to beriberi.

The following description will furnish the reader with a general idea of the disease, and certainly, by the place of precedence very correctly given to the paralytic symptoms, tends strongly to confirm the pathological views subsequently developed by the author.

“It usually commences gradually, with a feeling of numbness, sense of weight, and slight weakness and stiffness below the middle of the thighs, sometimes preceded by muscular pains. There is slight œdema of the feet and legs, especially along the tibiæ, often found to come on after the other symptoms. The walk is unsteady and tottering, even when the patient is not aware of weakness in the limbs, which are occasionally tremulous; spasms occur in the calves and soles of the feet, sometimes becoming general, and occasionally shooting to the chest and larynx, obstructing respiration and speech. The want of power often rapidly increases to almost total palsy, especially of the extensor muscles; and in a few cases the patient, after slight indisposition, suddenly loses the use of his legs. Rigidity and various painful affections of the nerves accompany the paralytic symptoms; and there is sometimes pain along the spine, commonly at the two last lumbar vertebræ. In some cases the disease goes no further, and a cure is effected; but more frequently the numbness extends upwards towards the abdomen, there is general sense of lassitude and aversion to motion, and the hands, arms, and chest (and in a few cases even the neck and lips,) are gradually benumbed. There is oppression and weight at præcordia, dyspnœa on slight exertion, diffused and irregular pulsation in the cardiac region, and the face and hands are puffy and œdematous. The patient is often found dead in bed, or sinks after several fainting fits or throbbings at the heart; or the œdema rapidly increases and extends up the trunk, violent dyspnœa and inability to lie down in bed comes on, with anxiety, cold sweats, cold extremities, rapid feeble pulse, urgent thirst, and partial suppression of urine. At the commencement, the urine is always scanty, of a deep red colour, without cloud or sediment, and possessing very peculiar properties; in some old cases, it becomes copious, turbid, and pale, with a large white deposit, and is passed with pain from an irritable bladder. The stomach is irritable in many bad cases, and pain and tenderness in the epigastrium is sometimes complained of; there is, in a few, pain in the abdomen, or a sense of heat is diffused over it and the chest. Effusion takes place into the chest, and more rarely into the abdomen; and there are now and then some signs of inflammation of the pleura or bronchi. In the early stage, the pulse may be full, hard, and frequent, or little altered: when the face is puffy, and there is weight and oppression at the præcordia, it is quick, often irregular, and usually small, although it is occasionally strong.

“Various dyspeptic symptoms occur; the bowels are often costive, the stools green and variously disordered, and the eyes are often tinged yellow. The skin is rather cold, unless there is pyrexia, which is often present in the evening. The disease is

sometimes fatal in a few hours, but is often chronic; and in these the patient is liable to sudden death, to rapid aggravation of the symptoms, or supervention of new and more formidable ones, by which he is soon carried off; and, if he survives these, he may live for a long time bedridden, dropsical, and a true paralytic." (P. 4.)

Mr. Malcolmson gives a detailed and elaborate exposition of the various symptoms, and, in the first place, of those of palsy. This portion of the work will be read with interest and instruction; our abstract of it must of necessity be so brief as to be calculated rather to lead the reader to consult the original, than to allay his curiosity. The author is of opinion that the lower portion of the spinal cord is the seat of the *primary* affection, and that this affection is indicated by an influence on both sets of nerves, sensitive and motory, derived from the *cauda equina*, and supplying the lower limbs. This morbid condition of the spinal cord has unquestionably a tendency to spread upwards, and, when the superior part of the body partakes of the disorder, the different sets of nerves are more unequally influenced; a difference for which we are furnished with the reasonable and ingenious explanation, that in the former instance the nerves are combined into bundles, in which those possessed of different functions must be in general equally affected by disease; and in the latter, where they branch off from distinct roots from the opposite surfaces of the cord, either may partake of diseased action without the other. The affection of the spinal cord, though always existing and essential to the disease, the author believes to be, in the first instance, rather functional than organic. Among the evidences of the accuracy of the pathological views here adopted, we should not omit to mention that, besides the imperfect or disordered action of the nerves of the lower extremities, for the source of which all sound physiology would lead us to look to the inferior portion of the spinal medulla, severe internal pain is felt in the sacro-lumbar region, which, it should be remarked, is not increased by pressure.

The affection of the nerves of sensation, indicative of the lesion of the spinal marrow, is depicted with great minuteness, and, we feel no doubt, fidelity. Numbness affects both sides of the body, but not always equally, and, in the early stages, very generally extends no higher than a little below the middle of the thighs, and, in slight cases, not above half-way up the legs, and, as the disease declines, successively descends to the ankle, instep, and toes; and, when the arms are affected, the skin is usually sensible over the upper two-thirds or half of the forearm, but, in either case, the numbness seldom terminates quite abruptly. The explanation, Mr. Malcolmson remarks, is, in the majority of cases, sufficiently evident by attention to the origin of the nerves and to the fact that, when the functions of a nerve are impaired by artificial pressure or disease, the extreme branches suffer first, and, as the obstruction increases, the larger trunks in succession; and that, on removing the pressure, the functions are restored in a reverse order. If the pressure is made on the cord itself, this is greatly modified by the origins of the different classes of nerves being unequally injured. The injury, for instance, of the roots of the ischiatic nerves, formed of the anterior sacral branches, will fully account for the loss of sensation of the legs; whilst the numerous branches of these nerves distributed on the soles of the feet about the roots of the toes, and on the calves of the legs, will explain

the fact that these parts are more affected than almost any other; whilst the higher origin of the obturator, anterior crural, and cutaneous nerves explains the common termination of numbness one-third above the knee. In one case, the numbness was confined to the lower part of the *legs* and to the *hips*, which derive their cutaneous nerves from the sciatic or sacral plexus.

There is frequently diminished sensibility over the body, when the motory nerves are little affected. This the author believes to arise, not from the posterior column of the spinal marrow alone being injured, but from that general law of the nervous system, according to which the more delicate function of sensation is disordered by slighter causes than the motory. In many examples, however, the distinct nature of the two classes of symptoms is evident: sensation may continue unimpaired when the powers of motion are entirely lost; and instances occur in which entire loss of feeling has extended to the umbilicus, the nipples, and even to the neck, without palsy of the muscles of the trunk. So also, when the diseased action has extended to the upper part of the cord and the medulla oblongata, numbness of the lips, appearing to depend on an affection of the ganglionic branch of the fifth pair, is common in Ceylon, and is not unknown elsewhere; but loss of muscular power of the face has not been observed in beriberi.

Various modifications of sensation are observed in the affected parts, as in diseases of the spine in Europe, particularly pain and a sense of burning, especially of the feet, but occasionally occupying other portions of the body; a subject to which we shall have subsequently to revert. There are some interesting experiments and remarks on the influence of the disease over the actual temperature of the affected parts, for which our limits compel us to refer the reader to the original. On the affections of the muscular system, the author states that additional observations are required. Palsy is the most constantly present of all the muscular symptoms, generally coming on slowly, but now and then very rapidly. It exists in very different degrees, and, in one case reported by Dr. Herklots, amounted to total loss of power of almost every muscle of the body. In the majority of recent cases, spasmodic rigidity is also present; and in a few the flexor muscles were permanently contracted. The cramps are most distressing in the calves of the legs and soles of the feet; and, in one example, the muscles of the back were thrown into such rigid contraction as to give the appearance of opisthotonos.

The following are the proportions in which certain modifications of feeling and affections of the muscles occurred in sixty-five cases.

"Sixty had more or less paralysis, and in the other five it had been prevented by the early use of remedies; in one, almost the whole body was affected, and in some only a finger or toe; 57 had numbness of the feet or hands; in a few, a spot only on different parts of the body was affected, and in others the head and breast were the only places not benumbed; 48 had pain or soreness; 40 œdema; 33 spasms; 11 had the gait of the sheep; 12 tottering in walking; and 24 had sense of weight in limbs or thorax. There was a sense of coldness in the extremities in five; of biting of ants in three; in ten, sense of tingling; and in four, of the feet being covered with clay." (P. 86.)

Mr. Malcolmson proposes his views of the pathology of the disease

with great but laudable diffidence, regarding it as a possible case that the membranes of the medulla are greatly, but not exclusively, involved in the morbid actions on which the symptoms depend. The brain, he remarks, rarely suffers, and the instances that occur do not often appear to be extensions of diseased actions from the spinal canal. When such suffering takes place, it arises from a febrile paroxysm, or from the state of the urinary organs; or it consists of effusion into the cerebral cavities, as a part of the general tendency to serous infiltration; but it is most frequently the effect of obstructed circulation from thoracic disease. From this exemption of the brain from immediate participation in the train of morbid actions, he deduces the inferences that they do not consist in inflammation of the substance of the cord, which usually extends to the brain, or of fluid within the arachnoid lining the dura mater, which would readily pass into the skull, the communication being free. The following is his account of the probable immediate seat of the primary affection.

“The pia mater of the cord (considered by some anatomists as a distinct membrane terminating at the foramen magnum,) is a strong resisting tissue, intimately adherent by its internal surface to the spinal marrow, and continuous laterally with the neurilemma of the vertebral nerves and the ligamentum dentatum. The arachnoid membrane is nearly confounded with its external surface, from which it is reflected to cover the inside of the dura mater, to which it is very closely united, and it forms a *cul de sac* at the extremity of the canal. The dura mater is connected to the vertebræ by very loose cellular membrane, but is so intimately united to the margin of the foramen magnum as to limit the extension of disease upwards, while its morbid states are readily communicated to the other membranes. Its actions are usually slow, (Bichat *Traité des Membranes*, par Husson, 1816, p. 103, 167, &c.) Connecting these anatomical particulars with the symptoms, and with the fact of the disease more rarely extending to the brain than affections of the cord itself or of the arachnoid are stated to do, and with the severe pain experienced in some cases, in the situation of the *cul de sac* formed by the junction of the membranes, it may be safely proposed, as a probable inference, that the sheath of the cord partakes principally in the disorder; and that, through the continuity of the arachnoid, the morbid action is communicated to the proper membrane of the spinal marrow and nerves.” (P. 96.)

These pathological views of Mr. Malcolmson are not left destitute of the support to be derived from the physical lesions discovered after death. The following description of the condition of the medulla in such cases we consider so confirmatory of the doctrine contained in the volume, that we request the reader's particular attention to it.

“The spine was opened from behind, and did not appear to contain any fluid, except what passed from without on opening it, and some slight infiltration of the cellular substance. There was no fluid external to the theca anteriorly, and the contents of the canal, at first view, from the second dorsal to the fourth lumbar vertebra, appeared healthy. At the fourth lumbar vertebra, to the left side posteriorly, there was a small mass of reddish coagulated lymph. This increased towards the sacrum, the cavity of which was nearly filled with a thick mass of firm lymph, evidently organized, of a reddish colour, with some fine vessels and longitudinal and transverse fibres. Towards the coccyx, it passed into the fatty substance. It was not confined to the posterior surface, but passed between the bone and sheath anteriorly, and some way along the sacral nerves, especially those of the left side. It was more firmly adherent to the lining of the canal than to the sheath of the nerves, which in several places appeared healthy below the lymph; in some others, the lining of the canal came away with it; and anteriorly the bone seemed softened, but this was very doubtful. The membranes and nerves did not appear otherwise diseased. Of these

appearances the drawing gives a sufficiently accurate representation. Pursuing the dissection upwards from the second dorsal vertebra to the attachment of the sheath to the occiput anteriorly, a red mass, like a coagulum of blood, of considerable thickness and well defined, was found; which was probably the cellular membrane injected with a bloody fluid. It was confined to the anterior surface of the cord. On opening the theca, no fluid, congestion of the vessels, or disease of the arachnoid, or of the proper membrane or substance of the cord, could be detected. The nerves supplying the lower extremities, and some of the abdominal twigs without the spine, were healthy.—Head: Much bloody serum escapes from the scalp; a very little fluid between the arachnoid and pia mater; convolutions well marked, and the substance of the brain is firm and healthy; a very little water in lateral, none in third, and a little in fourth ventricle. No fluid at the base of the brain, and the cerebellum is healthy.” (P. 110.)

This case is illustrated in the original by an engraving.

It is remarked that there is much ambiguity in the inferences to be drawn from serous effusion between the dura mater and inner membrane of the cord, on account of the free communication which this space has with the cavity of the cranium; but Abercrombie has observed, that this is removed when the fluid in the spine is bloody, while that under the arachnoid of the brain is colourless. These circumstances occurred in the following dissection.

“There was slight serous effusion between the dura and pia mater, without signs of diseased brain. On examining the spinal marrow, there were redness and vascularity of its investing membranes along their whole extent, and slight effusion of reddish coloured serum, with general vascularity of the substance of the cord in the lumbar region.” (P. 116.)

Mr. Malcolmson very properly regards mere vascularity within the spinal canal as a very deceptive appearance. He is of opinion, too, that cases will occur in which alterations of the cord may be so minute as to elude our search, even when existing; and in purely functional cases we cannot, of course, expect to meet them.

Having concluded his remarks on the direct effect of the spinal disease, the author proceeds to consider those affections of distant organs so often associated with it, but which, none of them being universally present, cannot be considered as essential. He takes first in order the morbid changes in the urinary organs and secretion, which appear to have an important influence in the production of the other derangements.

In recent cases, the urine is scanty, of a deep red colour and pellucid. In some cases it is suppressed; but very scanty secretion is the more usual condition, even in the worst cases. There is a difficulty in discovering whether the change in the urine precedes or follows the spinal affection; but the probability is, that the renal disorder is secondary. In Mr. Malcolmson's experiments, the urine possessed powerful acid properties, was of low specific gravity, and was deficient in urea, without generally containing albumen, or, if any was present in any case, it was in insignificant quantity. The acidity was found to be owing to the presence of free phosphoric acid; as, from 1000 parts of urine, 11 grains of phosphate of lime were precipitated by the addition of pure limewater, giving, by the table of equivalents, 5.5 grains of phosphoric acid.

“When the free acid was almost neutralized, the colour of the precipitate changed to a most beautiful pink; purpuric acid appearing to be developed by the limewater precipitating its coloured salt, after precipitation of the free phosphoric acid, which

can be conceived only to take place as a free acid of less affinity. The deep red of the urine is not diminished by the separation of these substances, and is destroyed by nitric acid and boiling, and is altered by sulphuric acid. The urine, kept for two weeks, retained its acidity and red colour; and its smell, though disagreeable, was not urinous or ammoniacal." (P. 136.)

It was remarked that, in rapid cures, effected by medicines which act principally on the nervous system, the urine speedily regained its healthy appearance.

In cases which remain long unrelieved, or which have frequently relapsed, the urine passes into the condition in which it is found in ordinary paraplegia, or injury of the lower part of the spine. It is then copious, sometimes as much as eighty or ninety ounces in twenty-four hours; generally pale in colour, although sometimes reddish, of a specific gravity from 1.020 to 1.025, uncoagulable by heat, and exhaling a strong ammoniacal odour without fetor. This state, which Dr. Prout considers as indicating what he calls the phosphatic diathesis, has been rendered so familiar to the British reader by the labours of this distinguished chemist, and by the melancholy experience furnished by injuries of the spine, that our extracts from this portion of the work will consist less of a general analysis than of any facts or opinions that appear to us to belong especially to the writer. This we mention by way of explaining a degree of abruptness of transition from one subject to another in our brief extracts.

Coinciding with the phosphatic condition of the urine, the author has observed a discharge of what he considered to be ammoniaco-magnesian phosphate from the bowels; and in another case he found the spleen studded externally with calcareous tubercles, and a few superficial tubercles of the same character in the lungs. The secretion of urine is not uniformly copious, but is occasionally greatly diminished; and the consequence has been anasarca, unattended by inflammatory symptoms, and not difficult to remove; and, in a case of this kind, the author would dissent from a precept of Dr. Prout, generally valid, that, in the phosphatic diathesis, we should abstain from diuretics. He regards, in common with all who have witnessed its sequel, this condition of the urine as indicative of great danger; and considers the risk of its occurrence a reason for deviating from the cautious plans inculcated by certain writers on Palsy, of waiting till all chance of excitement is over, before resorting to peculiar stimulants useful in diseases of the nerves; a view in which we concur with him, though admitting that great experience and discrimination are required for its practical application.

The indications of structural change in the urinary organs attending this alteration of secretion are briefly but clearly portrayed. There is weakness in the loins and pain in the region of the kidney, which in one case was observed to occur previous to the earthy deposition, when the change from the acid to the alkaline state of the urine was about to take place. The ureters, in one case, could be felt thickened and tender through the abdominal muscles during life. When the bladder is distended, there is great tenderness in the hypogastrium, increased on pressure; and, when the disease has made much progress, there is fullness and sense of weight in the pubis, the bladder can be felt thickened, and may be pressed between the finger and thumb. These organic

changes render the hope of cure very slight; and, as an illustration, we may state that, of nine cases of this description, Mr. Malcolmson found six fatal, two were lost sight of, and but one was known to be cured. The condition of the kidneys and bladder, as displayed by dissection, is shown to be essentially the same in beriberi and sporadic disease of the spine, by the comparison of cases of each form of disease. An opinion of Dr. Alison's is controverted, that the kidney is affected in consequence of the extension of disease from the bladder. Mr. Malcolmson, on the other hand, thinks that the latter organ suffers partly from the irritating secretion, and also from a similar irritation to that affecting the kidneys, probably communicated along the numerous branches from the anterior sacral and lumbar nerves. That the kidney is previously affected appears from the pain early experienced in its site, and from the facts that lime and acid may be both simultaneously in excess in the urine; and that ammonia, either naturally produced or added, causes the phosphatic deposit only when the phosphatic change in its qualities had previously taken place. The nearly total absence of mucus in the deposit in the early stage also shows that the secretion of the bladder is not, as supposed by Dr. Alison, first increased or altered; and the products of its decomposition are evidently very different from the almost pure carbonate of ammonia extricated from the urine immediately on its being passed. An opinion of Dr. Scudamore's is likewise corrected. This writer supposes that the white deposit is prevented from crystallizing by the admixture of mucus. Mr. Malcolmson remarks, that the phosphate of lime is seldom anything but pulverulent, the crystallized phosphate being the ammoniaco-magnesian compound. The opinion of Brande, that phosphate of lime calculus is formed only in the bladder, is refuted by the plain fact that, in Case II. of the *Essay on Beriberi*, it was found in the pelvis of the kidney. The author's experience of the condition of the urine in beriberi enables him to confirm the doctrine of Prout, that the fusible sediments are deposited in the early stage of the phosphatic diathesis; and, as the symptoms become more severe, the proportion of phosphate of lime to the ammoniaco-magnesian phosphate gradually increases.

Berzelius* and others, observing deposition of the phosphates to be followed by paraplegia, consider the altered action of the kidney as the cause of the loss of power in the lower part of the body. Dr. Prout, too, has remarked that various distressing sensations in the limbs, and even palsy, have been produced by the thickened bladder pressing on the nerves.† In such cases, Mr. Malcolmson would reverse the order of causation, and view the altered secretion and structure of the urinary organs as induced by a spinal affection, which, in its early stage, from the insidious nature of the primary symptoms, has been overlooked. We think this infinitely the more probable and rational view, though we might hesitate before assenting to the proposition "that in *all cases* the phosphatic diathesis arises from affection of the spinal cord." Mr. Malcolmson's experience in beriberi, however, is, so far as it goes, strongly corroborative of its truth.

* See *Medico-Chirurgical Transactions*, vol. iii.

† *Diseases of the Urinary Organs*, p. 253.

The œdema, so important a part of the disease, bears a proportion to the affection of the nerves and the condition of the urinary secretion. In its simplest form it is confined to the lower extremities, occupying a space proportionate to the extent of the numbness, pain, or irregular contractions. In acute cases and when the quantity of urine is much diminished "its progress upwards is exceedingly rapid, and it frequently ascends higher than there is any marked numbness, and as it rises up the chest, water is too frequently effused into the cellular structure of the lungs and rapidly destroys the patient." This œdema of the lungs is a frequent cause of sudden death in the disease; and, as its importance justifies, it receives copious illustration from numerous and well-reported cases. Besides the deficiency of urinary secretion, other causes, especially affection of the heart, and dropsy of the pericardium, conspire to produce it, and it often supervenes very suddenly and unexpectedly when the patient is thought convalescent.

Besides the lungs, the face furnishes an exception to the general law of this disease, that the dropsy is commensurate in extent with the numbness; for it is frequently œdematous when the ganglionic branches of the fifth pair are not affected. Any pectoral disease, as is familiarly known, will cause puffiness of the face, but its actual œdema is with few exceptions dependent on affection of the heart or its envelopes, and especially on dropsy of the pericardium. Our own experience, and we believe that of almost every practitioner who has made cardiac diseases an object of attention, might be adduced in confirmation of the author's opinion. In beriberi this swelling of the face is sometimes the first symptom of dangerous thoracic disease, and, where it is present in the slightest degree, there is the greatest risk of sudden death. This subject, like every other in the volume, is illustrated by valuable cases and dissections, and in the majority of the latter, besides effusion into the pericardium, a fluid state of the blood is observed. The author mentions this condition of the blood as a frequent but not a universal occurrence; and very properly points out the facility which it affords to cadaveric congestions, and the errors into which it might lead the inexperienced in their conclusions respecting morbid appearances.

Whilst the author regards diminished nervous power through its effect on the capillaries as the principal cause of the anasarca, he does not overlook the influence of other agencies in its production. Among these he concurs with Dr. Christison and much earlier writers in classing diminished urinary secretion, though he thinks its effect less than that of the diminished nervous power, the œdema sometimes not being removed by a copious flow of urine of the natural quality, whilst in other instances it is corrected by the treatment which has restored the functions of the nerves, though the urine continues to be scanty and of morbid quality. The form of anasarca which follows exhausting diseases, and which is truly a disease of debility, is not uncommon in beriberi: the legs are much swollen and the emaciation great. It often attacks those who have undergone severe mercurial treatment.

We have already adverted to hydrops pericardii, and from the chapter on the cardiac symptoms it will be found, that, besides this important affection, pericarditis, hypertrophy, and passive dilatation of the heart, and degeneration of its muscular fibre are also associated with beriberi

and are often the cause of its sudden fatality. We pass rapidly over this chapter, because, though it comprises an elaborate description of the symptoms of these affections required to establish the fact that they frequently form a portion of this singular endemic, we do not perceive that it contains much information regarding them likely in the present state of our knowledge to be considered valuable in Europe, beyond the fact of their connexion with beriberi, and the influence this may have on our views respecting the etiology of such disorders occurring under different circumstances. The foundation of those connected with beriberi, Mr. Malcolmson discovers in the doctrine now recognized in pathology of the influence of the nerves on the sanguiferous system, "the fact that continued nervous irritation always tends to produce inflammatory action."

"It is exceedingly probable, (he says,) that the pericardium suffers in this way, the symptoms of cardiac affection seldom appearing when there are not unequivocal symptoms of the part of the spinal cord, above the origin of the nerves which assist in forming the cardiac plexuses being more or less diseased, and in the same patient it may frequently be observed that the lower part of the body may suffer much, and recover from several relapses, but on a return of the disease, in which symptoms of the affection of the spine having extended upwards are apparent, effusion or irritation of the cardiac region carries off the patient. We shall also find that treatment which cannot be supposed to have any sudden effect, except through the nervous system, removes the cardiac affection in a very short period." . . . "The nature of the part exerts a certain influence in producing the diseased actions in question. It (the pericardium) is a compound fibro-serous membrane, and enters readily into the same train of morbid action as other tissues of this class, or of either of those of which it is compounded. If, then, we are right in considering, that the fibro-serous envelopes of the spinal marrow are diseased, it will not appear improbable that similar actions will be readily excited in the pericardium."

The author illustrates this latter view very appropriately by the analogy of rheumatism. In one point we cannot agree with him. He assigns the imperfection of the union between the fibrous and serous portions of the compound membranes, and the feebleness of the sympathies between them, as reasons why pericarditis and many of the complicated phenomena of rheumatism and beriberi are nearly unknown in childhood. Now, at this period of life, pericarditis and ultimately hypertrophy or other disease of the heart itself, associated with rheumatism, is by no means rare in proportion to the number of such subjects attacked with this disease. We have, on the contrary, been very forcibly struck with the number of persons "in early youth," who are affected with rheumatic disease of the heart.

As might be expected, other organs, besides those we have enumerated, became the seat of numerous secondary affections of this multiform disease. Of these not the least interesting are the affections of the larynx. These are strikingly corroborative of the views of Dr. Ley in his essay on *Laryngismus Stridulus*, though from the dates of their respective works, it is impossible that the doctrine of Dr. Ley should have been known to our author, whilst it is highly improbable that the former writer should have seen the *Essay on Beriberi*. Two distinct modifications of laryngeal symptoms are observed in the complaint, aphonia without difficulty of breathing, and spasmodic constriction of the glottis threatening instant suffocation. The branches of the eighth pair are naturally referred

to as the source of these symptoms, the aphonia without difficulty of breathing being ascribed to the condition of the recurrents, the branches of which are distributed to the crico-arytenoideus and thyro-arytenoideus, whose office is to expand the glottis; whilst in spasmodic constriction of this aperture, the superior laryngeal branches are supposed to be affected. If we mistake not Dr. Ley's opinion is that, in the latter case, the paralysed condition of the recurrents contributes to the constriction of the glottis by the withdrawing of the antagonizing power to the influence of the superior laryngeals. But this difference diminishes only slightly the coincidence in sentiment; in each case the superior laryngeals being the supposed active cause of the constriction. It should be borne in mind, moreover, that although the manifest morbid condition, closure of the glottis, exists alike in both diseases, the cause in laryngismus stridulus is pressure on the recurrents from tumefied glands, and in beriberi affection within the spinal canal.

The author refers the abdominal symptoms to the influence of the disease on the eighth pair and sympathetic, finding that when the lumbar nerves are so much injured as even to paralyse the abdominal muscles, there is but little affection of the stomach. The intimate connexion of the eighth pair, through its branches joining the lower cardiac plexus, will lead us to expect that the parts directly supplied by the nerve, will correspond in its morbid actions with the pectoral affections; and similar actions will in many cases be extended through the solar plexus, and the direct inosculation of the sympathetic with the lumbar nerves, to other organs. The stomach is sometimes affected with inflammation, as likewise is the peritoneum of the intestines and general cavity of the abdomen. Of ten dissections there were traces of inflammation with abdominal effusion in five, effusion without inflammation in one, inflammation without effusion in another, and in three there was no abdominal affection. "The bowels are usually slow, and the evacuations are frequently composed of disordered bile, sometimes greenish, at others black, like clay, white or lumpy. The liver frequently suffers, but seldom with severe symptoms. Those most usual are, yellowness of the eye, sallow complexion, and fæces displaying a superabundance of bile.

Referring those who have a prospect of being called on to combat this disease to the work itself, for some valuable remarks on diagnosis and prognosis, we pass to the subject of treatment. After remarking, with much truth, that general principles when applied to unknown complaints are worse than other kinds of empiricism, he proceeds to consider the different remedies employed in beriberi, and in the first place bloodletting. We scarcely need remark that this remedy is not recommended indiscriminately, but according to circumstances. In inflammatory affections of the heart and pericardium, great benefit is, as might be presumed, derived from it; and even in more obscure cases, where inflammation is latent, excepting so far as it is indicated by a scanty secretion of high-coloured urine with deficiency of urea, should the pulse be firm, it may likewise be advantageously employed. In affections of the pericardium where water exists within the unyielding membrane, and restrains the action of the heart, thus producing smallness and weakness of the pulse, bloodletting is well-supported and produces at least temporary relief. Cupping and leeches over the cardiac region are likewise very beneficial.

The author has not found bleeding required for hepatic symptoms; but local bleeding produces benefit in the inflammatory affections of the stomach, kidneys, and bladder. Both general and local bleeding are beneficial when there is much pain of the spine; but the spasmodic symptoms, whether situated in the legs, larynx, or elsewhere, are not relieved by bleeding of any kind.

There are three classes of cases in which bleeding has been uniformly fatal: the first is œdema of the lung which has taken place suddenly, a dangerous condition under any treatment; but by the use of stimulants and antispasmodics, it may for a time be surmounted, though apt to recur in an aggravated form. The next is spasmodic dyspnœa, accompanied with lowness of pulse, cold extremities, and prostration of strength. For this condition, stimulants and warm frictions to the limbs have appeared to be very beneficial. In another class, in which there is a spasmodic, enfeebled, or obstructed action of the heart, with paroxysmal dyspnœa, cold sweats and tendency to syncope, arising from various causes, but in some instances from water in the pericardium with softening of the heart, bloodletting is likewise fatal, whilst purgatives produce pernicious effects. Stimulants, with large doses of laudanum, sulphuric ether, and warm frictions, are the appropriate remedies.

Mercury, the author remarks, has in a few instances aided bloodletting in subduing the inflammation of the membranes. There are, however, exceptions, and his general opinion is adverse to the mineral, as he considers that it possesses no salutary influence over the nervous symptoms, and does not prevent the accession or the return of the visceral affections. The disease has attacked patients when under the influence of mercury for other complaints, and this medicine has been found to aggravate the paralytic symptoms. *Purgatives* are in general very beneficial, and of these Mr. Malcolmson considers compound powder of jalap the best; one or two doses of this medicine, he remarks, frequently removing the œdema of the lower extremities; but when the disease is confirmed, and the phosphatic diathesis prevails, the disordered secretions of the bowels are best evacuated by mild medicines. *Diuretics* are of importance in the removal of the œdema and in preventing its extension upwards; but their utility seems limited to these objects, for they are destitute of any influence over the spinal affection. Squill with *small* quantities of blue pill or calomel, so that the mouth may not be affected, given conjointly with tincture of digitalis and nitrous ether, is a suitable medicine, so likewise is the acetate of potash; but our author thinks cream of tartar the best of the class.

Under certain circumstances, internal stimulants are of use, but we must refer the reader to the original for the specification of these circumstances. Of those employed the root of the moringa tree (*Hyperanthera Moringa*) and preparations of iron, such as the tincture of muriated iron, are spoken of with most approbation. Frictions of the body and limbs with stimulating liniments are recommended, and of the substances employed for this purpose, turpentine, ammonia, and cajaput-oil, have been found most effectual. Blisters, sinapisms, ointment of tartarized antimony, caustic issues, moxas, setons, stimulating frictions to the spine, have all been used. Of blisters and issues he speaks more favorably than of the other means of counter-irritation. Walking, or immersion

of the feet in hot sand at noon is a favorite remedy of the natives, and receives that attention from Mr. Malcolmson which native practices very generally and properly do from our medical officers in India. An hospital for convalescents was formed at Chicacole where this practice was adopted, and, conjointly with the change of climate the inmates were enjoying, it seems to have been very beneficial. Benefit has been derived in a few instances from galvanism transmitted through the spine. Nux vomica did not produce any useful result; and strychnia does not appear to have been tried—perhaps for want of a supply of the medicine.

Treeak Farook, an expression compounded of a corruption of the Greek *θηριακά* and of an Arabic word which may be translated “optima, or præstantissima,” is the name of a remedy of the disease popular among the natives of India. Mr. Malcolmson had some difficulty in learning what this remedy was; but finally discovered that it was the old and very compound medicine, the Theriaca Andromachi, prepared at Venice. It has the power of producing rapid absorption of the effused fluid, and in the opinion of many persons, including our author, its beneficial influence over the palsy has been clearly ascertained, the numbness and peculiar unsteady walk of the patient are diminished by it. Over the affection of the pericardium, and œdema of the face and hands depending on this affection, it has a more prompt influence than over the œdema and numbness of the lower extremities. A smart purge should precede the employment of this medicine, and purgatives are frequently necessary in the course of the treatment. Cream of tartar, too, is sometimes necessary to aid in removing the œdema. The *treeak* has been found most useful in that form of the disease in which there is little excitement, the pulse being usually, but not always, feeble and small. Its virtues seem sometimes to be counteracted by inflammatory affection, but the author does not feel himself able to state the extent to which this counteraction operates. Mr. Geddes, an authority evidently much esteemed by Mr. Malcolmson, ascribes to it the power of lowering the pulse. This is disputed as a general fact; when the pulse was frequent from constitutional irritation of the whole system, no change was caused by the medicine, but when from cardiac affection, especially dropsy of the pericardium, it was restored rapidly to a natural state. It is evidently a medicine of great power over many symptoms. The form of its preparation is a very complex one. In a general view it may be said to consist of many stimulants and tonics, with opium. It seems probable that our Indian practitioners will discover which are the essential ingredients of this singular compound, and accomplish the same amount of good in a less costly and operose manner. We think the subject deserves further investigation, both in the direction we have adverted to, and with reference to the influence which this *farrago*, or its essential ingredients, may have over paralytic and cardiac affections in general, as well as those which constitute a portion of this very singular disease; and, should he have the opportunity, we know no one more likely than Mr. Malcolmson to be successful in such a research. Another medicine, introduced into European practice from that of the natives by Dr. Herklots, received from this gentleman the name of *oleum nigrum*: it is an empyreumatic oil obtained by a rude process of distillation from the seeds of the *Celastrus nutans* combined with a small proportion of benzoin, cloves,

nutmegs, and mace. Mr. Malcolmson thinks this medicine has less power over the œdema and perhaps more over the nervous affections than the treeak farook, and it is through the latter effect only that it removes the hydropic symptoms.

II. RHEUMATISM. (*a.*) *Rheumatism in Natives.* This is described as a very frequent disease, and one which, though not often fatal, is the cause of more men being lost to the military service than all other diseases put together. In some instances it arises from direct exposure to cold and wet, and in character approaches to the disease observed in colder climates, requiring a cautious employment of a treatment similar to that applied to Europeans. In the majority of instances, however, it is a low, chronic form of disease, with little heat in the joints, but little amenable to medicine, and laying the foundation of numerous constitutional ailments. The author gives a very precise and copious enumeration of the parts affected, concluding with the remark that the tissues most generally involved in the disease are the ligamentous and tendinous, the periosteum, and more rarely the muscular, and many others are occasionally engaged. There may be no constitutional affection at the commencement, but, in a large proportion of protracted cases, the patient loses flesh and strength, and a cachectic habit is the consequence.

The indications of this cachectic condition are, loss of strength, emaciation, stooping, languor in motion, pale or puffy countenance, despondency and indolence. The abdomen is puffy or tympanitic, the digestion depraved, the appetite bad, the bowels are costive, and the evacuations dark. The tongue is generally swollen and white. In aggravated cases there is a burning sensation in the abdomen, which is extremely distressing, and probably depends on chronic inflammation of the mucous membranes. This affection proves frequently fatal with symptoms of diarrhœa and dysentery.

The remedies of this *cold* variety of rheumatism, (to use Mr. Malcolmson's expression,) are stimulating frictions to the affected parts, flannels and other warm clothing, blisters, warm baths, Dover's powder, guaiacum, and decoction of the woods. Of colchicum the author speaks slightly, but supposes that its failure may arise from the decay in virtue of the acetous solution with which the military hospitals are supplied. From the reputation enjoyed by a hot sulphurous spring in the bed of the Godavery, and from the remarks of Dr. Bardsley, he is disposed to augur favorably of the effect of warm sulphur-baths on the disease. Of all its remedies, however, mercury is the chief, excepting in cases where pain, numbness, or pricking in the calves of the legs, arising from beriberi or neuralgia co-exist with the disorder, or where there were strumous ulcers or swellings. Even when pains and swellings of the bones and joints could be distinctly traced to mercurial action, or exposure when under such influence, still was the beneficial effect of the mineral marked and sudden. One grain and a half of calomel two or three times a day, combined with three grains of antimonial or James's powder is the mode recommended for administering the medicine.

(*b.*) *Rheumatism in Europeans.* This is the subject less of a formal treatise than of illustrations of important and interesting points in its history. The common acute rheumatism of Europe, as has been already

remarked, is seldom seen in India, and when cases in some degree resembling it occur, they are not attended with that full development of local symptoms and fever, and with the buffy blood observed at home. Among the causes of the disease in general, cold acting on weakly constitutions is a frequent one, though numerous cases, which cannot be traced to any cause, occur during hot weather. In feverish districts rheumatism is very frequent, partly from the moisture and variability of the climate, but often from this disease originating in long continued attacks of intermittent and remittent fever. The rheumatism arising in this way manifests itself in neuralgic pains and inflammation of the periosteum. The author controverts the supposition which might naturally arise in such cases of some venereal disease existing. He finds nodes a common symptom of rheumatism consequent on protracted fevers, and likewise on that which results from the use of mercury. He states from extensive experience, that pains in the limbs, periostitis, and nodes, arise in India from mercurial action for whatever reason it may have been induced. On this ground he applies himself to the correction of an error, into which Dr. Alison had been in some way led, when he stated it as "an ascertained fact" that in tropical climates, symptoms resembling syphilis never result from the use of mercury. Mr. Malcolmson has observed various affections of the skin arise from its use, especially an eruption of inflamed pustules over the body and face; and in one man who had not had any venereal complaint for twelve years, repeated courses of mercury for hepatic affections and fever, and subsequent exposure on a march in the wet season, caused severe nocturnal pains, tumefaction of the tibia, an eruption of round dark-coloured scaly blotches over the body, and very slow ulceration of the throat, of which he died. The author says that rheumatic pains follow immediately the use of mercury, at a time when rheumatism from other causes is almost unknown. The nature of the disease for which it is administered, does not seem to make much difference in the frequency and severity of the pains, as they follow hepatitis, dysentery, and simple membranous inflammation when treated by mercury.

Certain visceral affections are described by the author as the sequel of chronic rheumatism. White diarrhoea is a common symptom in these supervening diseases. Enlargement of the liver, inflammation, ulceration or abrasion of the mucous lining of the intestines, a tubercular condition of the peritoneum, and effusion into the abdominal cavity, are the changes, as we gather from the hospital reports, found in fatal cases.

(c.) *Burning of the Feet*. A considerable portion of the work is dedicated to the consideration of a singular complaint, only observed since the Burmese war, and called from its most urgent symptom "burning of the feet." Some confusion has arisen between this disease and beriberi, of which burning of the feet is an occasional symptom. Only by extensive observation could the question be cleared up; and this Mr. Malcolmson was enabled to effect by observing that on the return of the troops from Ava, in 1826, the corps sent to the northern division were suffering from burning of the feet; but no beriberi showed itself till some months after, when the other complaint had disappeared, and the period of residence and the season exposed them to the endemic of the country,

He considers it a disease totally distinct from beriberi and from rheumatism.

The sensation whence the disease derives its name extends over the surface of the lower extremities which are affected with severe pains, in many cases occupying especially the soles of the feet and calves. In some cases the burning has extended to the hands, and in a few to the whole body and even to the face. Besides the pains and emaciation, symptoms of a generally depressed habit are present. The skin is dry and harsh, often scaly or covered with itch; there is irregular fever; debility; and a tongue, pale, swollen, smooth, or furred, or red when the intestinal mucous membrane is excited. The gums are occasionally swollen and soft, and there is night blindness as in some forms of scurvy. Cough and dyspnoea are not uncommon symptoms, the digestion is in almost every instance impaired. The abdomen is often tympanitic and tender to the touch; diarrhoea, dysentery, pain in the course of the colon or around the umbilicus, frequently occur, and increase the emaciation, or destroy the patient. Dropsical swellings of the legs are common; they are usually the result of debility alone, and disappear on the patient's preserving the recumbent posture; but they are sometimes the effect of organic changes, and complicated with effusion into the cavities. Men labouring under the disease are subject to sloughing ulcers.

The lesions discovered on dissection are of a complicated nature. The lungs are often tuberculated, and fluid is frequently accumulated in considerable quantity in the pleural and pericardial cavities. In the abdomen there is generally observed serous effusion, a thickened and opaque condition of the peritoneum, vascularity and ulceration of the intestines, particularly of the ileum and colon. Fluid is sometimes found in the ventricles of the brain, and sometimes within, and very generally and abundantly around the spinal theca. In some cases softening of the cord has been observed.

The remedies recommended for burning of the feet, are nutritious food and removal from the damp climate in which the disease is engendered; and the author is very urgent that these measures should be adopted before irremediable organic changes occur. He states that he has not seen any disease in Europeans of the same nature as this; but refers to a case of neuralgia in the sciatic nerves and spasms of the legs and thighs, recorded in the London Medical and Surgical Journal for April 1832, as bearing some analogy to it. We cannot vouch for the resemblance between all the parts of two diseases, one only of which we have seen; but the most uniform and distressing symptom of the disease described by the author, the pain and sense of burning in the feet and legs, is a very common accompaniment in this country, especially in females, of those complicated organic changes which indulgence in ardent spirits engenders. So frequently have we observed this affection in female tavern-keepers, that among our professional brethren, merely to avoid circumlocution, we have been in the habit of calling it the public-house disease. The most evident organic changes with which these sensations have been found associated were seated in the liver, stomach, and intestines. The disease we have found almost uniformly fatal. Circumstances, which will be readily understood, have prevented our discovering the condition of the

spinal medulla after death; and we have been obliged to rest satisfied with referring the sensation in the feet and legs to that principle designated by Dr. Marshall Hall "the reflex function of the spinal cord," and suppose that a perception of the diseased condition within the abdomen existed in the nervous centre, and that sympathetic sensation was experienced in the extreme branches of the sciatic nerves.

We thought we should best consult the interests of the reader by presenting him with an analytical rather than a critical review of Mr. Malcolmson's work. The subject treated at the greatest length, that of beriberi, being at least under the point of view in which the author presents it, comparatively new to the European reader, we feared that any direct commentary from us was calculated rather to obscure than illustrate the text; whilst the relation which this disease bears to the spinal affections observed at home, would make a more permanent and practical impression on the mind if discovered by the European practitioner, than if suggested by ourselves. We understand that copies of the work are very scarce, and so conscious are we of the impossibility of giving an adequate view of its valuable contents within the ordinary compass of an article in a journal, that we venture to suggest whether justice to the work, and to the object of its original publication, general utility, does not require that another edition should be published.

ART. VIII.

Ueber Paralyse der Inspirations-Muskeln. Von Dr. LOUIS STROMEYER. —Hanover, 1836.

On Paralysis of the Muscles of Inspiration. By Dr. LOUIS STROMEYER. —Hanover, 1836. 8vo. pp. 144.

IN introducing Dr. Stromeier's work to our readers, we shall, doubtless, recall to the recollection of many of them an intelligent young German who, ten years ago, was assiduously engaged in studying the practice of this country in the London hospitals. That he has not been unprofitably occupied since that time the present work is a sufficient evidence, as are also the better-known and successful efforts which he has made to extend and improve our knowledge of the nature and treatment of club-foot.

The present volume professedly treats of a very common disease, *Curvature of the Spine*, although it will not readily be recognized under the disguise of the new title in which it is here presented to the profession.

Minute investigation of the mechanism of inspiration has led Dr. Stromeier to conclude, that the ordinary form of lateral incurvation is produced by paralysis of the respiratory nerves of the external muscles of inspiration. This is his fundamental proposition, the very groundwork on which all his researches are based; and he has, therefore, chosen to bring it prominently forward in his title-page, in order to attract attention to what he considers as almost alone important in the pathology of the disease. Whatever opinion may be formed of the general soundness of Dr. S.'s views, or of the extent of their application in the care and treatment of lateral curvature, no one who studies them will deny that they

are at once novel and ingenious, and that they must lead to such a reconsideration of the whole subject as cannot fail to be highly beneficial, even if the author's own doctrines should fail to be adopted either wholly or in part. We ourselves by no means assent to all our author's views, and think that he has failed to prove, in a satisfactory manner, the very fundamental proposition of his system. Still, as the doctrines promulgated are of a very original character, are based on anatomy and physiology, and only framed after long and extensive observation of the morbid affections in all their forms, and as the author is a man of probity and great knowledge, and writes in the full conviction that what he advances is of high importance to the medical art, we deem it our duty, to lay before our readers an analytical epitome of the work. In doing so we shall, in the first instance, interpose but few critical comments of our own. At the conclusion of the article, we may perhaps introduce some observations on the general theory or on particular parts of it; but our professed object, on the present occasion, is rather to supply our readers with materials for reflection than to direct their judgment in one particular course. On considering for a moment the very extensive prevalence of lateral curvature of the spine, (or *Scoliosis*, as Dr. Stromeier calls it, applying correctly the ancient term *σκολιωσις*) among the fashionably-educated young women of the present day, and the numerous evils, immediate and remote, of which it is the source, it is hardly possible to estimate too highly any doctrines which offer a prospect of preventing or curing it with greater certainty.

After devoting about thirty pages to the detail of cases, the author reminds us, that the inspiratory muscles possess two distinct and separate functions. The one pertaining to voluntary motion, the other to respiration. According to Sir Charles Bell, those different functions are performed through different sets of nerves. Therefore, in palsy of the respiratory nerves, the motive influence of volition is not necessarily withdrawn.

"The external inspiratory muscles, which have been hitherto believed to cooperate in the instance of accelerated and deep inspiration only, exert nevertheless a continued influence upon the dilatation of the chest. If the sterno-mastoid, trapezius, levator anguli scapulæ, rhomboidei, together with the serratus muscle of one side, no longer conduce to inspiration, in consequence of paralysis of their respiratory nerves, namely, of the accessorius and posterior thoracic; then are the scapula and ribs dragged downwards by the diaphragm, and the circumference of the corresponding side perceptibly lessened; in simultaneous paralysis of all the outward inspiratory muscles, the whole thorax becomes narrowed to the extent sometimes of five inches. It might seem doubtful whether the above-mentioned effect did not depend on the action of the intercostals alone; were it not demonstrable by experiment, that these muscles are of themselves incompetent to maintain the intermediate degree of thoracic dilatation. This dilatation is not the result of sensible contraction alternating with relaxation, but of prolonged organic tension: and is effected, by means of muscles subservient to the constantly shifting positions of the body, and to the incessant wants of respiration. In consequence of the extinction of this organic tension, the diaphragm is found to be much more elevated in the dead body than in expiration during life." (P. 34.)

In reference to the diaphragmatic retraction, there is a conclusive experiment related by Magendie. That physiologist laid open the costal pleura in a young animal, and marked the point to which the diaphragm rose. He then killed the animal by dividing the medulla oblongata, and observed the diaphragm to mount considerably higher. It is possible that

a person during forcible expiration might elevate the diaphragm to much the same height as occurs in the dead body, but such an expiration can only be imagined in conjunction with complete relaxation of the inspiratory muscles. From these and subsequent remarks, the author seems to refer the phenomena in question to some power which maintains the due degree of balance and antagonism of the muscles, and firmness of the limbs; in other words, to that which Dr. M. Hall has termed the *reflex*, or complementary function of the nervous system.

Having enumerated the nerves which supply the principal external muscles of respiration, the author proceeds:

“Concerning the inspiratory nerves which supply the *scaleni*, the *serratus posticus superior*, the *supra* and *infra spinatus*, and the *subscapularis* muscle, we are still in the dark. It is to be hoped, however, that the increasing zeal of experimental physiologists will induce them to investigate the subject, now that its importance is so prominent.” . . . “The configuration of the chest depends entirely on its normal dilatation. Interruption to the uniform respiratory action of both halves will lead sooner to deformity than interruption to the motive influence of volition; inasmuch, as the calls for respiratory action are unceasing, while voluntary motion is only occasionally brought into play.” (P. 36.)

The author impugns Sir C. Bell’s view of the mechanism of expiration, and adopts that of Borelli.*

“Of the respiratory motions inspiration is unquestionably the most important; requiring the greatest effort, it is essentially active; while expiration may be deemed the return to a state of repose. The elasticity and tone belonging to muscular texture suffices for the automatic movement of expiration. But for inspiration, a peculiar set of nerves is indispensable, denominated *respiratory*.” (P. 41.)

In concluding this the introductory chapter, the author observes, that, “since nothing is known of the sympathies of the inspiratory muscles in disease, it is probable that various symptoms have been imputed to the lungs, which are alone explicable on the supposition of functional disturbance in these muscles. Where is the physiologist who has bethought himself of this principle for elucidating the phenomena of fever, of asthma, or of spinal distortion?”

The second chapter is an enquiry concerning the erect position of the human body and the lateral equilibrium of the trunk. The author’s ideas on these matters are peculiarly deserving of notice. Although at first sight paradoxical, they nevertheless repose on principles of sound mechanics.

“In speculating as to the cause of the erect position, and the equilibrium of the trunk, most writers seem impressed with the belief, that the pelvis is the base or fixed point whence the muscles proceed to the spine, like the stays to the mast of a ship. In this way a part of the muscles of the spine certainly do operate. The mechanism for sustaining the column is however more complicated, and inconsistent with such a comparison. The head and cervical vertebræ constitute the basis whence the equilibrium of the chest along with the spinal column may be preserved. This is accomplished by means of the muscles of inspiration attached to the occiput and vertebræ of the neck; on the other hand, the expiratory muscles derive the great measure of their strength from the pelvis. To comprehend how the trunk of the body forms a

* “In placid and natural expiration the air is expelled from the lungs, not by the motive power of any muscles, but in virtue of the cessation of action in the intercostals and diaphragm, with simultaneous dilatation of the *rima glottidis*.” Borelli *de Motu Animalium*. Part II, prop. xcii. Hagæ, 1743.

strong but moveable shaft or radius, the head being the fixed point or centre, it will be convenient to contrast the arrangement of parts with a mechanism occasionally employed in the construction of bridges. It is thus that a long slender ladder or other similar object can be considerably strengthened by fastening to both ends cords firmly stretched and held apart at intervals by *straining-pieces*; an artifice resorted to in time of war, for mural-ladders required to bear the burthen of a number of men at one time; or for weak rafters laid across a stream, to supply a temporary bridge. The extremities of the ladder, or plank, constitute two fixed points whence the cords are stretched out, and by the equable distribution of the force of sustentation over the several points of disjunction, equilibrium is assured. The spine may be said to represent the weak ladder, the muscles the cords rendered tense by organic tension, and the alternate acts of inspiration and expiration; the strain being effected superiorly by the ribs and lungs, inferiorly by the abdominal viscera and their gaseous contents. According to this view, the intermediate state of activity of the collective muscles of the trunk upholds the erect position, and at the same time keeps the shoulder and ribs at their natural elevation. Towards inspiratory motion, the having a fixed point in the head is requisite; indeed the nature of the costal articulations upon which hinges the elevation of the thorax implies as much, because the chest does not advance upon the first rib as was formerly asserted, but upon the head and cervical vertebræ. Magendie has shewn the first rib to be as susceptible of motion as the other, provided its greater shortness be taken into account. It visibly rises at each inspiration. The inspiratory muscles tend to amplify the thorax in width, so that the direction backwards and upwards is always the preponderant; as is indeed manifest from the course of the muscular fibres. In any position of the body, provided the inspiratory muscles possess their due tone, they will effect this peripheral dilatation of the chest." (P. 44.)

Dr. Stromeier points attention to the well-known circumstance that exciting passions (as joy) dilate, while those of an opposite description constrict the cavity of the chest, even to sense of suffocation; that during violent paroxysms of hysterical dyspnœa, the patient can draw a deep breath at will; proving, that neither the influence of volition, nor the action of the diaphragm or lungs is at fault; but that there is an interruption in the supply of nervous energy to the inspiratory muscles; and that, in like manner, public singers have suddenly lost their voice in certain complaints, and only recovered it after the use of gymnastic exercises. In such cases, it is not hoarseness but a want of power and intonation. There fails that resonance so essential to full vocal development, the result of a well expanded chest.

The author accounts as follows for the variation in the corporeal height between the morning and evening.

"Heretofore the diminution observed towards the close of day has been attributed to a compression of the intervertebral substance, which subsides with nocturnal repose. To expose the fallacy of such a mode of explanation, subject the lumbar spine to powerful pressure, taking measures at the same time to prevent lateral yielding, and no appreciable shortening will ensue. On the other hand, let this experiment be performed: Measure with exactitude the height of an individual standing upright; this done, bid him try to elongate himself without letting the soles of his feet quit the ground. Measure him now, and you will find him taller by a quarter of an inch or more. In the effort thus made, he merely diminishes the natural inflexion of the spine. The evening waning of the vital powers produces the opposite result." (P. 49.)

The next section is devoted to the consideration and (as Dr. S. believes) refutation of the currently received opinions concerning the causes of lateral incurvation, or Scoliosis. According to the author, writers on

this subject inconsequently ascribe the deformity to an affection of the several muscles of the neck and trunk; while they contend, at the same time, that the constitutional derangements usually concomitant must operate on all the muscles.

“They did not take the pains to study individual muscles relatively to their function, so as to deduce therefrom the share each had in sustaining the healthy configuration of the body. Even skilful practitioners, as John Shaw for instance, peremptorily assert that the multiplied anatomical subdivisions of the dorsal muscles are so many integral parts of the same whole, conspiring to one and the same end. . . . Hence the consideration of curvature assumes quite another aspect, and acquires new interest, when directed to those particular muscles, the defective energy of which has first disturbed the general equilibrium. That the primary and secondary forms of incurvation are to be carefully discriminated, and that the organs of motion are not to be contemplated in a mass, is therefore obvious. . . . To detect the peccant muscles, it is advisable, first of all, to select and set apart those of the trunk, which, from their modes of origin and insertion, cannot participate in the initial development of lateral incurvation. The latissimus dorsi, being distributed over the greater part of the back, its individual layers implicate but a small part of the spine; it can only, therefore, when acting partially, transform the dorsal and lumbar vertebræ into one large curve, the convexity of which would be towards the side of the impotent muscle. The longissimus dorsi and sacro-lumbalis are likewise insufficient to induce scoliosis; they influence less the lateral equipoise than the antagonism of the flexors of the trunk. . . . That paralysis of the longissimus dorsi and sacro-lumbalis produces incurvation anteriorly (Lordosis,) when double, and in addition lateral incurvation when single, has been tolerably well established. To the ordinary forms of lateral curvature, however, these deviations bear no similitude. . . . The greater number of small fleshy bellies of the spinales and semi-spinales dorsi, multifidus spinæ, levatores costarum, interspinales, and intertransversales are also inadequate to produce curvature. They must be partially palsied on one side to determine that. Nor is it easy to conceive simultaneous paralysis of a number of muscles furnished with such different sets of nerves; yet, in order to illustrate the small serpentine curves of the spine, of which the arcs include but one or two vertebræ, it is needful to take the above muscles into account.” (P. 51.)

That the abdominal muscles exert no real influence on the origin of lateral incurvation, may be inferred from the fact of their preserving their elasticity and tone, even under a high degree of this affection. Wherefore, after excluding so many muscles of the trunk, it will be seen that the few remaining are chiefly those concerned in the act of inspiration; and of these the author has instituted an elaborate analysis, of which we subjoin an abstract.

“1st. The sterno-cleido-mastoideus. Its influence on cervical incurvation has been long acknowledged, and by many intelligent writers held to be the only source. But the causes of that affect are manifold. That this muscle has no share in the origin of scoliosis cannot be reasonably imagined; indeed, the result of numerous cases prove the contrary. Where the head, for instance, acquires a bias, it is owing to the partial action of this muscle, so that it is turned in one direction, and the chin in an opposite. This is a condition not seldom met with, and which frequently is the only thing that awakens the attention of bystanders; the scoliosis meanwhile passing unheeded.

“2d. The trapezius, levator anguli scapulæ, and the rhomboidei. Of these muscles, the first is the one most commonly engaged, and that along with the mastoid; a circumstance plainly denoting the ground of its spasmodic or paralytic state to be referrible to the accessory, or nerve common to both. It sways the head principally, less the scapula. This is explainable upon the circumstance that the levator anguli scapulæ, and the rhomboidei, receive fewer branches of the above nerve; and thus the

normal position of the shoulder-blade is preserved. Not long ago, I witnessed an instance of congenital paralysis of the left acessorius, in a delicate girl of three years old.

"3d. The scaleni. That these muscles contribute to the origin and development of scoliosis, we are led to conclude from those rare cases in which, together with considerable sinking-in of one side, the upper rib remains *in situ*, and the shoulders exhibit a slight inequality in height. This state of things is tolerably constant in scoliosis induced by empyema, cured by evacuation outwards. The depression of the shoulder bears no relation to the sinking-in of the chest. . . . It is a matter of regret that Sir C. Bell has not directed his attention to the important function of the scaleni muscles. Concerning their respiratory nerves we know nothing. As they, however, receive branches of the cervical nerves from the third downwards, (Bock,) we may ultimately arrive at the truth, although difficult experimentally.

"4th. The serratus magnus. No muscle appears to me so much interested in the formation of scoliosis as this one. . . . The lack of action of the inspiratory muscles in the so-called rhachitic curvature of the thorax, has not escaped observation. But the effect has been mistaken for the cause; inasmuch as it has been referred to the faulty direction of the ribs. For, should the ribs be depressed, they are nevertheless in a condition to be restored, provided the inspiratory muscles have sufficient power. On the other hand, if we suppose the impoverished energy of the serratus to be the cause, not the effect, of the phenomenon in question, we at once obtain a solution of all the symptoms. . . . The office of the serrati muscles is to keep the ribs stretched backwards, outwards, and somewhat upwards. Hence the diaphragm pulls the ribs inwards when the action of the serrati fails. This can be best seen during the tranquil respiration of delicate children. But the want of action is most conspicuous when we confine the movements of the diaphragm by firmly pressing on the belly, and thus bring the external inspiratory muscles into full play. Then the pectorales, trapezius, &c. act vigorously, whilst the serratus remains still; so that the capacity of the thorax is not augmented in breadth, but its sides are drawn inwards by the diaphragm. Again, on the subsidence of the action of the serratus, the diaphragm drags the ribs of the corresponding side inwards and downwards: hence the resulting concavity on one side of the chest, commencing immediately below the axilla. It is not always, at an early stage, associated with depression of the shoulder, because the upper digitations of the serratus pursuing a course from above downwards, the superior ribs are not necessarily dragged with it. . . . The respiratory motions of the several inspiratory muscles of one side are so intimately united, that any imperfection in one is followed by a corresponding defect in the rest. For this reason, the impaired energy of the serratus must affect concurrently the trapezius. Hence the subsequent falling of the shoulder, the lateral deflection of the vertebræ; the longissimus being no longer able to sustain the lateral equilibrium. The disproportionate balance of the pectoral extremity now tends to increase the curve; yet this is of little moment, independently of other co-operative circumstances; such as the progressive affection of the inspiratory muscles, and the incidental consent of parts. This is evidenced from the fact that the symptoms do not get worse after the equipoise is lost, in the case of a contracted limb, or after empyema." (P. 59.)

Dr. Stromeier thinks that, by referring the ordinary form of incurvation to one-sided paralysis of the serratus magnus, you are enabled to account for its most usual form, namely, that in which the thoracic vertebræ form one curve, and those of the neck and loins two others, for the purpose, as it were, of preserving the equilibrium. This *vis conservatrix* is the consequence of the involuntary efforts made by muscles still retaining their integrity of action; namely, the longissimus dorsi, sacro-lumbalis, and multifidus spinæ. It, moreover, coincides with the fact that healthy muscles gradually accommodate themselves to faulty positions of the skeleton, as is instanced in cases of unreduced luxation

and ill-united fracture, where, in the course of time, the functions of the limb are partially restored; and, as is strikingly illustrated, in the very disease under consideration, in the dissections of Dr. Gunther, recorded in our last Number.*

The author has started some queries relative to thoracic rhachitis; as, whether the serrati are not always primarily affected, thereby furnishing one of the first elements of the serious constitutional malady in question, and one most likely to induce rapid destruction of the system, by its prejudicial influence on the function of respiration? or, in other words, whether chicken-breast does not, in every case, precede rhachitic incurvation? The frequent occurrence of chicken-breast without trace of diseased bony structure in the extremities, must at all events render it probable that the change in the form of the chest ought to be ascribed to something else than softening of the skeleton. This is a very important subject for further enquiry. If the question can be answered generally in the affirmative, and there be no accompanying softening of the skeleton, Dr. S. will have a strong argument in favour of his theory. In several cases which have lately come under our own notice, we found, on enquiry, that the chicken-breast was the first symptom that had attracted attention; and our former experience leads us to a like conclusion.

Dr. Stromeyer does not conceive the serratus magnus to be more liable to paralysis than any other muscle of inspiration; but contends its chief function to be respiratory. "The voluntary motion of the serratus appears highly problematical. After repeated examinations in muscular men, I am led to believe that, in the motions of the shoulder-blade, the serratus is only passive. We cannot rely on anatomists in such matters."

By pressure upon the abdomen, we impede the action of the diaphragm, and thus solicit the involuntary action of the external inspiratory muscles. In this manner we can, in every instance of lateral incurvation of the spine, demonstrate the paralysis of the serratus on the concave side of the curve. In voluntary deep inspiration, that muscle passively follows the ascension of the scapula. The author has experimentally proved the nervus thoracicus posterior to be its *energizer*, as conjectured by Bell. The intercostal muscles, according to Dr. Stromeyer, ensure a uniformly diffused action of the other inspiratory muscles over all the chest; more especially of those which, like the scaleni and sterno-mastoid, are affixed to small points remote from the inferior ribs. They are inadequate alone to keep up the dilatation of the chest. But those limited inflections of the ribs, sometimes found distinct from other curvatures, and situated near the sternum, he considers to be the effect of the disproportional action of these muscles. To their paralysis we must assign the collapse of the ribs observable in rickety subjects. The pectoral muscles, destined for preserving the arched form of the chest, when not counterbalanced by the action of the serrati, occasion chicken-breast. When deficient in energy, the thorax assumes a flat form, increasing in lateral diameter; and the diaphragm and triangularis sterni drag the sternum inwards. The supra-spinatus, and infra-spinatus, and subscapularis seem set apart for communicating tension to the pectoral muscles. The diaphragm participates in the formation of this disease, inasmuch as

* British and Foreign Medical Review, Vol. IV. p. 509.

it influences concurrently that portion of the chest, of which the external inspiratory muscles are enfeebled. The widening of the thorax in the axis of its long diameter, with simultaneous ascension of the abdominal viscera into its cavity, seems to be explicable only on the ground of diminished tone of that muscle.

The quadratus lumborum is principally engaged in upholding the action of the *psœ*; it preserves the natural anterior inflection of the lumbar spine, and tends on both sides to sustain the balance of equilibrium. The author is in doubt whether its affections be not coexistent with those of the *psœ*, or whether it be spontaneously capable of originating spinal disease. The *psœ*, from their mode of insertion, are peculiarly adapted for preserving the corporeal equipoise; and, consequently, through partial paralysis of their involuntary power, for aiding in the development of curvature.

"The *psœ* are extremely complex in their actions. Hitherto they have been regarded as flexors of the thigh, and of the upper part of the body when the thigh is fixed. They, however, are further intended to maintain the natural flexure of the lumbar vertebræ; not only necessary for the sake of equilibration, but likewise for relaxing the great expiratory muscles, and thus promoting the ascent of the chest during inspiration. In proof thereof, let a person, while standing voluntarily, depress the vertebræ of the loins, and attempt a full inspiration. Then will he experience, along the entire back, as far as the arm, the resistance offered by the *latissimus* and *longissimus dorsi* to the elevation of the ribs. Hence it is in a measure true that, proportionally as the back is bent with age, have the *psœ* lost their organic tone, and is the breathing embarrassed. That these muscles can spontaneously determine these inflections of the lumbar vertebræ, is moreover established by the fact that the movement alluded to is not possible when the thighs are held closely approximated." (P. 86.)

The author has drawn some curious practical deductions from the study of this function of the *psœ*, and endeavours to explain by its means the circumstance of the retraction inwards of the lumbar region during the acts of station and progression in congenital luxation of the hip-joint. He also explains, by its paralysis, the phenomena of what is commonly termed "the high hip;" but we cannot stop to notice the particulars.

In the fifth section of the work, the subject of permanent muscular contraction (*contractura*) is particularly discussed; a morbid condition which may be produced in one or other of the following ways:

"1st. By organic changes in the muscular texture itself, in consequence of inflammation, wounds with loss of substance, and the like.

"2d. By weakness and defective action in the antagonist muscles. This may proceed from—*a.* Wounds of the antagonist muscular bellies, or their tendons; *b.* Paralysis of the nerves of the antagonist muscles."

This paralysis may be complete or incomplete, and involve either the voluntary or the organic motions of a muscular part. Complete paralysis arises, for example, from division of a nerve, as the facial, producing contraction in the opposite muscles. Incomplete paralysis occurs, for instance, in the mastoid muscle. Here the influence of volition no longer serves to restore the bent head to its vertical position in many cases. It is further observable in certain people who involuntarily squint, but cease to do so when they direct attention to the circumstance. Here the paralysis of one muscle, which led to the contraction of its antagonists, is merely temporary, and removable at will. The motor nerves of

volition are in a state of perfect integrity; those of organic movement are alone palsied.

“3d. By diminished influence of all the voluntary motor nerves of an extremity.”

In reference to the prognosis of palsy generally, the presence of muscular contraction is a favorable indication; since the persisting tone and elasticity of the muscles prove the persistence of the *reflex-motory* power. Thus, as Dr. Stromeyer states, children labouring under paralysis of the lower extremities, the sequence of congenital hydrocephalus or hydrorachitis, generally recover the dormant functions after the tenth year of their age.

“4th. By the painful condition of a part; whereby its voluntary movements are crippled, as in articular inflammation. This resembles the preceding state, where the abstraction of the influence of the will is referrible to the nerves. The more powerful masses of flexor muscles equally preponderate over the extensors. The process of inflammation occurring in muscles, tendons, or the intervening spaces, often associated herewith, induces certain organic changes, which, after resolution of the inflammation in the joint, lead to contraction and false ankylosis.

“5th. By augmented power in a muscle. This is only possible during spasmodic action, where muscular motion is preternaturally increased; because increase of power is not compatible with prolonged repose. A muscle contracted for a length of time becomes progressively useless and atrophied.” (P. 79.)

It is well known that, in a high degree of lateral incurvation, the face presents a peculiar relaxed look, and is often awry. The following observations on this condition are interesting:

“Medical practitioners regard this appearance as the sure criterion of a congenital and incurable disease. It is in reality not so. The distortion of the countenance is so far from being an unfavorable sign, that I have always known it to yield sooner than the curvature itself, when the respiratory energy of the muscles had been restored by judicious gymnastic treatment. Nay, the expression of the features visibly improves under the above plan, when the incurvation remains unaltered. . . . This phenomenon is intimately related with spinal curvature; and that side of the face uniformly wastes away whither the head inclines. . . . It is attributable to the interruption which the respiratory action of the cervical muscles has undergone. Their points of attachment being unduly approximated, they are incapacitated from exercising organic tension. Let it be understood that we here include not only the sterno-mastoid, but also the muscles of the upper part of the throat.” (P. 105.)

The author adverts to a fact heretofore overlooked, namely, that the same predominant weakness of the left side usually recognized in curvature may be also present in the face; limited to its respiratory nerve, the facial: and, besides, “if we recollect the close union subsisting betwixt the brachial plexus and the nerves of inspiration, as the phrenic, thoracic posterior, anterior, &c., we shall perceive that the less habitual use of the left arm must have a debilitating effect on the respiratory energy of that side, comprehending the neck and face.”

According to Dr. Stromeyer, particular attitudes and postures of the body can have little, if any, control over the development of spinal incurvation. He, therefore, inveighs against the various contrivances,—such as straps, irons, and other *instruments of distress*,—which act by severe extension and pressure. These have been recommended by men whose skill in the application of mechanical principles has been generally commensurate with their ignorance of sound physiology; yet, however much importance he may attach to the efficiency of partial paralysis of

the inspiratory muscles in determining lateral incurvation, he does not assign to it an exclusive prerogative.

"Far be it from me to undervalue the concurrence of other constitutional causes in the development of scoliosis; for I am convinced that a certain debility in the osseous system is required, in order that the irregular degree of stimulation, or (if we may use the phraseology) the incomplete and partial paralysis of the nerves of the respiratory muscles, may induce a permanent impression on the conformation of the body. Hence incurvation is a disease incident to the period of youth, rarely occurring in adult life. The perturbations to which the inspiratory muscles are obnoxious, after the skeleton has attained its mature growth, engender other affections,—as asthma, dyspnoea, and the like; which, transient or periodical in their character, cannot modify to any extent the compact bony framework. But this susceptibility to change of form, so remarkable in childhood, is not without its advantages. Thus, it exerts a sanatory influence in the event of external injuries, as falls, and in the case of empyema that has opened outwardly; recovery at that age being comparatively rapid, from the suppleness of the ribs. As respects the prompt formation of curvature, the degree of softness in the skeleton is very influential. Thus, in infants, that disease makes as much progress in the course of a few weeks as it will in as many years at a subsequent period of adolescence." (P. 114.)

Perhaps, one of the most frequent sources of paralysis of the muscles of inspiration is a long-continued cough, especially hooping-cough, in consequence of the oft-renewed powerful efforts made by these muscles when their antagonists are in a state of quiescence. That general rha-chitis is also often associated with spinal curvature, and that there is frequently a transition of one disease into the other, (individuals, for example, who, in their early years have suffered from rickets, become towards puberty the subjects of incurvation,) are incontestable facts, and as such have been noticed by the author.

We shall conclude our analysis of Dr. Stromeier's treatise with a brief abstract of his principles of treatment, referring to the original for fuller details.

"The theory that lateral incurvation is an affection of the respiratory nerves, unlike that proposed by Delpech, is in perfect accord with the commonly received therapeutic indications. . . . It is, indeed, interesting to reflect how much the powers of truth could, in the mind of an eminent observer, overbalance perverted pathological hypothesis; since, whilst Delpech attributed curvature, for the most part, to inflammation of the intervertebral substance, he recommended a mode of treatment diametrically opposed to his etiology of the disease. . . . The rational treatment of spinal curvature consists in improving the functions of assimilation and nutrition, and placing the body in such positions as shall be best fitted for animating the organic energy of the inspiratory muscles. These principles of cure suffice for the majority of cases; for the number is very small indeed in which the local disease is completely unconnected with constitutional causes. . . . The utility of gymnastic exercise is therefore proportional to its capability of meliorating the functions of assimilation and nutrition; and is in general so remarkable, that the exhibition of internal medicines is superseded, digestion being in a few weeks so much bettered, that the appetite is doubled, and all the functions restored to their healthy condition." (P. 131.)

Dr. Stromeier has ascertained, in corroboration of his views, that well-directed gymnastic training communicates tone in an especial manner to the muscles of inspiration.

"It will be seen, in any orthopedic establishment, that not only do the muscles of the arm become larger and firmer, but the thorax often, in the course of a few months, gains several inches in girth. This is easily explained when we reflect that most gymnastic exercises are conjoined with suspension by the hands. Expansion of

the chest naturally results from the increased vigour imparted to the inspiratory muscles; and manual suspension is the only means of ensuring the full action of the serratus magnus, as it compels it to support part of the bodily weight. Sometimes, it is true, in inveterate scoliosis, many months may elapse ere the serratus of the concave side of the curve will resume its activity; although, while the patient is hanging by the hands, the concavity disappears from the elevation of the ribs. It is absurd to believe that such exercises can only avail when the inflexion is situated below the level of the shoulder; because, as paralysis of the serratus becomes sooner or later coupled with atony of the trapezius, the scaleni, and the remaining muscles of inspiration, restoration of its functions will actuate consentaneously these others. . . .

"Diversified exercises, performed in the company of others, are alone advantageous; partly because a considerable time may be so devoted to them, and partly because, when so conducted, they cease to be irksome, and young females find them really pleasurable. Balance of power and bilateral symmetry are the result." (P. 133.)

The author countenances, under certain limitations, the employment of gentle mechanical means,

"On the one hand, because the secondary phenomena of scoliosis, as the shortening of particular muscles, stiffness and rigidity in the costal and vertebral articulations, require the intervention of mechanical extension, so as to fit these again for service; on the other, because moderate extension, especially of an elastic kind, acts as a stimulant to the lax muscular fibre. In proof of this, we need only look to the augmented bulk the calf of the leg acquires when club-foot is treated by extensional apparatus." (P. 135.)

He recommends the *stretching-bed*, as he terms it; and chiefly for the reason that, when the patient is placed either horizontally or upon an inclined plane, a very slight spring-force suffices for maintaining the adequate pitch of tension, inasmuch as the resistance from the weight of the body is then null. He is very far, however, from countenancing that extreme degree of mechanical extension, which, when combined, as it usually is, with almost total inactivity of the body, we see productive of such lamentable results in the practice of some persons in this country.

"It is a vulgar and a dangerous mistake to suppose that, by powerful apparatus and forcible traction, you can speedily subdue spinal incurvation. Undoubtedly, if you stretch the patient on his back, you straighten the spine; but, by repetition of this racking process, his general health becomes impaired, his muscles enfeebled, his cartilages loosened, and, upon his resuming the vertical position, the original distortion is again present, and sometimes in a more aggravated form than before." (P. 136.)

The method of treatment where confinement to the lateral position is advocated, by which the superior half of the chest is elevated, and its external inspiratory muscles brought into full activity, leads the author to comment upon the views of MM. Pravaz and Guérin, who have adopted this judicious practice, despite their false reasoning concerning the causes of curvature.

"The patients are obliged to lie in a kind of hammock, upon the convex side of the curve; of which side, by different movements, the energy of the muscles is meanwhile quickened. Future experience can only decide on the value of this plan; at all events the posture is comfortable. . . . It is desirable to conjoin some sort of mental occupation with it. Of course, it is improper for those cases in which the palsy of the inspiratory muscles is not strictly one-sided: for these the supine position is decidedly preferable, as a slight degree of extension can be advantageously conjoined, and free play allowed to both lungs." (P. 137.)

Dr. Stromeyer conceives the views he has unfolded will materially facilitate the early diagnosis of such affections.

“By studying attentively the external inspiratory motions, we shall not only be enabled to distinguish, with precision, slight curves, but likewise to predict and counteract their supervention. Since paralysis of the inspiratory muscles does not immediately produce change of form, a considerable interval may elapse before the latter criterion is appreciable. Therefore, the possibility of preventing distortion by simple measures at the commencement ought to awaken public attention on this head.”

For the purpose of investigating the respiratory movements of the external muscles of inspiration, we ought, as formerly mentioned, to make pressure upon the abdomen, according to the mode of examination introduced by Bichat, in reference to another object.

“Delicate children are best suited for learning this plan at first. As with them the want of atmospheric air is great, thoracic respiration is produced so soon as the abdomen is compressed; and, in consequence of the extreme mobility of the joints and cartilages of their ribs, the motions of the latter are more extensive and more palpable than in older subjects. Hence, in them, it is easy to trace the defective elevation and imperfect lateral expansion of the concave side of the chest, depending on scoliosis. The reason why palsy of the serratus muscle cannot be detected in voluntary deep inspiration, both sides of the thorax rising equably, may be understood when we reflect that the diaphragm, by distending the lungs with air and compressing the abdominal viscera, facilitates the elevation of the thorax. Then, only a slight consentaneous effort of the inspirators is requisite; whereas, on compressing the abdomen, the motion of the diaphragm is constrained, so that the task of filling the lungs devolves upon the external inspiratory muscles, which those of the concave side are no longer competent to fulfil. In a large amount of cases of scoliosis, in individuals up to the age of eighteen years, recently examined by me, I have found this relation constant: it failed, on the other hand, in those instances where an amendment had been previously wrought by a course of gymnastic exercise.” (P. 141.)

The various topical remedies which seem to act by stimulating the nerves of the part, promoting the circulation of blood, and consequently favoring the increase of muscular size and strength,—as friction, sham-pooing, and the like,—ought, according to the author, to be applied to the concave, and not the convex side, as commonly prescribed.

“Of the beneficial effects of stimulant liniments (Liq. Ammonia 3j. c. Sp. Vini rect. 3vij.) to the region of the serratus muscle, we have ample evidence from the pressio abdominalis test, tried before and after. That such effects will not be lasting unless the function of nutrition be at the same time exalted, may be reasonably inferred. Therefore, no great reliance is to be placed on the use of external treatment alone. In the instance of *sinuous* incurvation, the stimulant frictions are to be made over all the outward muscles of inspiration.” (P. 143.)

In concluding this brief notice of Dr. Stromeier's book, we must remark, that its practical value is likely to be diminished by the too rigid adherence of the author to the theory by which he wishes to explain all the phenomena of the disease. As we said at the beginning of the article, we even doubt the truth of the theory altogether, if, by the term *paralysis*, as here applied, we are to understand that kind of loss of muscular power which is usually termed paralysis in this country. If he will allow us to read *debility* for *paralysis*, muscular debility produced by muscular inaction, we shall not differ very widely from him in many of his views. It is certainly true that, while the author has rendered a positive service, by forcibly calling attention to the influence of *inaction* of the inspiratory muscles in weakening the natural supports of the spine, and thus contributed to improve our means of both prevention and cure,

he is disposed to make his fundamental proposition much more general and sweeping than strict observation would warrant, and to deduce many of his peculiar views from a theoretical enlargement and one-sided consideration of his facts, rather than from their plain and literal meaning. In many instances he traces the absence of respiratory motion to nervous paralysis, where other observers would ascribe it to muscular debility dependent solely on inaction. Experience does not warrant the belief of real paralysis being so common as his doctrine would make it; and why should it lay hold of the inspiratory nerves, in preference to all others? If, on the other hand, the evil is debility from inaction, we can at once understand the history, symptoms, and progress of the case. We cannot see any good *a priori* reason why paralysis of the accessorius should so often attack and lead to curvature in females, and so rarely in males; but we see a very cogent reason why muscular debility should arise from muscular inaction in quiescent and corset-encased females, who cannot distend their chest even to save their lives,—and why boys and active and uncompressed girls should escape. That nervous debility accompanies muscular is no doubt true; but that is quite a different thing from paralysis, and owns a different cause. We could point out females whose muscles employed in deep inspiration, have not moved a fibre for months, or perhaps years; not from paralysis of their motor nerves, however, but from sitting all day working in constrained or bent positions, and enclosed in stays which prevent any considerable expansion of the ribs. In many such cases, we believe that the power of making a deep inspiratory movement is lost for the time by disuse, but requires only freedom, and the natural action of the inspiratory muscles, to be restored.

That an inactive and debilitated state of the inspiratory muscles conduces to spinal curvature, we fully believe, because their healthy action is evidently conducive to a firm and erect carriage; and Stromeyer deserves credit for shewing the share they have in the result, and its *modus operandi*. The indifferent general health, common in such cases, seems to us to be produced not by the curvature, but by the previous mode of life impeding an important function—respiration, and debarring the exercise without which health can scarcely be looked for. It is aggravated by, and is the concomitant, but neither the cause nor the effect of the curvature.

We quite agree with Dr. Stromeyer, that numerous cases of short and difficult breathing are owing more to want of muscular action than to the state of the thoracic contents themselves; and to the improved action of the inspiratory muscles we have been accustomed to ascribe the freer breathing which many invalids, suffering from chronic diseases of the chest, experience after exercising moderately for a few weeks on hilly ground. In such cases, a part of the benefit is no doubt owing to a less congested state of the lungs and internal viscera, but much is owing to more vigorous inspiration. In the improvement of the wind in training, we have no doubt that a great deal arises also from increased tone and readiness in the inspiratory muscles. The good effects of lecturing and reading aloud are partly owing to the same cause. Hence the evil of the quiescence creeping into the modern amusements of the young. Many girls, and even some boys, scarcely know what a full

deep inspiration is, and hence their chests never become adequately expanded, or the trunk vigorous and erect. When the deep inspiratory muscles are not adequately developed by habitual exercise, the shoulders fall down by their mere weight, diminish the caliber of the chest, and act as weights against its expansion. It may be that, in some instances, the primary cause is want of nervous influence to the inspiratory muscles; but we much doubt if it is often so, except from the patients being deprived of such plays, pastimes, and exercises as constitute nervous stimuli. It is no loss of power inherent or originating in the nerve. We have often observed benefit from regular friction along the cervical and upper dorsal spine, and over the shoulders and scapula; but we regard that fact in the same light, first, as in itself exercise to the very muscles which want it, and, secondly, as promoting their nutrition and vigour, by the friction acting directly on their vessels and nerves. Of course, the spinal nerves are stimulated, and aid in the result, but still not as in real paralysis.

ART. IX.

On the Nature and Treatment of the Diseases of the Heart; with some new Views on the Physiology of the Circulation. By JAMES WARDROP, M.D., Surgeon to his late Majesty George IV.—London, 1837. 8vo. pp. 120; four Plates.

THE title-page intimating that an author of Mr. Wardrop's powers and great professional acquirements had some new views of the circulation to offer, we took up this book with no little interest and expectation. The first part only has yet appeared, and consists entirely of the physiological observations and speculations which the author has deemed it requisite to prefix to his dissertation on Diseases of the Heart. We are sorry to say that, after an attentive and impartial perusal of these observations, we have formed a considerably lower estimate of their value than the author seems to have done.

In the very commencement of the work we meet with several statements, to the truth of which we cannot assent: such as the total want of repose in the muscular fibres of the heart; the existence, in the composition of this organ, of "a considerable quantity of the yellow fibro-cartilaginous substance, similar to the fibrous coat of the arteries," &c. But we pass these by, in order to notice doctrines which Mr. Wardrop considers as new, and to which he evidently attaches great importance.

The first of these is a particular influence exerted by the muscles over the circulation, to which he gives the name of the *Musculo-cardiac function*. After noticing the effect of muscular contraction on the flow of blood in the veins, he observes,

“But, whilst the pressure caused by muscles during their contraction propels the blood onwards in the contiguous veins, it seems never to have been contemplated what must be the effect of that compression on the adjacent arteries; although these vessels are doubtless alike exposed to its influence. It will, however, be shown that the effect of muscular contractions, both on arteries adjacent to, as well as on those imbedded in the substance of muscles, must be to compress these vessels, by which compression the flow of blood through them will be necessarily impeded: hence, the

contraction of muscles will increase the accumulation of blood within the heart in two ways,—by accelerating the flow of the venous blood to the right heart, and by impeding the transit of the arterial blood from the left heart." (P. 18.)

An instructive case related by Mr. Hyslop, of a lady in whom syncope was arrested by compressing the brachial arteries, is brought forward to illustrate the stimulant effect on the heart, produced indirectly by accumulation of the blood in its left cavities. Various proofs are adduced in support of the doctrine of the muscular compression of the arteries.

"If the radial artery of a person who is powerfully contracting the muscles of the arm be examined, its pulsations are soon found to become feeble, and at last they are scarcely perceptible; whilst, the moment the muscles are relaxed, the artery is perceived to beat with its natural force. By the same experiment it may also be shown that the effect of impeding the transit of the blood in the arteries is to increase the action of the heart; it being found that, in proportion as the strength of the pulsations of the radial artery diminishes, so is the impulse of the heart increased." (P. 20.)

This experiment requires a little explanation. It might be supposed, on first reading the passage, that the radial artery itself suffers compression; but this cannot be the case; for we have often examined the pulse during the strongest contractions of the muscles of the forearm, and found no change whatever, either in its force or in its volume. Nor do the ordinary actions of the flexors and extensors of the arm, even when very powerful, produce the effect in question. It may be noticed, however, when the biceps and triceps are made to antagonize forcibly, and thus to compress the artery between them; an action which occurs but seldom, and certainly not often enough, in our opinion, to operate importantly on the general circulation. The author proceeds to show that, in those parts which do not require an equable supply of blood, the artery is liable to muscular compression; whilst, in those which do not admit of changes in the quantity, it is protected. Thus, in the limbs, the artery accompanies the vein, in order that both may be exposed to compression, and thus may subserve the musculo-cardiac function. Certainly, as Mr. W. remarks, the importance of the fact of this relative position, though long observed, has not been pointed out; but we must be permitted to doubt the value of his discovery, till he has proved in some more satisfactory manner than by the experiment above noticed, that compression is a frequent occurrence. Mr. W. will pardon us if we venture to conjecture that the principal artery and vein are companions because it is expedient that each should be situated in parts (as in muscular beds,) where they will be defended from external pressure, and be out of the reach of many external injuries. But some organs cannot well bear any interruption in the supply of blood, and therefore they are provided with anastomosing circles, as in the stomach, lips, and iris; all which, but for this provision, would (as Mr. W. thinks,) have been subject to arterial obstruction, from their unceasing movements. We should have thought that the large quantity of blood required by some of these parts would have been an adequate explanation of the anastomoses. The heart, according to Mr. W., is saved from obstruction of its circulation by the position of the coronary arteries; and we quite acquiesce in this explanation of the final cause of the situation of these vessels; for if the trunks had perforated the ventricular substance, they could not have escaped compression. But we really think that the principal arte-

rial trunks, imbedded among the muscles of the limbs, occupy an analogous situation, being guarded in aponeurotic canals, and never passing through the body of a muscle, and therefore similarly shielded from compression.

Like many men who have made discoveries, or imagined that they have done so, Mr. Wardrop often carries his theory to situations seemingly very remote from their legitimate operation, and occasionally applies them in a very fanciful manner. The two following, we conceive, are examples of this sort.

"But an equal and uninterrupted return of the venous blood from the head is of no less importance in the due performance of the brain's functions than a regular supply of arterial blood: hence the importance of that peculiar structure, the unyielding parietes of the veins or sinuses, for warding off the effects of that very compression which is provided in some other veins for the purpose of accelerating the return of the venous blood." (P. 27.)

Mr. W. does not tell us what muscular compression endangers these veins or sinuses unless thus "warded off," and gives us no reasons why the numerous veins of the pia mater and the *venæ Galeni* do not possess unyielding parietes like the sinuses.

"The vermicular motions of the stomach and intestines during the process of digestion must doubtless have a very considerable influence on the circulation of the blood, both in the veins and in the arteries of these organs; and hence, during the movements of the alimentary canal, we observe an increase in the impulse of the heart, indicated by an increase in the frequency of the pulse. In this respect, the circulation of the blood in the intestinal canal in part resembles that which exists in worms, the almost unceasing movements of these animals being sufficient, as I have before noticed, to circulate their blood, unaided by a heart." (P. 29.)

The author believes that this function "enables us to explain many important phenomena which are constantly occurring in the living body, and which could never have been explained until a knowledge of this function of the muscles had been discovered." The first of these is the remarkable fact that, in passing out of sleep, a man first stretches, then yawns, and then awakes! The movements create an accumulation of blood in the heart, whereby its action is increased; and thus more blood is sent to the heart, which causes the resuscitation of the mental powers. Has Mr. W. never met with persons who awake without any muscular movement except the opening of the eyelids? and with others who, during profound sleep, execute muscular movements amply sufficient, upon Mr. W.'s hypothesis, to arouse the heart? Is it wonderful that, if sleep is a suspension of sensation and motion, the cessation of that state should be often first evidenced by returning movements?

The application of the musculo-cardiac function to the explanation of many familiar facts is often very ingenious, as in the following instances: we wish we could say that it is always as just as it is ingenious.

"If a man be about to make any great exertion, such as running or leaping, he prepares himself, as it were, by first vigorously contracting the muscles of the arms and clenching his hands. For the same reason, when a person is subjected to pain, as that of a surgical operation, he prepares himself to endure it by throwing into action almost all the voluntary muscles, grasping firmly with his hands, and pressing the feet against some resisting body. And when the female, during parturition, is about to make a powerful expulsive effort to assist the uterus in giving birth to the infant, she in like manner throws into violent and long-continued contractions the muscles

of the extremities, clenches the jaws, and squeezes with a convulsive effort whatever may be within her reach." (P. 33.)

The first question probably that will occur to the reader of this extract, is,—What good object can be answered by thus forcing the heart to contract by virtue of its distention, allowing that the latter effect is produced? Is a throbbing heart serviceable to a runner or leaper? Does the person who is expecting a surgical operation or the parturient female derive relief from the excited action of the heart? If Mr. W. is right in conjecturing that muscular contraction hurries the blood *away from the muscles* through the veins, and prevents *its access to them* by the arteries, in order to stimulate the heart, we again ask, what object does this stimulation achieve? It appears, contrary to all analogy, that the blood should be sent impetuously to all parts but those which from their vigorous action would seem to need it most. If Mr. W. had found out that persons about to put their brains to unusual exercise, begin by clenching their hands, &c. one might discern a good reason for it in the hypothesis that they thus furnish the thinking organ with a plentiful supply of blood. But setting aside this difficulty arising out of the final cause (which we should have less insisted upon, but that the author devotes himself particularly to this description of causes,) a more natural explanation, if we are not mistaken, may be found for the actions in question. The runner, the leaper, and the cricket-player, exercise their muscles, before their efforts, not to quicken the heart's action, but to bring the motory organs into free play and readiness, as well as to prepare the respiratory apparatus for the change about to take place in the circulation during the exertion. The person suffering or about to suffer pain grasps with his hands and presses with his feet, for the same purpose as he bites his lips; namely, to produce a new sensation *counter-irritative* to the pain. The strivings of the parturient woman have a more complex operation.

We have not space for adverting to the other illustrations of the new function. We may only observe, in passing, that the argument apparently most favorable to the idea of arterial obstruction forms the conclusion of the present section. It is the provision with which certain animals are endowed against the compression of arteries. Thus, in the feline tribe the brachial artery passes through a bony canal in the humerus. But this arrangement appears to us intended to protect the vessel from "pressure from without;" that, for instance, encountered in climbing and in clasping the prey with the anterior extremities. The same arrangement is found in the tardigrade animals which remain suspended for a long time to the branches of trees. In the shark, the aorta runs along a bony channel in the caudal vertebræ, to avoid compression from the powerfully muscular tail. This distribution is not peculiar to the shark or to strong-tailed fishes; but supposing it to be so, would the vessel be liable to this accident from the muscles themselves or from the altered form of the whole organ?

We shall only remark, in addition to what has been stated, that the difference in the structure of the arteries and the veins seems to hint that they are not likely to be assisted in their circulation by the same means, and that while the yielding tissue of the latter, and their valvular provi-

sion in parts of the body where they are liable to muscular compression, explain the influence of this action on their function, the firm resistant coats of the former and the absence of any apparatus capable of giving the right direction to the flow of blood in their channels, certainly do not indicate any fitness for the reception of the supposed influence. Another anatomical difficulty occurs to us:—is the structure of the aortal valves at all fitted to cope with such a strain as must be exerted upon them during the musculo-cardiac function? We suspect that they have quite enough to do in withstanding the reflux occasioned by the arterial elasticity; and, at all events, we might naturally expect them to be far stronger than those of the pulmonary artery upon which no muscular effort can directly operate.

To the framer or the supporter of the musculo-cardiac hypothesis, the following question must very soon arise: If the blood, while it is thus pouring into the right heart from the veins, is not allowed to issue in corresponding quantity from the left, and is therefore prevented from entering into the latter, what becomes of it when thus rudely expelled from the one and refused admittance into the other? The answer is presented in another discovery or invention of our author, that of the *pulmo-cardiac function*.

“If there be only a slight increase in the quantity of blood within the heart, such additional stimulus by increasing the vigour of the heart’s movements may, along with the elastic quality of the fibro-cartilaginous portion of its structure, which is placed at the roots of the large vessels, be alone sufficient to equalize the circulation. But if the increased supply of blood to the heart be so considerable that the surplus quantity cannot be received within its cavities, the lungs are then required to lend their assistance. To fulfil this *pulmo-cardiac function*, the structure of the lungs is admirably adapted. The pulmonary vessels, being imbedded in a soft and yielding substance, are susceptible of various degrees of distention, so that they readily give way for the reception of any surplus quantity, whether of venous or of arterial blood, and retain it until it can be received within the heart. The structure with which the lungs are endowed to enable the air-cells to accommodate themselves to those differences in the quantity of air which take place during respiration, also enables the pulmonary vessels in their turn to accommodate themselves for the reception of the various quantities of blood which may be impelled into them. A function is therefore performed by the respiratory organs which is quite unconnected either with arterialization of the blood; with aiding the return of the venous blood to the right heart, or with assisting the circulation in the pulmonary vessels; and when it is considered how often and by what slight causes the current of the arterial blood is impeded in leaving the left heart, and how frequently the return of the venous blood is accelerated to the right heart, this *pulmo-cardiac function* must be regarded as one of primary importance. It exercises, as might be expected, a remarkable influence on the function of respiration, for whenever the egress of the blood from the left heart is impeded by muscular contractions, and the exit of the systemic blood from the lungs into the left auricle is interrupted, the velocity of the blood in the veins will also be increased, and venous blood congested in the pulmonary arteries. Thus the blood being accumulated in the pulmonary arteries and veins, as well as in both hearts, respiration becomes quickened, and its frequency increased in proportion to the degree of impediment which is offered to the distention of the air cells; and the respiration will continue quickened until the circulation be equalized, the heart become tranquil, and the congested lungs relieved of all surplus quantity of blood.” (P. 47.)

To us it appears unfortunate that, if the lungs are to play the part of a *diverticulum sanguinis*, they do not remain passive and allow the blood to accumulate, till the right heart is less disposed to impel, and the left

in a better condition to admit. Instead of this, the respiration is quickened, a greater quantity of venous blood is converted into arterial, and pours on towards the left heart, the excitement of which distributes it (in spite of the musculo-cardiac function,) in proportionate quantity to the arteries, as may be lamentably proved in the subject of an aneurismal tumour. We suspect that pulmonary congestion is not so *normal* a condition as to be fitly denominated pulmo-cardiac function. Mr. W. is aware that diving animals are furnished with dilatations of the vena cava, but we might not unreasonably have expected, upon his hypothesis, to have found, in these instances, some convenient sacs at the extremities of the pulmonary arteries also.

The author offers a curious rationale of the method of "training" or "putting a person in wind." He begins with the position that full inspirations by distending the air-cells retard the pulmonary circulation; a position founded upon an observation by Sir E. Home, that "when the arteries of a sheep's lung were injected, the injection returned very readily by the veins; but when the air-cells were previously distended, this did not take place, and the injection could not be forced into the pulmonary veins." But it is one thing to have the lungs distended with air, which produces a change in the blood essential to the capillary circulation, and acting probably by the determination of molecular movements in the fluid itself, irrespectively of the containing parts; and it is another thing to fill the cells with mercury and inject wax into the vessels. The notion, at all events, is the very opposite to that which Haller adopted in explaining the physiology of asphyxia; and to Sir E. Home's experiments we may oppose those of Magendie, which demonstrate that the pulmonary circulation is actually promoted by inspiration. Presuming, however, on the truth of the above statement, Mr. W. infers that "training" consists in acquiring such a control over the breathing as to limit the inspirations, and thus to prevent such a distention of the air-cells as would impede the passage of the blood into the pulmonary veins. The author seems here scarcely consistent with himself; for, if the left heart is struggling with the muscles during their exercise, its difficulties must be rather increased by a greater influx of blood from the pulmonary veins. The greater the stasis in the capillaries of the lungs, the more perfect would be the pulmo-cardiac function!

But, that control over the respiration is much needed during great exertions, and that the inspirations are limited, cannot well be denied.

"Hence in running a race it is found essential that the mouth be kept shut, a sufficient quantity of air entering by the nostrils for the arterialization of the blood, and experience having taught that if any additional quantity is inspired by the mouth, the proper balance between respiration and circulation is more or less destroyed, and the person obliged to discontinue the effort. The common practice, when running, of putting a pebble in the mouth, by the effort of retaining it, keeps the mouth closed. An animal at full speed, it will also be observed, has the mouth always kept shut, either until his muscular powers begin to be exhausted, or when from alarm he inspires by the mouth; so that, whenever an animal which is pursued opens its mouth, it is well known that he cannot long sustain his speed. In like manner for limiting the inspirations not only does the man who runs a race keep his mouth shut, but he also places his arms close to his sides, with the fore-arms in a state of flexure, firmly contracting all the muscles. Artisans also whose avocations require a great muscular effort, are in the habit of tying a belt round the waist; thus preventing too great an expansion of the chest; and when the sailor prepares himself for battle, in order the

more powerfully to exert himself at the guns, he ties a handkerchief firmly round the waist, by which, during the excitement of the fight as well as the powerful muscular exertions which he is compelled to make, the movements of the respiratory apparatus are confined within certain limits, and he is prevented from making such full inspirations as would disturb the balance of the circulation within the chest.' (P. 59.)

We do not think that these facts are elucidated by Mr. W.'s views. The influence of training is of a far more complex description than would appear from the author's shewing, and involves the consideration of the irritability of the heart, and of the whole nervo-muscular apparatus of respiration. A sudden or unusual exertion occasions a sense of constriction of the chest, which prompts the accessory muscles to active play, and is soon followed by palpitation. Now the quick movements of the thoracic parietes, under the action of the accessory muscles, are quite incompatible with the exertion of other muscles which require the chest for a fulcrum all but immoveable. Panting therefore soon brings the runner to a stop. The respiratory perturbation must be restrained, and this may in a great measure be accomplished by one who possesses such a command over the accessory muscles as prevents their obeying the stimulus of the sensation. Practice will give this degree of command; but the training comprehends a number of processes, all calculated to lessen irritability, and to place the organs of circulation and respiration in a state the very reverse of what we see in hysterical patients, whose heart and respiratory muscles are thrown into convulsive action by an amount of exertion that would have no such effect upon a person of less nervous mobility. The individual trained, however, has diminished even the ordinary irritability, and he manifests in his feats of strength the same superiority over the unpractised though healthy performer, as we noticed in the latter over the hysterical subject. But the gymnastic, however well-prepared, is always cautious to begin his exertions slowly, so as to accustom the heart to the increase of its natural stimulus, and the lungs to a greater accession of blood. The use of limiting the inspirations by the methods enumerated in the passage last cited, we apprehend to be partly to diminish the movements of the chest, and partly to counteract the effect of the muscular exertion on the venous circulation; for we do not doubt that inspiration favours the flow of blood towards the right side of the heart. But it must be borne in mind that although the inspirations are diminished in extent, they are increased in frequency.

Mr. W. next employs himself in unfolding the purposes answered by such phenomena as laughing, crying, sobbing, sneezing, &c.; and although we do not completely accord with his views, we must admit them to be ingenious. The following remarks upon sea-sickness are also ingenious.

"It is from a want of the power of adjustment, or cooperation in the respiratory and circulating organs, by which we can explain sea-sickness. Dr. Wollaston observed, that in those who did not suffer sickness at sea, their respiration was altered. In waking from very disturbed sleep, he found that his respirations were not made with the accustomed uniformity, but were interrupted by irregular pauses, with an appearance of watching for some favorable opportunity to make the succeeding effort, and it seemed as if the act of inspiration were in some manner to be guided by the tendency of the vessel to pitch with an uneasy motion." * * *

"The mode," says Dr. Wollaston, "by which I afterwards conceived that this action could primarily affect the system, was by its influence on the motion of the

blood; for, at the same instant that the chest is dilated for the reception of air, its vessels become also more open to the reception of the blood, so that the return of blood from the head is more free than at any other period of a complete respiration. On the contrary, by the act of expelling air from the lungs, the ingress of blood is so far obstructed, that when the surface of the brain is exposed by the trepan, a successive turgescence and subsidence of the brain is seen in alternate motions with the different states of the chest.* Dr. Wollaston's views of the cause of sea-sickness accord with the means that are usually resorted to for its relief. One mode is placing the body in such a position, that the weight of the column of blood in the head shall be diminished as much as possible during the 'pitching' of the vessel. The other is by modifying the respiration, to prevent congestion within the head. The horizontal position, and placing the head as much as possible to the ship's centre, mitigates the sickness according to the first principle; whilst by the common practice of tying a belt firmly round the waist, or by keeping the mouth shut, and thus limiting the expansion of the parietes of the chest, respiration is so regulated, that the balance of the circulation within the head and chest is preserved." (P. 72-4.)

Several sections are devoted to illustrations of the reciprocal influence of the heart and the brain, and of the heart and the digestive organs; to the blood, and to the pulse; but we find nothing in them to detain us either for exception or for particular commendation. The "Impulse" and "the Sounds of the Heart," have each little more than a page allotted to them. Upon the causes of the latter, we have only the following unsatisfactory paragraph:

"Much has been written and many unnecessary and cruel experiments have been made on living animals, in order to find out the causes which produce the sounds of the heart. But, as Magendie has justly observed when alluding to the discordant opinions on this subject, 'the results of physiological enquiries have not been in proportion to the number of authors who have written on the subject; on the contrary you are disturbed by contradictions of the most glaring nature, or bewildered between a mass of theories and explanations which have no existence but in the minds of the inventors.'" (P. 100.)

Certainly "much *has* been written," and Mr. Wardrop might have relieved the extreme barrenness of his remarks upon this subject by quoting from the works of Williams, Hope, and Bouillaud, and from the highly valuable reports of the committees appointed by the British Association of Science to investigate the causes of the sounds of the heart. We infer that Mr. W. has not paid sufficient attention to the researches of these physiologists, or he would not have been misled by the depreciating observations of M. Magendie, who, however deserving of consideration his dicta may be in other departments of physiology, has so signally failed in this, as to be the last authority that we should have thought to find quoted on the present occasion. We shall be curious to see whether Mr. W. will be able to place his views of the diagnosis of diseases of the heart on a level with the knowledge of the day, if he abstains from referring to the physical states of the organ on which its normal sounds depend.

We conclude our notice of this treatise by remarking that, although it exhibits considerable ingenuity of speculation, we cannot point to any new fact of any consequence that it has established respecting the action of the heart, nor to any such views of facts previously discovered as would seem to have made it worth the author's while to publish them separately. It may be hoped, however, that the forthcoming part, on diseases of the

organ will present so rich a store of facts, gathered from the author's experience, as to make us only regret that it was not at once affixed to the physiological introduction; because we might then have passed over in silence what was deficient in the latter, for the sake of the more valuable contents of the former. It is but just to add, that the present little work is written with much clearness and simplicity, and is really very interesting. We have felt much regret in being obliged to differ so frequently with a man of no ordinary talents, as the author proves himself to be, even when most in the wrong.

ART. X.

Mémoires de la Société Médicale d'Observation, de Paris. Tome premier.
—Paris, 1836. 8vo. pp. 495.

Memoirs of the Medical Society of Observation, of Paris. Volume 1st.
—Paris, 1836. 8vo. pp. 495.

THIS volume is the first proceeding from a small Society, founded by a few medical students in 1832, for the professed purpose of improving the science of observation in medicine, which they considered as extremely neglected and underrated; believing it, as they say, to be "contre l'opinion commune," difficult, and requiring a long apprenticeship; although it might perhaps be objected to these ardent and able reformers that the common opinion is not quite so erroneous as they represent it. Out of the forty or fifty names which compose the new Society, a few only are known and distinguished. M. Louis is the perpetual president, and MM. Chomel and Andral are the *honorary* presidents: among the rest, we observe some of considerable celebrity; but, as must happen in all societies, a few whose names carry little recommendation with them.

M. Louis is, of course, put into the front rank: he writes the preface, introduces his friends, French, English, American, and Genevese, the "young medicine" of the existing world; supplies an article of sixty pages on the Examination of the Sick and the Investigation of General Facts; and another of 100 pages on Emphysema of the Lungs. M. Maunoir gives a contribution of 100 pages on Cataract; and M. Bizot inserts 150 pages on the Heart and Arterial System in Man, divided, after the manner of the old theologians, into three parts, each part into chapters, and each chapter into articles; whilst to the whole are prefixed an "avertissement" and a preface. The remaining pages, about 100, are filled up by M. Marc d'Espine, with an analytic memoir, "sur l'Orchite Blennorrhagique."

Notwithstanding some degree of arrogance of tone, however, the plan of this Society is admirable. Its main object being to form observers, cases are contributed by the members at the weekly meetings, which are read and criticised; the omissions are commented upon, and the means of investigation into facts difficult to be established are freely enquired into. Thus, all the members are, it is said, gradually so improved as medical reporters, that the facts they relate acquire all the certainty which is necessary, and become part of groups to be hereafter analyzed by the Society. As to the method to be pursued in order to pass from

particular to general facts, the necessity of the numerical analysis has always appeared to the members to be so evident, that no discussion, says M. Louis, has ever taken place on the point. It is, we confess, with respect to this latter particular that we have a slight distrust of some of the members of the Society of Observation. To maintain the character to which they aspire, it is at all events necessary that they should carefully distinguish the statistical and practical boundaries of the numerical method. Observers of one set of medical facts may easily group them, and with some statistical utility; but the observation necessary for the safe treatment of diseases comprehends many circumstances which distinguish every individual case from the rest. This view has been ably set forth by M. Double; and, although we doubt not that his notions are thought of small account by the professed observers and numerators, we believe the voice of the veteran is that of sage experience.

The perpetual president of the Society proposes, every year, one or more subjects of study, in which all the members are invited to take a part: a subject being fixed upon, some member presents a plan for its observation, and this plan is discussed and determined by the members. M. Bizot's treatise on the pathological alterations of the arterial system and of the heart was commenced according to this arrangement; and is certainly a striking proof of its value.

M. Louis' essay in this volume, on the Examination of the Sick and on the Investigation of General Facts, is intended as an exposition of the principles maintained by the Society. It very appropriately begins with a general, and by no means a just, condemnation of the physicians of antiquity and also of modern times, as bad observers and imperfect describers. Having advanced this indiscriminate remark with an air of remarkable satisfaction, M. Louis proceeds to account for it; and the reason of the lamentable deficiency is, of course, no other than the want of the numerical method. M. Louis thinks medicine may be advanced and rendered certain, just as chemistry may be and has been, by reckoning *every thing*. To take every thing into account is doubtless important to medical observation; but between chemical facts and medical facts there is surely small analogy. Atoms, and grains, and cubic inches are always the same, which cannot be rigorously said of any single symptom. A cough may not be weighed, nor the exact extent of a pain meted out, nor the combination of symptoms in a fever determined by the atomic theory. It is but fair to M. Louis to state, that he endeavours to justify his sweeping censure of all past observers by mentioning that Pinel intrusted to very young men the task of collecting the facts on which he founded his theories; and that, fifteen years ago, when he (M. Louis) persevered in the examination and collection of facts, being no longer a mere medical student, his application excited, on that account, so much surprise and pity, that it required some courage to go on with it. Greatly, indeed, was it to the honour of M. Louis that he did go on with it. Whatever he may say and think in reference to the numerical method, he has not himself forgotten its most useful applications, but has rendered them really available to the advancement of sound medical knowledge.

M. Louis points out the importance of a previous acquaintance with the age, occupation, usual diet, habitual constitution, mode of life,

ordinary state of body, and former illnesses of the patient whose case is to be investigated; and he warns the examiner against the many sources of deception to be avoided. The object of our examination, he justly says, is not solely to verify the descriptions of authors, but to acquire knowledge; and each malady should be studied, in particular instances, under the greatest number of points of view possible. He dwells strongly on the two points of determining when the disease commenced, and what was the actual state of the patient's health previous to the attack; giving as instances of the important conclusions to be drawn from these circumstances, that of pleurisy, and that of erysipelas of the face, occurring in persons previously in good or in disordered health, by which the prognosis would be considerably modified. Each symptom is then, says M. Louis, to be studied in the order of its development. Every function is separately to be investigated.

"Besides this, it is not sufficient, in the actual state of the science, to have ascertained the beginning, the degree, and the progress of the symptoms: their study, to be complete, requires a further precision, the importance of which is easily understood, and need only be briefly remarked upon. Suppose, for example, that the abdomen is, or has been, the seat of pain, it is not sufficient to make known its existence, character, and commencement; we must ascertain, if possible, the point of its commencement, since this may be the exact point from which the disease commenced; as may be seen in the typhoid affection, in which the pain of the abdomen ordinarily begins in the iliac fossa on the right side, or in the point corresponding to the first patches of the glands of Peyer which have undergone change. I may say the same, and for the same reasons, of pains of the chest, of percussion, and of thoracic auscultation. It would be quite insufficient, for example, to note that the thorax returns an obscure sound on the right side, anteriorly or posteriorly: the point where the dull sound is rendered must be precisely indicated, at the summit, at the base, or the sides, and its extent and degree should be marked. The same remarks apply to the different kinds of râle, and particularly to the subcrepitous. Merely to ascertain its presence, for instance, in acute pulmonary catarrh, in which it is so frequently observed, would be almost to do nothing, or to pass over one of the most important points in the history of this kind of râle. Observation, in fact, teaches that, at its commencement, it only exists in the posterior and lower parts of the chest; by which a very essential difference is established between catarrh and phthisis. By daily studying the subcrepitous râle, and noting exactly the part of the thorax where it ceases, it is soon perceived that it extends itself from the base to the summit, without interruption; and we thus establish, by the only means of observation which we yet possess, that pulmonary catarrh extends like other affections of the same kind." (P. 11.)

The same minute attention, M. Louis maintains, is necessary in the prosecution of pathological anatomy. Every organ should be examined, and its condition, healthy or diseased, carefully observed; the object of the examinations being not merely to verify the seat of the disease, its nature, and complications, but to attain a fuller knowledge of the laws of the animal economy, which can in no other way be attained than by an examination of every organ, whatever may have been the cause of death. We owe, he observes, to pathological anatomy, cultivated after this method, the knowledge of the fact that, in a subject above fifteen years of age, we never find tubercles or grey, semi-transparent granulations in any organ, without the same being existent, and often far advanced, in the lungs. In the same manner it has been established that ulcerations of the pharynx, of the œsophagus, and of the small intestines, belong exclusively (with the exception of syphilis,) to two

affections, severe fevers and phthisis; and that ulcerations of the larynx and trachea, particularly the latter, only occur (with the same exception,) in phthisical subjects. The knowledge of such laws may occasionally serve to interpret such diseases, in the absence of ordinary symptoms.

"Thus, that form of peritonitis which is chronic in its character from the commencement, is, according to all the facts I have been able to collect, constantly tubercular in adults, or in patients from fifteen years old to old age, or associated with grey semi-transparent granulations, developed upon or under the peritoneum. But, as I have just observed, neither of these lesions exists in any organ without being also observed in the lungs; so that a chronic peritonitis being actually established, we may recognize phthisis, independently of symptoms connected with the state of respiration, and even in their absence; meaning the existence of more or fewer tubercles, or grey semi-transparent granulations, in the lungs. If there are any exceptions to this law, they are very rare. I have more than once announced the existence of phthisis in patients who had all the symptoms of chronic peritonitis, without any alteration of the pulmonary parenchyma appreciable by the stethoscope or by percussion; and even in patients who had no cough: a diagnosis which was sometimes deemed very uncertain, or even rash, but proved to be correct, and which I could not have renounced without abandoning the laws of the animal economy and science itself; for the laws constitute the science." (P. 17.)

In the rules which M. Louis lays down for the investigation of the predisposing and exciting causes of disease, in each patient submitted to examination, we find the same exactness of method, and the same contempt of labour for the great object of increasing the knowledge of all that appertains to disease. He is careful, throughout, to point out every proper object of enquiry, and he notices every probable source of deception.

The second chapter of M. Louis's paper (for it seems the habit of all the members of the Society to divide their essays into chapters,) commences with an earnest repetition of the advice, already many times given in the first, to observe *all* the symptoms in each case, and not those which are merely diagnostic or proper to the malady supposed to be present. He lays it down as a principle that truth is not to be divined, but to be found, and that nature is a problem only to be solved by means of a sufficient number of exact facts. These facts being numerous, are to be arranged in proper groups, according to their similitude, and the identity of the affection they indicate or disprove. It is of little consequence with what symptom the enquiry begins; the first thing being to ascertain in what proportion of cases it shews itself, without which physicians are not justified in speaking of it as rare or as frequent; expressions implying that they have counted, when they have not. Mere counting, however, he admits, may lead to absurd conclusions; each symptom enumerated must be considered in relation to the age, sex, and various circumstances of the patient; but he still maintains that it is necessary to count; and he denies the possibility of avoiding error, or arriving at exact conclusions, by any other method.

Now, although we do not dispute the general correctness of these remarks, it is impossible not to see that not only in medicine, but in the general conduct of life, there is much wisdom acquired, and of a practical kind, on which men are obliged to depend, and which was never acquired by this formal and mechanical process. What is called know-

ledge of character and experience of mankind is not learned by counting, but by reasoning from impressive events: it is a knowledge acquired by induction, not by numeration; by induction of like particulars, not by counting circumstances, including such particulars with various other particulars. A man who is addressed by a swindler, and who suspects, from certain characters of the address made to him, that the person making it is not an honest man, does not derive his suspicion from his tablets; he does not look at his memorandum-book to see, having been so addressed twenty times before, whether he was cheated eight times, or twelve times, or eighteen times, and whether in all the cases the swindler had the same manner, the same cast of countenance, used the same words, or more than half of the same words, or was of the same age, stature, embonpoint, countenance, and colour of hair. He attains the useful conclusion much more quickly, and with no less certainty, by inductions from looks and manner associated in his mind with villany: and we are convinced that, if we regard medicine in relation to the practical art for which it is chiefly studied, the laborious counter of facts would often be found too minute to be useful, and his processes both elaborate and deceptive. The patient would die before he had finished his observation, or, at least, before he had come to the end of his arithmetic. The most accurate and extensive tables of life, whatever number of years they may promise to fifty per cent. of men of a given age, cannot secure any one of the fifty from an accident at any moment of any one day; but each case in medical practice is as this one case of the fifty. Each case in the fifty is liable to contingencies affecting its character, and calling for a corresponding modification of the means employed. Therefore, although, for statistical purposes, it is evident that recourse may be had to the numerical method in all its formality and exactness, the mental enumeration followed in practice will ever be a shorter process, and perhaps must be shorter that it may be promptly serviceable. It may be objected that the short method of numeration is never rigidly correct, or is open to error; but it would require more faith in minute observers, and in tables, and all the long array of figures, than we possess, to impress us with a conviction that the longer process would prove infallible, even if adopted by innumerable observers, and continued for centuries. The almost universal distrust of other men's observations which now makes every man rely much upon his own experience, would not yield, in particular instances, to all the figures in the world. The practitioner would bleed or not bleed in pneumonia or in fever, not because so many millions had been successfully bled at the same age, period of disorder, &c., but because, by certain mental processes, no less valuable than tables of cast-up figures, he was convinced that bleeding would save or endanger life. To suppose the contrary, would make little more necessary to the practitioner than a moderate share of the perceptive faculties, and a book of tables, and every case would become a question in addition and subtraction. Superiority of understanding, the ready exercise of judgment, and good sense, would go for nothing. They would wholly disregard the philosophical truth advanced by M. Broussais, in his spirited letter to M. Risueno d'Amador, (*Rev. Méd., Juillet, 1837, p. 141.*) that the causes acting on the impressible animal economy are ever changing, in combinations or in kind, and

would mechanically rule the future by the past. These objections are not merely theoretical. We have had sufficient opportunity of observing the observers, as they exclusively consider themselves, to know how the vast pile of details which they laboriously accumulate, shuts out from their view those general views which distinguish a good practitioner from a bad one; views which alone impart the quality of sagacity. The mistake of the numerators is, that they depreciate all calculation that is not reduced to figures, as if no one could reckon ten without counting his fingers. To use their own words, *more* and *less*, and *often* and *seldom*, have no meaning, unless we can say how many times *per cent.* They would object to the expression, *Many people suffered severely from the influenza last winter.* In the first place, what is *many*? Is it ten, twenty, thirty, or sixty per cent.? Secondly, what is *severely*? Thirdly, what is *influenza*? Fourthly, what season of the year can strictly be called *winter*? First prove all these things by tables. And yet we imagine many of our readers will think that the assertion may be safely relied upon, without all this apparatus; and that the laborious individual who sate down to solve all these problems before venturing to make the assertion, although he might become a "full man," would not by any means be a "ready" one. But readiness in practice is every thing. The most undeniable conclusions, if only carried in the pocket, are of no service to the sick. A man is seized with apoplexy, and the bystanders say his father died of the same malady. You give half an hour to the investigation of the apoplexy, and half an hour to investigating the age, habits of life, and mode of death of the father; and you become convinced that the disease before you is apoplexy. Should you bleed? You have some tables at home which show you how many people have been bled in apoplexy with good results; and these are connected with other tables which show you their ages, and these with others which shew in how many it was hereditary. The pupils are contracted, and the table shews only forty-nine per cent. of recoveries after bleeding when the pupils were contracted; but the pulse is full and slow, and the tables shew fifty-five per cent. of recoveries where the pulse was slow and the patient bled:—then the breathing, the degree of intelligence;—more tables! Who is to decide amidst such thickly serried columns? At length you ascertain that fifty-nine per cent. of apoplexies at that time of life, and hereditary, are benefited by bleeding, and you resolve to bleed; but in the mean time the patient is dead. Still, the case will add another unit to the tables, and shew your accuracy of method. If the patient had been bled at once, he might have lived; but what would have become of science? and how could you have held up your head in the Medical Society of Observation?

If we take a case leaving more time for calculation, the advantage to the numerical methodists is not practically so great as they imagine. Suppose that we have been disciples of the school in which every fever is represented as a gastro-enteritis, this notion will of course very much affect our practice. To be delivered from this, observation is necessary, but numeration is of little importance. If we become convinced that fever exists in five cases, or in two, or in one in a hundred, without gastro-enteritis, the fact is as practically useful as if the proportion instead of five was fifty per cent. These are surely the facts which are to be

weighed rather than counted. But, with all this, we admit, that for scientific purposes, and for the advancement of statistical knowledge, the numerical method is essential. Our objections apply only to its abuse; or to a reliance upon it in single cases, as the guide to safe practice.

The enumerators tell us, that fatty transformation of the liver is more frequent in phthisical women than in phthisical men; and ulceration of the epiglottis, larynx, and trachea more frequent in phthisical men than in phthisical women. This kind of fact will scarcely find general acceptance, resting wholly upon figures. It will always be considered doubtful. Now, if admitted, is its importance considerable? for no figures would convince any man of tolerable experience that the exceptions as regarded the ulcerations of the larynx, &c. were not so numerous as to make the arithmetical balance practically little worthy of regard. Yet M. Louis would call the person not keeping the account ever before him no observer, but, in derision, a "man of genius;" whilst the person who disregarded the laryngeal symptoms on arithmetical principles, and took his determined stand upon the liver, would be held in honour, as a man who knew that two and two made four.

"Up to this time," exclaims M. Louis, (for his language is rather declamatory in this essay), "people have never or rarely counted: well! of what malady, except some eruptive affections, in which it has been very imperfectly observed, of what malady do we know the mean duration? Books of pathology say, indeed, it is true, that the age, the temperament, &c. have an influence on the march and duration of maladies: but where is the rigorous proof of this assertion? Where is the measure of this influence? No where to be found. And how, in fact, could they be attained except in the mode which has been indicated." (P. 33.)

This kind of language reminds us of poor Dr. Armstrong. It is assuredly unjust to say that the mean duration of maladies in general is unknown. It is equally erroneous, if we may trust to our own reading, to say that books of pathology lay great stress on the age, or on the temperament, as influencing the *duration* of maladies. If M. Louis thinks there is no proof that the age and temperament influence the *progress* (or march) of maladies, it says little even for his boasted method of observation. Besides, there are many diseases of which if we know not even the mean duration, we should not attain to it by numeration. What numeration table would satisfy any experienced man concerning the probable duration of epilepsy, or of mania, or even of a continued fever, the duration of which so many circumstances may prolong. M. Louis does wisely to laugh at the "man of genius;" for his numerical method is found to be the idol and practical guide of the dull alone. But he does unwisely to vaunt himself so unseemly; for he throws his past and great merits somewhat into the shadow. He has gained a high place, but he has grown giddy.

It is with reluctance and concern that we make these comments upon the manner of M. Louis; and we regret it the more because we are convinced that his challenging way of bringing forward the numerical method will create in many readers an indisposition to acknowledge its unquestionable merits; for that many facts are taken upon trust, on small foundation, cannot be denied. The causes of diseases are often arbitrarily and often erroneously assigned; their symptoms are not always well distinguished; and, perhaps, above all, the foundations of therapeutics

are extensively unsound, and require inspection, consideration, and even the correction of the numerical method. Exposed to this test, the general opinion of many medicines would, we believe, undergo considerable modification; but to make therapeutical experiments conclusive, we must have calm observers, not anxious to catch a little fleeting fame by magnifying the merits of a new compound; and, as another preliminary, of small dignity but vast importance, we must have authentic drugs. Unless our information is very incorrect, there are not many prescriptions faithfully prepared in the British dominions. We believe there is scarcely a medicine, however simple, which the chemist's art cannot imitate in cheap and base material. There are many industrious "commercial gentlemen" in the chemical line, whose section of the business it is to supply the materials of adulteration to the brewer, the baker, and the retailer of drugs; and yet we eat and drink with indifference, and physicians prescribe with calm satisfaction. We fear these evils will be proof even against the numerical method; for so long as factitious drugs are given, there are no credible therapeutics to be counted, and the medicinal treatment of diseases is overlaid with fallacies.

If ever the time should arrive when in the majority of instances patients receive from the druggist precisely what the physician prescribes, pure and unadulterated, real roots, pure powders, and unexceptionable extracts, the numerical conclusions must still be received with caution, and with a continual remembrance that two cases can seldom be in every respect alike. Nor will this caution really vitiate the just numerical conclusion, any more than the general conclusions to which every practitioner arrives as results of experience. The numerical conclusion ought even, if carefully attained, to be more exact in itself, although it may not be, and we believe will not prove to be, more exact or useful in reference to single cases, the event of which must ever be regulated by individual modifications and complications. "To group facts according to their resemblance, and then to count them," is unquestionably one of the most likely ways of advancing towards truth; but all depends upon the rigid correctness of the groups. The whole question is, whether he who registers his facts in tables will be better guided by the numerical majorities he finds on adding up his columns, than he who reflects and registers his reflections in his mind. If both registers were kept with equal correctness, there could be no question on this subject. The written register would be infallible. But in the mental register, we apprehend that the material resemblances alone would be registered; whilst in the more formal tables the groups would often comprehend discordant particulars, especially in relation to the constitution and habits of patients, and the practical conclusions would be unsafe. In estimating the average success of surgical operations (one of the applications of the numerical method mentioned by M. Louis to recommend it), we think the fallacy incidental to mere counting is obvious. We may seem to dwell too much on these points; but we regard nothing with so much apprehension as a supposed infallibility in any part of the art of medicine. It is the mechanical exactness of the numerical method, supposed by its supporters to be its strongest recommendation, that makes us suspicious of it.

M. Louis furnishes some illustrative tables, which the reader may consult with interest, if not with advantage. Their tendency is certainly not

to remove what may be our prepossessions. Let any one consider the number of tables and sub-tables required to present accurate numerical results concerning twenty cases of continued fever alone. First, a table of symptoms, subdivided into a table of ages, habits of life, and duration of the malady; a table of tongues, with a sub-table of habitual states of tongues in a certain portion of the patients; a table of the continuance of the malady, with sub-tables of the accidental causes of prolongation; a table of pulses, with a sub-table of peculiarities;—these and half a dozen more tables and sub-tables of treatment, diet, and examinations after death, would alone be likely to lead to any accurate calculations. Whether they would really lead to much or not is a question with which it is unnecessary to harass ourselves, for we may rest assured that such a method of mental progression will never be tried, to adopt the numerical language, by ten per cent. of medical practitioners.

It was our intention to have added to this notice of M. Louis's preliminary essay, the analysis of some of the medical memoirs which follow it, several of which, being on subjects rigidly admitting the application of the numerical method, are excellent illustrations of its real value. Our limits, however, oblige us to postpone this to another number.

ART. XI.

An Experimental Essay on the relative Physiological and Medicinal Properties of Iodine and its Compounds, being the Harveian Prize Dissertation for 1837. By CHARLES COGSWELL, A.B. M.D.—Edinburgh, 1837. 8vo. pp. 179.

THIS is a work of considerable labour and research, and well merits the honour of being crowned with the Harveian Prize. It comprehends an excellent digest of the physiological and therapeutic effects of iodine and its compounds, derived from the most authentic sources. As the author himself states, where a blank seemed to exist he has sometimes attempted to supply the deficiency; where statements were found to clash, it has been his endeavour to shew in what manner they might be reconciled; and in doubtful points, although he has carefully examined the observations of other men, yet he has trusted only to his own. The whole is executed in a methodical and distinct manner, and displays much of that sober spirit of induction, which may be said to characterize the school to which he belongs.

The medical properties of iodine, as is well known, were first brought into notice by Dr. Coindet of Geneva, from the idea having suggested itself to him, that, the ashes of the *Fucus vesiculosus*, long celebrated for the cure of bronchocele and other glandular enlargements, might owe their specific virtues to the possession of this ingredient. This was in a memoir presented to the Helvetic Society in August 1820. The wonderful success which attended his treatment of the endemic malady of his country, goitre, at length enabled him to triumph over the professional opposition and even malice with which the first communication of his valuable discovery was so strongly visited.

"The wide extent of diffusion in which iodine now appears to exist throughout nature, magnifies its claims to every species of investigation. Davy detected it in six marine plants (Phil. Trans. 1814), and Dr. Fyfe added the sponge (Ed. Phil. Journ.), since which it has been shown to be present in several marine animals, in various European and American saline springs, (United States Dispensatory), in ores of silver and lead from Mexico, and by M. Mentzel in a zinc-ore from Upper Silesia (Feruss. Bull. 1828.) Lastly, two phænogamous plants of the genera *Salsola* and *Alôe*, growing in the interior of America, have been found to contain iodine." (P. 14.)

But a most remarkable fact, and which may be said to furnish an additional link in the chain connecting organic and inorganic matter, has escaped our author's attention, namely, that an insect has been found near Ascoli in Italy, which Savi has described under the name of the *Julus foetidissimus*, containing iodine. The animal emits when disturbed a yellow fluid strongly smelling of iodine, and which immediately strikes the characteristic violet colour with starch.*

There is a schism among medical writers regarding the manner in which the agency of iodine on the living body is affected by chemical union. Some are of opinion that iodine and its compounds all follow essentially the same line of action, while others maintain that the compounds in question "combine the properties of the substance united to the iodine with those of the substance, and operate accordingly."

"As these theories cannot be both true together, the writer imagined, correctly or not, that the path intended for him to pursue was that of making original investigations, with a view of determining whether either, or which of them, had its foundation in nature. Unfortunately, he considers that his results do not distinctly warrant him in adopting the conclusion of either party." (P. 11.)

It would be a difficult matter to reconcile the statements promulgated by practitioners of repute as to the excessive doses of iodine which can be borne without detriment to the system, in opposition to physiological experiments, did we not hold in mind the fact that this element in certain officinal preparations is prone to spontaneous changes, and liable to modification or decomposition from various substances it may encounter in the first passages. Thus with albumen it forms a compound insoluble in water and wine; beer or cyder convert it into iodic and hydriodic acids.

"If there are facts on one side which seem to shew that iodine is devoid of energy, it certainly would not be easy to draw the same inference from those we have just mentioned, more particularly the results of Orfila. The latter are likewise more in accordance with general experience. While examples of inertness, therefore, are comparatively rare, they constitute no rule, but must rather be regarded as exceptions to a general law; nor, in the present instance, do the exceptions appear to be entirely inexplicable, by reference to the tendency of iron to combine with various constituents of our common nutriment." (P. 27.)

Roulin has attempted to shew that iodine, chlorine, and bromine are identical in their therapeutical effects. This notion, however, is based rather on imperfect truth than fundamental error. Their effects are congenerous in as far as they are all powerfully irritant and corrosive. In the fatal cases recorded by Zink and Jahn, the iodine exerted a marked irritant action on the digestive apparatus.

Dr. Cogswell next proceeds to describe the injurious effects of iodine

* Magazin für Pharmacie, Bd. xxi. p. 31.

on the several systems of the body. Thus, in the alimentary canal it is capable, as above mentioned, of producing notable disorder in the primæ viæ. Touching the nervous system, we are told, that, "generally speaking, when iodine begins to disagree with a patient, one of the foremost among the bad symptoms is a sensation of giddiness or headach, or both united." Hence great caution is enjoined in the use of iodine by Sir Benjamin Brodie, who asserts that it has been known to occasion paralysis. We ourselves have repeatedly seen affections of the nervous system approaching paralysis produced by it, namely, extreme irritability, trembling, and jactitation. These effects were of long duration and were accompanied by general emaciation. The whole tract of the air passages seems to be under the influence of iodine, as is proved by its giving rise to hæmoptysis and other symptoms of thoracic irritation. On the salivary glands it has a peculiar action, as indicated by its both determining and arresting ptyalism. In the liver and spleen it has been found to occasion inflammation. Udall expressly states, in connexion with its producing hepatic irritation, that patients recently cured of ague relapsed so soon as iodine came to be administered to them.* In reference to the generative system, it may be averred that iodine is a powerful stimulant to the uterus; it has determined atrophy of the mammæ and testes. Lugol believes it to be a diuretic, and Udall confirms this opinion. Dr. Cogswell has failed to notice the singular circumstance mentioned by Hoffmann and Udall of its darkening the urine and causing the precipitation of a black deposit. The last-named observer instances the case of a person in whom this phenomenon occurred, who had within a space of six days about four grains and a half of iodine and double that quantity of hydriodate of potash, and which lasted for several months, although he had only taken the medicine during ten days.†

On the skin it produces sometimes a dingy smoked appearance, at other times gives rise to erythema and pustules. Dr. Cartwright, of Natchez, has mentioned a singular phenomenon incident to the application of ioduretted ointments; namely, their causing a total loss of sensation in that part of the integuments on which the friction is made, and sometimes extending the paralyzing influence to those parts which are supplied by the same nerves of sensation.‡ It is not only a stimulant to the absorbent system, but in some instances appears to exercise a modified influence over the nervous or vital actions of a diseased surface, as pointed out by Mr. Key. Udall entertains the opinion that iodine produces emaciation in virtue of its powerful incitement of the absorbent system. Hence the emaciation proceeds faster in children than in adults. Dr. Gräfe, of Berlin, employed iodine as an "emaciating agent" with complete success in a case of *polysarcia*, after having used active depletory means with partial relief only.§

To the question, whether iodine be a cumulative medicine, Dr. Cogswell has not been able to give a very satisfactory answer. The experimental enquiry instituted for this purpose turned out to be both complicated and

* Diss. de Effect. Iodini Havniæ, 1833.

† Op. cit.

‡ American Med. Recorder, No. xlv.

§ Hobson on Iodine, in Transactions of the Medical Society of the State of New York. Vol. II. p. 299.

difficult. His experiments on the urine of individuals taking the medicine led to approximative results only.

"I think we can deduce from the above results that iodine taken, as it has been usually prescribed, in the form of tincture mixed with water, becomes less readily absorbed, and probably remains longer in the bowels, than in the other forms already mentioned. Sometimes the *iodine* made a quick passage to the kidneys, but in the greater number of cases its appearance in their secretion was exceedingly tardy, unless indeed it made its escape in the intervals of testing the urine, which does not appear probable. It even varied in the same individual at different periods, appearing, departing, and reappearing without any evident cause. The same was not the case with the iodide of potassium, and perhaps of iron; the former is even noted for passing rapidly out of the system." (P. 62.)

Were we asked what are the chronic ailments in which iodine has been employed, we should answer, that there are few, if any, in which it has not. When judiciously administered, it improves the appetite, strengthens the patient, proves diuretic; is, in short, a powerful *energizer* of the functions of nutrition. Hence its utility in struma; in atonic affections of the uterus; in secondary lues complicated with scrofula, where there are diminution of strength and morbid irritability; in hydropic effusions; in varicose veins; in visceral enlargements, &c. A remarkable example of the curative effects of iodine in Angina pectoris occurred in the person of Dr. Oliver, of Massachusetts. He took the medicine dissolved in alcohol, of the strength of twenty grains to the ounce, thrice in the day, beginning with six drops, and gradually increasing it to sixteen or twenty. "I think," says Dr. O., in a letter to Dr. Silliman, "that I derived as much benefit from the iodine in a fortnight as I had from the solution of sublimate in eleven months, and indeed I may say much more."*

The iodic preparation now generally employed is the hydriodate of potash; although, according to our author, the combination of this with iodine is the form most generally approved of, whether for internal or external use. We should be here in a like perplexity as we were in respect to iodine, as to the quantity of the hydriodate which may be exhibited with impunity, were we not aware that the salt of commerce is oftentimes very impure. This source of error, which appears to have misled some of our most enlightened physicians, may be ascribed either to spontaneous conversion of the iodide of potassium into carbonate of potash, through atmospheric influence, or to its adulteration with the same drug by fraudulent venders.

"For an easy method of detecting the sophistication with carbonate of potash, I am indebted to Dr. Traill, who practised it as early as 1826. Tincture of iodine, added to the suspected compound, loses its colour if the carbonate be present. Mr. Pereira proposes to immerse one of the crystals in a little lime-water, when the presence of carbonic acid will be indicated by the fluid being milky. To test for common salt, the same pharmacologist describes a process founded on the capacity of ammonia to dissolve the chloride, but not the iodide of silver. On adding nitrate of silver to the suspected sample, any carbonates, chlorides, and iodides are thrown down; digesting the precipitate in ammonia, you take up only the carbonate and chloride; and, on adding nitric acid to this solution, the carbonate is converted into nitrate, and the chloride is precipitated." (P. 87.)

It is fully established that the hydriodate of potash may, and does .

* Hobson on Iodine, in lib. cit., p. 299.

exert a powerfully irritant action on the digestive canal. It may likewise induce deleterious effects on the nervous system. It has been known to give rise to coryza in the air-passages, pustules on the skin, pytalism, diuresis. As to its medicinal effects, it seems to be less efficient in goitre than simple iodine. In scrofula, though powerful, it takes longer time to manifest its action; but, again, there is less danger in continuing it a sufficient length of time. Of its utility in phthisis we believe that no positive proof is on record. It has failed in nervous complaints, where iodine has been of remarkable benefit. We concur with what we ourselves have heard expressed by our most distinguished hospital physicians and surgeons, that the dose of hydriodate of potash ought not to exceed five grains. The above quantity, repeated at suitable intervals, and continued for a period of weeks, will prove advantageous wherever the remedy is indicated. When we go beyond this measure, we are apt to induce iodism, or, at all events, gastric derangement.

The iodide of sulphur has been employed successfully for the external treatment of skin diseases; as a resolvent of the tubercles in lupus and acne indurata, in inveterate lepra, in impetigo figurata, and in tinea.

Two compounds of iodine with carbon are described, a protiodide and a sesqui-iodide. Both are endowed with intensely irritant qualities. The sesqui-iodide seems to be allied to the toxicological group of Strychnos and Brucea; inasmuch as its chief influence appears directed to the spinal marrow, causing convulsions and difficult breathing, but no cerebral disturbance. The protiodide has been used with advantage as an outward application to lepra and porrigo, in an ointment containing half a drachm of the powder to six drachms of simple cerate.

In the compounds of iodine with the electro-positive elements, the energy of the latter seems to preponderate. Thus, the iodide of iron, administered in large doses, produces effects on the system analogous to those of the sulphate. Dr. Cogswell has ascertained by experiment,

“1. That it (iodide of iron) acts as a local stimulant, with the power of effecting peculiar disorganization. 2. That its action is more particularly directed to the tract of the alimentary canal. In these respects, although it may bear some faint degree of resemblance to iodine and hydriodate of potash, yet the details are far from corresponding with much accuracy.” (P. 132.)

A little further on, the author remarks, “The iodide of iron seems rather to affect the character of a ferruginous than that of an iodinous compound.” One great defect of the hydriodate of iron, and which must militate against its employment in practice, is its easy decomposition.

Two iodides of mercury have been introduced into practice, the protiodide and the biniodide. The former would seem to be a compound of no inconsiderable energy. It has chiefly been exhibited in syphilitic affections implicating the cutaneous tissue. The biniodide is, without doubt, a most powerful irritant. Lugol considers it nearly as escharotic as corrosive sublimate. Dr. Cogswell is not acquainted with any specific example of its causing salivation. We, however, have witnessed this result follow the continued application of a minute quantity, in the form of ointment, to ulcers in the legs.

In terminating our analysis of this work, we subjoin the summary of the author's conclusions from the facts collated by him.

"1. That iodine and hydriodate of potash act very much in the same way, but that there is still a difference, not merely in point of power, but of specific properties. 2. That whatever be the proper action of the iodide of sulphur, its facility of decomposition gives it a resemblance to iodine. 3. That the iodides of carbon, so far as examined, have an action peculiar to themselves. 4. That, in those metallic iodides which were submitted to examination, the preponderance of power is on the side of the bases." (P. 167.)

ART. XII.

Memorie della Società Medico-Chirurgica di Bologna. Vol. I. Fasc. 1, 2.
—Bologna, 1835, 1836.

Memoirs of the Medico-Chirurgical Society of Bologna. Vol. I.
Fasciculus 1 and 2.—Bologna, 1835-6. 8vo. pp. 223; with Plates.

THIS Society has published, for the last nine years, the *Bullettino delle Scienze Mediche*, a monthly Journal, consisting of extracts from foreign Medical Journals, and concise Reports of the proceedings of domestic and foreign Societies; but it is only since the reestablishment of its meetings, that had been suspended from 1831 to 1834, in consequence of the political events of that period, that it has published its own Transactions in a separate form. The first fasciculus was printed in 1835, the second in 1836, and each contains five articles; which, although rather deficient in originality and importance, display a pretty accurate acquaintance with the labours of foreigners in the same departments. Indeed, we have frequently had occasion to remark, that the Italian physicians are much better read in English medical literature than our nearer neighbours, the French, whose vanity, or ignorance of what is going forward in other countries, causes their honesty to be occasionally called in question when asserting their claims to new discoveries. Although the number of papers selected for publication in the Transactions is small, yet the quantity of writers among our Bolognese brethren is far from being so; for eight out of the twelve memorials read during the session 1836 to the Academy of Sciences were upon strictly medical subjects; some of which we shall have occasion to notice among our Selections.

The first paper is by Dr. BREVENTANI, upon the auscultatory Signs of Pregnancy, as observed by him in forty women supposed to be pregnant, who all, except one not pregnant, exhibited not only the placental murmur, synchronous with the maternal pulse, but also the double foetal sound, of the existence of which there could be no doubt, from its similarity to the sounds of the heart in the new-born infant. The placental murmur was never heard before the third month of pregnancy. At first it was perceptible in one spot at the lower part of the hypogastric region; and, as the uterus increased in bulk, it became more marked on one side, usually the right, and over the uterine enlargement; always preserving, however, its relative position with regard to the uterus. At first the sound is continuous, of greater or less intensity, but always louder than at a later period, and diminishing during the progress of pregnancy, till it is marked by the foetal sound. Once, when the belly was slightly pressed by the stethoscope, the murmur was heard at the upper and

anterior part of the uterine tumour; and then, by continuing the compression, it ceased, giving place to the sound of the foetal heart.

In common with Dubois, Evory Kennedy, and others, Breventani ascribes the placental sound to the uterine vessels in contact with the placenta; and he controverts the opinion of Bouillaud, that it is owing to the pressure upon the iliac arteries, by adducing a case in which he heard the placental murmur and the sound of the iliacs at the same time. The beatings of the foetal heart are not long in making themselves heard after the uterine murmur; but they are usually detectible a little after the fourth month, although before the motions of the foetus are perceptible. They generally diminish in frequency from the first moment they are heard to the end of pregnancy; but occasionally they become too rapid to be counted. The greatest number of double foetal sounds heard was 149, the least 120, in the minute. The situation and extent of surface over which they are audible varies with the motions of the foetus.

The next article is an account of two cases, with the dissections, from the pen of the same writer, in which the sounds of the heart were heard at a distance from the patients. In one of them a whizzing sound was audible at the distance of three paces, synchronous with the pulse, and proceeding from below the sternal end of the right clavicle, at which spot a purring tremor, contemporaneous with the pulse, was perceptible to the touch. The diagnosis was hypertrophy, dilatation of the aorta, and contraction of its orifice. The sound varied in intensity at different periods, and, a little before death, was only audible within a few inches of the chest. The diagnosis was confirmed by the dissection; but the valvular contraction was owing to an increased growth and development of the fibrous coat of the aorta, under the form of tubercular elevations projecting far into the interior of the vessels, obstructing its orifice, and extending in scattered clusters of less size along the descending portion. This degenerated structure contained some traces of atheromatous matter. The symptoms and morbid appearances of the second case resembled the first in every respect, but that the aortic valves were ossified, and the vessel was simply dilated.

Notwithstanding that, in almost all the dissections of such cases, the same morbid states have been found, yet, as there are numerous instances of the same lesions unattended with this peculiar loudness of sound, and that the noise itself varies in intensity, and in some cases has become inaudible except by the stethoscope, at the same time that the heart's impulse was augmented, the author is disposed to attribute it to a state of rarefaction of the blood, either by "the expansile vapours" of Rosa, or "the gases" of Testa, or "the nervous fluid" of Lobstein; as it is well known that thin fluids, when shaken, produce a far louder sound than thick ones.

Doubtless the crasis of the blood is liable to frequent variations in the same individual; and the phenomenon of the "bruit de diable" of Bouillaud and others goes far to prove that a poor and watery state of the circulating fluid is a condition favorable to the production of sound in the containing vessels; but, with respect to the three expansile or dilating matters above mentioned, we must withhold our assent to an explanation resting upon notions so purely hypothetical.

There is a very good digest of all that is known upon the subject of

Lithotrity, by Dr. BARONI; followed by a Latin dissertation by the president of the Society, Dr. VALORI, upon the question whether Celsus was a practitioner as well as a writer; which question, from the internal evidence afforded by his works, is decided in the affirmative.

The last paper in the first Fasciculus is upon the Symptoms, Laws, and Peculiarities of the Cholera, by Dr. VERSARI; which, though a valuable compilation, contains little that is original, either in theory or practice. Dr. V. is inclined to refer the remarkable absence of tears and complaints in the patients to the general arrest of all the secretions, except those from the stomach and bowels, rather than to any peculiar state of the sensorium; though he does not altogether deny that this may have some influence. Our own experience on this point, derived from the observation of some hundreds of cases, leads us to attribute this curious state of apathy to a "lesion of innervation," produced by the retarded flow of highly carbonized blood through the brain, that deadens the perceptive faculties as well as weakens the muscular powers; besides that the mere want of the secretion of the tears is not sufficient to account for the absence of all other signs of grief and terror. In speaking of the empty state of the arteries, Versari quotes a cruel experiment of Dieffenbach's, mentioned in the *Annali* of Omodei, No. 184, in which an elastic tube was passed from the upper part of the brachial artery to the heart of a patient in his last agonies, without giving exit to a particle of blood. He considers cholera to be a peculiar disease, affecting more particularly the ganglionic system, and secondarily those organs, as the stomach, bowels, &c., with which it has the closest connexions. Like most of the Italian physicians, he adopts the opinion of its contagious nature, but not without a severe scrutiny of the arguments for and against it. Of all remedial means, he places the greatest reliance upon an early bleeding; and speaks very favorably of the use of ice, swallowed slowly and in small quantities. In the cold stage he uses stimulants, astringents, and aromatics; but with little expectation of a favorable result. The consecutive fever he treats, in the usual manner, with salines and diluents.

The first article of the second Fasciculus is an account of a Monster by Defect, by Dr. SPSSA, of Crespino. This monster appears from its size, (sixteen inches long,) to have been born at the full period, and presented the following anomalies:—There was no connexion, by means of an umbilical cord, between the fœtus and the placenta, the fœtal portion of which was wanting; and, where it was attached to the uterus for a hand's breadth, it was thin and broken down. The liver and small intestines projected through a large round opening in the abdominal parietes, four inches in diameter, the edge of which was cicatrized, except for a space of three inches towards the left hypochondrium, where it was sloughy. The cord, had there been one, would have occupied the centre of the opening. An immense spina bifida occupied the whole vertebral column, from the lower part of the neck to the coccyx, containing five pounds of straw-coloured serum, and communicating with the spinal canal by numerous openings, caused by the absence of the spinous processes. The medulla, that floated in the fluid, as well as the brain, was very soft and diffuent. The feet and legs were turned inwards, so that the toes met each other. The pelvis was very narrow, and there was neither anus nor organs of generation. There was no sternum, nor car-

tilages to the ribs. The thymus was large; the lungs were merely rudimentary, and the pericardium and mediastinum were absent. The want of a diaphragm made but one cavity of the chest and abdomen, in which were neither spleen, omentum, nor stomach; for the œsophagus passed directly into the small intestine that terminated at the ileum in a cul-de-sac. The pancreas was small, and the kidneys had neither pelves nor ureters; nor was there any bladder or internal sexual organs; indeed, the pelvis was an irregular mass of bones, without any cavity. The liver was of an irregular form, and the gall-bladder was deeply sunk into its substance. The remains of the umbilical vessels were impervious cords. The distribution of the blood-vessels and nerves was as regular as it could be with the absence of so many important viscera.

The following is the explanation given of this extraordinary monstrosity:—1st. That there was an original defect of all the absent parts, together with the existence of an umbilical hernia, containing the liver and intestines. 2d. That this, compressed by the uterus against the brim of the pelvis, became inflamed and sloughed, forming the large opening into the abdomen, (not entirely healed at the time of birth,) besides separating the union of the fœtus and the mother; and that, 3d, The fœtus, thus separated, was enabled to maintain its independent existence nearly to the period of birth; and that this separate existence continued some time is evident from the healing of the edges of the abdominal opening, and the contracted state of the remains of the umbilical vessels. The author does not attempt to explain the way in which the nutrition of the fœtus was carried on at this period; but mentions a case from Poujol, where the umbilical arteries were both impervious. It is to be regretted that no account is given of the contents of the intestines, nor of the lymphatics and lacteals; as, by means of an examination of these organs, some light might perhaps have been thrown upon the *vexata quæstio* of the nutrition of the fœtus; particularly as it has been strongly denied by Adelon, that a fœtus has ever arrived at maturity whose umbilical cord has been divided while in utero.

In the same Fasciculus is another account, by Dr. LUIGI CALORI, of a Monster by Defect, in which, externally, the nose and nostrils were wanting, while a hare-lip and cleft palate converted the mouth and nasal fossæ into one cavity. To this deformity was added a vast serous cyst within the skull, compressing both hemispheres, particularly the right, of which only a rudiment remained. The corpora striata, the olfactory nerves and lobes, the commissures, the septum, fornix, and the mamillary processes, were all absent, and the carotids were much smaller than usual. There was no æthmoid bone, and the sphenoid was deficient in those parts that are connected with the nose; besides that there were many minor deformities within the skull dependent upon these more important anomalies. This "monster" lived eleven days, and then died jaundiced; but its death appeared to result from inanition, from the difficulty of taking food, rather than from any disease.

Professor Calori considers this case to be confirmatory of the law laid down by Meckel, "that, when the facial portion of the skull is deficient, the anterior part of the brain is imperfectly formed;" likewise, that the proximate cause of the deformity was (in accordance with the views of Serres,) the want of development of the carotid arteries, upon which

these parts were dependent for their nutrition; the remote cause being the pressure exercised upon the parts within the cranium by the contents of the serous cyst; the formation of which he attributes to inflammation set up at an early period of the fœtal existence. With but little respect for the tender and musical ears of his countrymen, Dr. Calori has denominated this monstrosity as *Coloborrhinocephalus Fissilabrus*!

The two remaining articles are upon Scurvy, as it appeared in the prison at Narni; and on a case of Ossification of the Costal Pleura and Cartilages, attended with dyspnœa, constant, and unrelieved by any posture or mode of treatment: neither paper, however, has any claims to originality, either in its pathology or treatment; indeed, there is a great deficiency of this quality in most of the Italian periodical publications, which is hard to be accounted for, consistently with the knowledge of the works of contemporaneous authors therein displayed, unless it arise from the Italian physicians being fonder of reading than of observation.

ART. XIII.

A Medico-legal Treatise on Homicide by external Violence; with an Account of the Circumstances which modify the Medico-legal Characters of Injuries and exculpatory Pleas. By ALEXANDER WATSON, Fellow of the Royal College of Surgeons, Edinburgh, and one of the Surgeons to the Royal Infirmary, &c. &c. &c.—*Edinburgh*, 1837. 8vo. pp. 355.

MR. WATSON, the author of the work before us, has long been known to the profession as a contributor of articles on Medical Jurisprudence to a respected contemporary Journal. That he has been a most industrious labourer in this department of science, the treatise which he has now published sufficiently shews. But other circumstances have concurred to place him in a most favorable situation for collecting and recording facts. There are probably few in Edinburgh, and we may confidently say there is not one medical practitioner in London, who can boast of having been consulted judicially in one-half of the number of cases in which we find, by this volume, Mr. Watson has been personally engaged. We rejoice to perceive that this golden opportunity of collecting interesting medico-legal facts has fallen to so shrewd an observer, and, generally speaking, so cautious a reasoner. The work, we are assured, will be welcomed not only by all professed medical jurists, but by those who have not yet been led to consider medico-legal knowledge as of great importance to a practising surgeon. We invite the attention of the latter class in particular to this volume. The great bulk of it refers to the duties of a *surgeon*; and its perusal will suffice to shew, not only that the kind of information which it contains is not to be obtained either from an attendance on surgical lectures or from a perusal of the works of surgical writers, but that, without this knowledge, the personal reputation of the practitioner is endangered, and the course of public justice must be embarrassed.

Although Mr. Watson's work is especially devoted to a consideration of the subject of Homicide by "external violence," yet we find treated

of, the varieties of homicide by asphyxia, spontaneous combustion, death by lightning, and infanticide. We do not object to the introduction of these accessory subjects; but the somewhat hasty manner in which they are dismissed rather tends to confuse than enlighten the reader. This, as we shall presently see, is particularly the case with the chapter on Infanticide; some of the most important questions connected with which are discussed in two or three pages. One striking feature in the volume is the number of cases which are distributed through it. Many of these have been derived from the author's personal experience, but with others, extracted from the works of Sir A. Cooper, Sir C. Bell, and Mr. Travers, most of our readers will be familiar. We are at a loss to conceive why some of the cases are introduced, since they occupy only a few lines, are unaccompanied with any comment, and merely illustrate facts which must be well known even to a tyro in the profession. Thus, the case of Dr. Hunter (p. 213,) is inserted in *three lines*, to shew that a man may die suddenly, while in a state of apparent health. Had there been fewer cases, and the comments of the author more frequent upon those which are inserted, they would, we think, have served the purpose of instruction better; for, after all, it is not so much their number as their individual value which really benefits the medico-legal student. We trust that this hint will not be lost in a second edition of the work; since, assuredly, the most instructive cases in the treatise are those in which the author has marked out the details, and exposed in clear language the reasons upon which his inferences were founded.

In defining the nature and objects of medical jurisprudence, after stating that "the duty" of the medical jurist is, by his science, in certain cases to assist judges and juries in the administration of justice, we find the following excellent remarks, which should be borne in mind by every practitioner who is called to act in a medico-legal capacity.

"In the exercise of this duty, it is obvious that the medical jurist must neither adopt nor endeavour to support one side or other of any case, when his opinion is required on it, to the exclusion of a just regard to the opposite side. If he did so, the ends of justice could not be accomplished. Such a proceeding would be a degradation of his character, and a prostitution of his profession. When it has been attempted, conflicting opinions have been given by the medical witnesses on the different sides of the case, arising either from a slight distortion of the facts to suit their own particular views, or from ill-judged zeal to maintain the side of the party by whom they have been requested to appear. These medical men have mistaken the nature of the duty which they had to perform. In place of acting as judges on the points referred to them for decision, they have become the advocates and partisans of one side of the case; and, sometimes, what ought to have been an opinion on the facts of the case before them has assumed the controversial character of a speculative debate; and this to the astonishment of the judges and jury, the amusement of the lawyers, the injury of parties, the obstruction of justice, the ridicule and contempt of the public, and the degradation of the medical profession." (P. 7.)

These remarks, which are evidently the result of long observation on the general conduct of medical witnesses, are followed up by a very proper caution, too often neglected, namely, that a medical jurist should never give an opinion on a case without first ascertaining *all the facts* connected with it.

A large portion of the volume is occupied with the medico-legal history of Wounds. It is upon this subject that the writer has evidently

had the largest experience, and has bestowed the greatest attention. Mr. Watson must excuse us if we consider his definition of a "wound" but little calculated to remove the vagueness, the existence of which among medico-legal reports of the present day he makes a formal matter of complaint. Thus, he includes fractures and dislocations among wounds; but he omits ruptures of the viscera, which, however, must fall under his acceptance of the term wound. Fortunately, the necessity for a strict understanding among medical witnesses in England on this point has now passed away. A recent statute (1 Victoria, c. 85,) has already provided a remedy for those defects in our criminal law, to which we called the attention of our readers in a late Number.

The first class of injuries treated of are those affecting the *nervous* system. These are very fully considered, and well illustrated by cases. The diagnosis of concussion, with its attendant symptoms, is well laid down. In speaking of the appearances occasionally met with in the cranium, the author does not omit to state the important medico-legal fact, "that concussion of the brain may prove fatal, without either fracture of the skull, effusion of blood within the cranium, or any other change being observed on dissection." (P. 35.) Cases are subjoined, which fully confirm the truth of this statement.

We next find a report of eight cases, to prove that a *blow of the fist* on the head may cause death, by producing concussion, effusion of blood, or both. The author attempts (but, in our opinion, not very successfully,) to determine whether, in these instances, the injury to the brain is caused by the blow or by the fall on the head, when the party is knocked down; a question frequently put to a medical witness, and the answer to which may have sometimes a material influence on the case of a prisoner. In several subjects which Mr. Watson examined, the chief clot of effused blood was on the side *opposite* to the external mark of injury which occasioned it. (P. 36.) Further,

"Although, in some cases of commotion, (concussion,) no change of structure can be observed in the brain after death, yet we cannot infer from this that lesion of the brain has not been produced. When death happens immediately or soon after the injury, time has not been allowed for subsequent changes to take place, which might have pointed out organic lesion on dissection. Hemorrhage, for example, may be suspended by the depressed state of the circulation which takes place, but afterwards occurs when reaction is established and produces compression of the brain. The same may be said of inflammation, which generally supervenes, if the patient recovers from the primary shock of the injury. Besides, the ultimate structure of the brain is so minute that some organic injuries of it may not be cognizable to our senses." (P. 45.)

An extravasation of blood, as a consequence of laceration of the brain, is often situated within the dura mater; rarely at the place where the blow has been inflicted, but most commonly at the base of the brain. Extravasated blood has been met with from violence, even when there was no external mark of injury upon the head. The question, whether the effusion of blood has arisen from violence or from intoxication and disease, is next examined. In the view of the author, the situation of the effusion may sometimes serve as a criterion of its origin. The effusion from violence is generally situated upon the *surface* of the brain, while that which is the consequence of diseased vessels is commonly

found within its substance. Of course, a fact of this kind can only be taken as *presumptive* of the cause of extravasation. The coexistence of a fracture with effusion of blood on the surface of the organ would render it almost certain that violence must have been the cause. The medical jurist is advised to be careful how he receives the dying declarations of persons who have suffered from concussion, but who have recovered sufficiently before death to converse upon their condition and the circumstances under which they were violently treated. Such persons, it is well known, have, in general, "no recollection of what took place immediately previous to or after the injury;" and, therefore, their statements regarding events that preceded the blow should not be trusted, unless confirmed by other evidence. (P. 57.)

In speaking of Inflammation and Suppuration of the Brain, the author adduces the following singular case, to shew the insidiousness of this process, as well as the slight cause from which it may occasionally proceed.

"CASE 41. William Watt, aged ten, had the birch end of a scavenger's broom thrust into his face two or three times, by one of his companions. By this he was so stunned; that he lay down and was carried home in a state of stupor. He afterwards complained of violent pain in the eyeball and forehead. Symptoms of inflammation and fever supervened, followed by coma, insensibility, and convulsions. These, by active treatment, subsided; suppuration took place in the orbit; but he again relapsed, and died seventeen days after the accident. On dissection, there was an opening in the orbital plate of the frontal bone, which extended into the brain, and an effusion of lymph and pus at the base of the brain. The left ventricle contained three ounces of pus, and communicated with the wound in the orbit. There was also considerable softening and disorganization of the brain. A small portion was partially separated from the orbital plate, and projected upwards." (P. 64.)

As injuries of the brain may, in the first instance, appear very slight, but at the same time may destroy life after the lapse of months, or even years, so should a medical jurist be cautious how he pronounces the individual to be out of danger. He must remember that, in these cases, there may be danger, though it is not apparent, owing to the absence of symptoms. We cannot join our author, however, in believing that any legal difficulty, of the nature imagined by him, would arise, owing to a prematurely expressed medical opinion on these occasions.

"It has often happened," he says, "that the individual, A., has assaulted another individual, B., and given him a blow upon the head. B. does not appear to be much the worse of [for] the injury, so that in a few days A. is taken to the police-court, or before the sitting magistrate, and is tried for the assault. After an interval of a few days or weeks, however, B. is taken ill with inflammation, and dies from the effects of the injury. A. is therefore again laid hold of, and indicted to stand trial for murder. By a law of the land, no person can be tried twice for the same crime; so that a difference of opinion has been given in cases of this kind." (P. 67.)

The author's hypothetical case appears to us to involve the assumption of an assault and a murder being the *same crime*. The plea of "*autrefois convict*" in England is very clearly laid down by Hale, Blackstone, and other eminent authorities. "A previous conviction can now only be pleaded in bar of any subsequent indictment for the *felony*, of which the defendant has previously been convicted." (*Archbold Plead. and Ev.* 80.) It is, therefore, absurd to suppose that a previous conviction for a

misdeemeanour will be a bar to a subsequent indictment for a capital felony.

There are some interesting remarks relative to Injuries of the Trunk and Limbs, causing death by probably sympathetic influence between the nerves and the brain. One point, which is generally omitted by medico-legal writers, is properly adverted to by Mr. Watson,—namely, that “injuries which would not have been fatal individually, sometimes prove so by their combination or multiplicity.” (P. 76.) The witness should not lose sight of this fact, when he is asked by a coroner, magistrate, or barrister, which, of several wounds on a deceased person, was *mortal*? A complication of injuries may as surely destroy life as any single wound which traverses a vital part. In speaking of the cause of death in *burns*, the author quotes the very obscure language of Mr. Travers, in which that gentleman refers the fatal effects to what he terms “functional concussion” of the brain; a form of expression rather tending to confuse than to assist our understanding of the subject. The phenomena of spontaneous combustion are referred, in our opinion very properly, to preternatural combustibility of the body; the ignition always proceeding from some external cause.

The second division of the subject brings us to Injuries of the *Circulating System*, which often demand medico-legal investigation. The loss of from five to eight pounds of blood, according to Mr. Watson, is required to prove fatal in cases of adults; but, in order that death should take place by hemorrhage, it is by no means necessary that a large blood-vessel should be opened. A wound of any very vascular part may be equally fatal. Two interesting cases which occurred to the author, one of which we shall presently notice, fully bear out the correctness of this view. In them the vagina was the seat of injury.

The means for detecting spots of blood on linen and weapons are so imperfectly detailed, that, in our opinion, they had better have been omitted. The author is not correct in stating that “ammonia changes the colour of all other red dyes,” (p. 92,) except the colouring matter of the blood. Alizarine, the red colouring principle of madder, one of the most common red dyes, is not, according to our observation, changed by ammonia.

The consideration of Death by Hemorrhage affords room for the introduction of numerous interesting cases, illustrating the various degrees of mischief to the circulating system under which a fatal result may ensue. We learn from some of these that the loss of a few ounces of blood from the heart, provided it cannot freely escape from the pericardium, may destroy life; that a rupture of the heart or its larger vessels may take place from a blow or a fall, without leaving any traces of fracture or injury externally; that a wound of the internal mammary artery, or of one of the intercostal arteries, may as effectually kill as if the aorta were the seat of mischief; facts, upon the important bearing of which in medico-legal practice it is needless for us to dwell.

The third division embraces Injuries of the *Respiratory System*. After a slight outline of the phenomena of death by asphyxia, the author passes to the consideration of its various forms. His view of the cause of death may be regarded as a kind of compromise between the exclusive opinions of Bichat and his opponents. He allows that life is extinguished

owing to the noxious influence, as well as the diminished quantity of blood sent to the brain. We find nothing new under the head of "Suffocation;" but we are tempted to extract the following instance of "accidental strangulation," from the singular circumstances under which it occurred. It is

"The case of an old woman, a salt carrier, who, on her journey, rested her creel upon a wall: it fell over the wall, and, the belt of it being round her neck, she was strangled by the weight of the creel. The same happened to a fishwoman, near St. Andrew's, a few years ago." (P. 126.)

Throughout the volume we have had occasion to notice a general neglect, on the part of Mr. Watson, of the labours of British and continental medical jurists. In his preface he apologises for this, by ascribing it to the active toils of the practical part of his profession. Willing, as we are, to admit the reasonableness of this apology, it does not form, as we conceive, a justification for the diffusion of an erroneous opinion. An author, who is obliged to trust solely to his own observation, should be proportionally cautious in making generalizations, which may have an important practical influence. We are sorry to meet with a neglect of this proper caution in the statement respecting the effects of a ligature on the neck, "that ecchymosis could not be produced by tying a cord after death." (P. 131.) Had the author consulted the recent works of Divergie and Casper, he would have found that the statement, however true in reference to his own experience, was wholly refuted by the experiments and observations of these distinguished writers. In the section on Hanging, we find also several passages which, we are sure, would have been, under these circumstances, modified or omitted. At page 142 we meet with the following: "If, upon examination, ecchymosis can be observed at any part in the rope-mark, it must have been formed during life."

To the chapter on Drowning we find prefixed some observations on the "physical effects of water in relation to the human body." We must charitably suppose that these have been hastily put together, and printed without revision. It is impossible that Mr. Watson can really entertain the singular notions which we here find announced under the title of the "General Laws of Hydrostatics." He first speaks of "a *square inch* of lead being heavier than a *square inch* of water," (p. 144;) and we next meet with the startling position, that, "when a floating body is depressed or forced down below the surface of the water, its specific gravity is increased by the pressure or weight of the superincumbent fluid upon it." (ib.) We should like to be informed how the specific gravity of a hollow copper ball, for example, becomes increased by submersion; and whether, when sunk, it must not be kept down by a weight proportioned to the difference between its specific gravity and that of water? Mr. Watson seems entirely to forget that there is an upward as well as a downward pressure; and that, in all *floating* bodies,—bodies really *lighter* than water,—unless the cause of their buoyancy be removed, the upward pressure will overcome the weight of the superincumbent fluid, and prevent the body from remaining anywhere but on the surface. The author next refers to what he calls the point of equipoise or equilibrium. As he considers a knowledge of this to be important in its application to the human body, and that it "affords an explanation of some of

the phenomena which occur in drowning," - we shall here quote his remarks:

"If the floating body is depressed to a sufficient depth, its specific gravity will increase until it becomes equal to that of the water. When the body arrives at this point, it will neither ascend nor descend in the water, but will remain stationary. Hence this has been called the point of equipoise or equilibrium. Now, it will be evident that, if the body is raised in any degree above the point of equilibrium, its specific gravity being lessened by a diminution of pressure upon it, the body immediately rises to the surface, and floats. And when, on the contrary, the body is depressed below the point of equilibrium, the increased pressure, by causing an augmentation of specific gravity, makes the body sink with a velocity greater and greater as it descends." (P. 145.)

We must confess our surprise at finding such palpably erroneous statements in a work of this description. A *floating* body, unless the cause of buoyancy be artificially removed from it, can have no point of equipoise except that which is actually close to the surface of the water. Equilibrium is not given to a *floating* body by sinking it to a certain depth, nor can it remain for an instant at this supposed point of equipoise, unless the weight which actually depressed it still continues attached to it. Admitting, however, that Mr. Watson had here given us the true exposition of a natural law, we must have some reason for a body remaining stationary at this point of equipoise; as also for the fact of its rising to the surface, and floating, when it accidentally gets a little *above*, or sinking deeper and deeper, with increased velocity as it descends, when it falls a little *below* that point. What can cause a body to rise higher, or sink with increased velocity lower, than that level in which it is already in equilibrium with water? The author states that "these phenomena are easily demonstrated by experiment." We need hardly observe, that his views upon this branch of physics are wholly inapplicable to the phenomena of death by drowning.

The section on the effects of "excessive Intoxication" is full of valuable practical remarks, for which we must refer our readers to the work itself. We object to the term asphyxia applied to this condition; since it appears to us to be as much a real form of poisoning as in the case of the exhibition of opium or any other poison.

We are much pleased with the chapter on the Medico-legal circumstances which modify Homicide. We think the author has been misinformed respecting the case of the fisherman, who was shot by Captain Moir, in 1830. If we mistake not, the deceased was wounded in the arm, not in the thigh; and died from tetanus, not from erysipelas. The cases in this part of the work are, however, given with great care and precision, and are highly interesting to the medical jurist. One of these, in illustration of the subject of *malum regimen*, after the receipt of severe injuries, we here present to our readers:

"J. Bell was tried at Edinburgh, November 14, 1836, for the murder of John Kerr, aged nineteen, by having struck him a severe blow on the forehead with a hoe, on the 20th July preceding, whereby he died on the 28th of the same month. Kerr was admitted into the Royal Infirmary, under the care of Professor Lizars, on the day after the injury, when he was able to walk to his bed, and did not seem to be very ill. The post-mortem examination, at which I was present, was conducted by Mr. Fergusson and Dr. W. Home, who drew up the official report on the case.

"About an inch above the right orbit, and close to the mesial line, there was a

circular "compound, comminuted, and depressed fracture of the middle part of the frontal bone;" the depression having the appearance of a shallow cup, of about half an inch in depth. The *external* wound of the scalp was like three incisions meeting at one of their extremities; the pericranium was in a sloughy state; the wound was in a state of suppuration. Internally, there was purulent matter between the broken cranium and dura mater; some spiculæ of bone attached to the dura mater, several of which had penetrated through it into the brain. There was some purulent matter and lymph below the dura mater, and softening with abscess in the substance of the brain, about an inch deep at the seat of the injury. The medullary substance of the brain was inflamed, and had a yellow appearance, to the extent of about an inch and a half around the abscess.

"A simple fracture extended from the circular depressed fracture into the orbit; another, about an inch upwards, to the crown of the head. No operation had been performed to remove the depressed portions of bone." (P. 236.)

Upon this case the author makes the following comments:

"This young man was in good health when he received the injury, and evidently died from inflammation of the brain. When he came to the infirmary, the symptoms of inflammation of the brain did not seem to have come on; nevertheless, they were to have been expected with certainty to take place from the irritation produced by the depressed portions of bone on the membranes and substance of the brain, if allowed to remain so situated. Hence it might have been fairly argued, that these depressed portions of bone should have been immediately elevated, and removed from the brain. In such cases, it is too late to attempt this after symptoms of inflammation or effusion have taken place, as the danger cannot then be averted: and, accordingly, as I have shewn above, it is an established principle in surgery to elevate or remove depressed fragments of bone from the surface of the brain, in all cases of compound fracture of the skull with depression, whether symptoms appearing to demand interference have come on or not.

"From the above circumstances in this case, if the person accused had not pleaded guilty of culpable homicide, he would probably have been exculpated on the plea of *malum regimen*. There was no effusion of blood within the cranium, and no concussion of the brain to have proved fatal: hence it might be legitimately inferred that, if the inflammation had been averted by removing the cause of irritation, he very probably would have lived.

"Bell was sentenced to seven years' transportation." (Ib.)

It is well known that the medico-legal examination of wounds is a matter of serious importance, and that much assistance may be often rendered to the course of justice by determining the nature of the weapon or instrument with which a wound on a deceased person has been inflicted. This can only be done by a close anatomical inspection of the injury, assisted by a cautious judgment. Several cases reported in the volume prove that the author possesses that tact and cool reflection which, in moments of difficulty are so necessary to a medical jurist; and without which he must sometimes be exposed to the risk of leading to the condemnation of an innocent person, or of ruining his professional character. Among the cases related by Mr. Watson is that of Mrs. Calderhead, (C. 207.) who had died from excessive hemorrhage, in consequence of a wound which she had received on the left labium pudendi. As the reasoning respecting the means by which this wound had been caused is given in our author's best style, we shall here quote it. It may serve as a useful model for medico-legal reports. Externally, the wound consisted of a clean incision, about three-quarters of an inch in length, having a straight direction, parallel with the margin of the labium. A finger, when introduced, entered into a bloody cavity, as large as a small hen's

egg, from which it might be passed to a greater depth in three different directions,—viz. upwards, towards the under part of the symphysis pubis; downwards, towards the perineum; and backwards, by the side of the vagina and rectum. Its greatest depth was between two and three inches. Now, it appears that some pieces of a broken wine-glass had been found adjoining some of the blood, on the spot where the deceased had received the wound; and it became, therefore, important to determine whether or not the wound could have been occasioned by her having fallen upon these broken pieces of glass. The following is Mr. Watson's remarks on this point:

"1. Let us consider, Is it physically possible that any of the portions of the broken wine-glass, or of any wine-glass in common use, could have made such a wound? A portion of glass, capable of forming such a wound, must have been between two and three inches long, about three-fourths of an inch broad, having a sharp cutting edge, and some degree of point; and it must also have possessed sufficient strength to permit its having been moved about and thrust in different directions, without breaking. To form such a piece out of a common wine-glass, is evidently beyond the utmost ingenuity of man; far less, then, could such a portion have been formed by accidental fracture.

"The broken portion of glass found consisted of the stalk of a common wine-glass, without the foot, having at its upper part the bottom of the cup of the glass, of about an inch in diameter, attached to it transversely. Nearly the whole of the cup was broken off, leaving only a few fragments of the sides, projecting upwards from the bottom which remained. The stalk was about an inch or an inch and a half in length, and its lower part had been clean broken across, leaving no sharp point. It was, therefore, quite obvious that this fragment of glass was completely incapable of forming the wound in question; for the length, form, and sharpness which were requisite were completely wanting, and any wound which could have been occasioned by it must have been quite different in its character. A wound from such a piece of broken glass would have been a lacerated wound, not a clean incision; not larger internally than externally, and could not have had several different directions internally. The upper end of the glass would have made several small lacerated wounds, while the lower end could not have made any wound at all. I, therefore, came to the conclusion that it was physically impossible for the wound in question to have been produced by any portion of a broken wine-glass such as that found." (P. 260.)

In the remarks on Gun-shot Wounds a curious instance is mentioned, in which the exact spot from which a gun was fired was determined by observing two fixed points where the bullets had struck. It occurred at Ayr, in 1831.

"Several shots had been fired into the church through a window. The bullets left holes in the window, and left marks where they had struck, at the opposite side of the church. By these, the exact place (the window of a house on the opposite side of the street,) from whence the shots had been fired was ascertained by mathematical observation." (P. 264.)

Chapter XI. is devoted to a slight sketch of the subject of Infanticide. We cannot say much for the manner in which the author has stated it. There is no attempt at original observation, and, on the whole, it may be considered as a condensed abstract of the essay of Beck on the same subject. Some passages are open to criticism. In examining the question, "Was the child born dead or alive?" we find the following among the "tests" by which it is said "this question is to be determined." (P. 287.)

"If the child died in the womb, it must have happened from five to twenty days previously to delivery, and, consequently, putrefaction must have advanced to a considerable extent." (Ib.)

This statement appears to us to be opposed to numerous well-ascertained facts. May it not happen that a child, which has died in the womb, may be expelled before the *fifth* day? and is it a necessary consequence that, in all children which have died in the womb, and been expelled after five or six days, putrefaction should have advanced to a considerable extent? We must refer the author to Divergie, and those continental writers whose works he has omitted to consult, for answers to these questions. We suspect, upon reference to the experience of others, he would be inclined to modify his statement. The average weight of the fetal lungs is described to be about *two ounces*; of the lungs after respiration, about *four ounces*; both of which estimates we believe to be much exaggerated. The remarks on the hydrostatic test are so imperfect, that we cannot suppose them to have been founded on actual observation. The author says, "If the lungs have respired, they are capable of floating the heart and thymus gland along with them;"—again: "If it (a lung) floats, respiration will be conclusive. Nor is this liable to fallacy; for the air which the lung may contain cannot be so completely pressed out by moderate pressure as to prevent the lung from floating, if it had breathed." (Pp. 290, 291.) We here find no account taken of feeble or imperfect respiration; and rules are laid down, as not liable to fallacy, which, if literally followed, cannot but lead to error. But how are we to define what is to be understood by the term "moderate pressure?" It is impossible that such vague directions as these can render the least assistance to a medical jurist; they must rather tend to make him distrustful of all medico-legal rules on a subject of such vital importance. There are several other passages which we had marked out for comment, but we omit them in the hope that, when a second edition of the work appears, the author will set himself the task of examining more fully what has been done by his contemporaries in this branch of science.

The volume concludes with a short essay on Insanity as a plea of exemption from criminal responsibility, which our limits will not permit us to notice. We consider the author's observations respecting Moral Insanity to be highly judicious; and we join him in the wish that this subject may call forth the attention of our high legal authorities, and lead to some amelioration in the law, which we are satisfied, as it at present stands, must occasionally operate with cruelty and inhumanity towards a very wretched class of beings.

We must here conclude our notice of Mr. Watson's work. We have not hesitated to point out what we regard as erroneous in it. This task, we hope, the author will consider we have executed with fairness and candour. If, in ordinary works, this duty must be performed to guard the public from being misled by their titles or pretensions, it becomes much more necessary in the case of a volume like the present, which, we are assured, will deservedly become one of reference and authority. Our readers may rest satisfied that there is much in Mr. Watson's treatise to repay a careful perusal; and, indeed, to be fully appreciated, it must

be studied, not merely read. The chapters on Wounds, which constitute four-fifths of the volume, embrace every variety of subject and illustration connected with local injuries; and for this alone the treatise deserves to take the foremost rank in British medico-legal literature. It is not only adapted for the medical practitioner, but it is equally fitted to instruct the coroner, the barrister, and the judge.

ART. XIV.

Beobachtungen auf dem Gebiete der Pathologie und Pathologischen Anatomie. Gesammelt von Dr. JOH. FRIED. HERM. ALBERS, Professor der Medizin an der Rheinischen Friedrich-Wilhelms Universität, in Bonn, &c. Erster Theil.—Bonn, 1836. 8vo. pp. 204.

Observations in the Department of Pathology and Pathological Anatomy. Collected by Dr. J. F. H. ALBERS, Professor of Medicine in the Rhenish University of Frederick-William, at Bonn. Vol. I.—Bonn, 1836. 8vo. pp. 204.

It is the ardent wish of Dr. Albers, as it is that of most authors, that his work may be esteemed a valuable contribution to the science of which he treats. His observations certainly afford proofs of great reading and research, and are in themselves both interesting and instructive; but they are too much loaded with quotation, and too deficient in originality, to warrant their being brought forward as a new work in a department in which medicine is so rapidly advancing.

The crying sin which pervades the whole book is a desire of novelty without its reality, which has led our author to invent hypotheses, in place of drawing inferences from facts. In many instances his speculations are so purely fanciful, that they seem to have been contrived more for the purpose of differing from other authors, than from any intention of conveying instruction.

Dr. Albers's first observation is entitled "On the Dropsy of the Ducts of Glands, a disease hitherto unknown;" but it may be fairly questioned how far this name is applicable to the appearances which he has described. The ducts of various glands are liable to obstruction from various causes; the gall-ducts and the ureters are often found obstructed by concretions or by adhesive inflammation, and the consequences are enlargement and dilatation of the ducts. When such obstruction takes place in the gall-ducts, jaundice is a uniform symptom; the ducts swell and dilate, and one of two consequences ensue,—either the bile continues to be secreted till the ducts are stretched to the uttermost, and, being then no longer able to resist the pressure, give way, and effuse their contents into the abdominal cavity; or the ducts are strong enough to resist the pressure, and continue to dilate sometimes to an enormous extent, and the pressure, reacting upon the hepatic ducts, dilate them also throughout the whole substance of the liver.

When this state continues for any length of time the secretion of the bile is altered, and it sometimes degenerates into a watery serous fluid; or, according to Dr. Albers, there ensues dropsy of the gall-ducts. This serous bile has never been observed by him except in cases of

obstruction of the ducts; and hence he ascribes it to a diseased condition of their mucous surface. It seems to us, however, to be altogether dependent upon the state of the parenchyma of the liver, which state may certainly follow as a consequence of obstruction of the ducts; for we have two cases recorded by Andral, in his *Clinique Medicale*,* in which the gall-ducts contained serous bile, without any trace of stricture or obstruction being observable in the ducts. Neither is dilation of the gall-ducts† nor serous bile discoveries of Albers; but our author glories in having been the first to assign them their true place in systems of nosology.

When the ureters are obstructed, either by calculi or inflammation, and the urine continues to be secreted, a gradual dilatation, first of the ureters themselves, and then of the pelves and calyces, takes place, so as in some instances to convert the kidney into one immense sac. It is probable that the chemical nature of the urine is at the same time altered; but of this, as in general only one kidney suffers, it is difficult to adduce positive proof. This state Dr. Albers calls *Dropsy of the ureters*; and in the same way we have dropsy of the parotids, of the testis, pancreas, &c., the dropsy (if such it may be termed,) being evidently an effect or symptom of the original disease, and not the disease itself.

The second observation contains a case of dilated thoracic duct, and its object is to support the view that the thoracic duct communicates with the venous system by frequent anastomosis, and not solely by the opening into the left subclavian vein. We noticed the subject in our Fourth Number, p. 535.

The subject of the next article is *Thymic Asthma*; but we have elsewhere spoken so fully on this affection, that we have only to remark that it is treated by Albers in an able and comprehensive manner. He does not view it as a peculiar disease, but considers its symptoms as common to a variety of affections of the circulatory and respiratory apparatus.

The fourth paper is of considerable merit, but affords another instance of the rashness with which Dr. Albers draws general conclusions from limited experience.

"There is," he says, "a form of softening of the spinal marrow, which commences in the grey matter, to all appearance in the neighbourhood of the central fissure, and extends slowly outwards, affecting the white substance, but much more rapidly upwards, confined to the centre of the cord. It generally commences in the cauda equina, and advances slowly upwards; after a lapse of several years, reaches the medulla oblongata, or extends even into the ventricles of the brain. This partial softening is sufficiently characterized by its peculiar progression from below upwards, but its symptoms and the very gradual and tedious course of the disease are also remarkable." (P. 73.)

In illustration of this form of disease, he gives the history of two cases, in the first of which the following morbid appearances were found on inspection of the vertebral canal.

"The dura mater appeared healthy, but between it and the arachnoid were contained two drachms of a reddish fluid. The arachnoid was partially thickened in the lumbar and lower half of the dorsal region, opaline, and contained three ossified scales of the size of peas. The pia mater was thick and firm, almost tendinous and stri-

* Vol. iv. p. 155. Cases xi. and xii.

† Vide Cruveilhier, *Dictionnaire de Med. et Chirurg. prat.* Art. Foie, maladies du.

ated, but red and vascular. On cutting into the cauda equina, the softened medulla flowed through the cut as a mere pulpy mass of a yellowish grey colour. Towards the fourth dorsal vertebra, a thin layer of white matter, enclosing the softened greyish pulp, adhered to the pia mater, and gradually increased in thickness till the medulla acquired its normal condition in the region of the third cervical vertebra." (P. 80.)

In the second case, the dura mater was healthy; the arachnoid adhered firmly to the pia mater, and contained ossified scales and white opaline patches. The pia mater was very firm, and adhered strongly to the medulla. In the lumbar region, the whole thickness of the spinal cord was reduced to pulp, but higher up the softening gradually retracted towards the centre, till it ceased about the fourth cervical vertebra.

The following symptoms, deduced from these two cases, are given by Dr. Albers as characteristic of central softening of the spinal marrow:—The course of the disease is very slow and tedious, and hence the accompanying paralysis is of very gradual accession, and of very long duration. Sensibility is affected at an earlier period of the disease than motion; and the patient complains of severe pains in the paralyzed limbs, a feature which always accompanies this variety of softening. The lower extremities assume a peculiar shape, analogous to that of the clubfoot, and the sensation as if a tight band were drawn round the abdomen, is experienced very early in the disease. Lastly, gangrene is said to be a rare occurrence in cases of this kind.

It is stated by Dr. A. that central softening of the spinal cord has hitherto been overlooked. This is only partially the case: Andral, Ollivier, and many other authors have noticed it; but we are not aware of any peculiar or characteristic symptoms having been ascribed to it. It could not possibly have been overlooked by Ollivier, who supposes that, in every case of softening of the spinal cord, the change begins in the centre; and, as Dr. A. himself quotes a case from Ollivier, we suspect that it is the importance of the variety which he thinks has been overlooked. Admitting that, in the majority of cases, softening commences in the grey matter, we are not disposed to agree with Ollivier in considering this as always the case; and there are no sufficient grounds, we submit, for denying the occurrence of primary softening of the white matter of the cord. Original softening of the white substance of the brain is generally allowed, and, reasoning from analogy, we should expect a like affection of the white matter of the medulla.

Whether the softening in the cases recorded by Dr. Albers commenced originally in the grey or white substance, we believe it impossible, in the present state of our knowledge, to determine with certainty; but the point is at once assumed as settled by our author, without even the shadow of proof being deemed necessary. The symptoms noted by him as characteristic of central softening are of little value in determining the presence of this specific variety: they might apply equally well to any case of this affection, and to many other diseases of the spinal cord. Dr. A. describes central softening as a *species* of medullary softening: he does not, therefore, agree with Ollivier in the opinion that it always commences in the centre; and he has said nothing of the symptoms which he regards as characteristic of the other varieties of softening, which we might contrast with those advanced as peculiar to central softening. We cannot afford space to enquire in how far the greater soft-

ening of the grey matter in Dr. Albers's cases might be allowed to support his views, or in how far they might be subverted by taking into consideration the morbid condition of the membrane of the cord: there is evidently too much speculation based upon a very narrow foundation.

This essay is followed by several well-compiled papers on various subjects; and the volume concludes with an important and interesting article, giving an account of the Structure and Functions of the Glands of the Intestinal Canal. Since the origin of the Broussaisian school, the intestinal glands have performed an important part in pathological history, and they are well known under the title of Brunner's and Peyer's glands. Attempts have been made from time to time,* but without attracting much attention, to enquire minutely into their intimate structure: the standard works of Andral, Louis, and Abercrombie are silent upon this point; and, although Billard has been more minute than any of these authors in his description of them, he has added but little to our knowledge. The subject, however, was some time since taken up by Dr. Boehm, of Berlin, whose excellent work on the subject† we analyzed at great length in our Second Number, and illustrated our analysis by copies of the original plates, (see Vol. I. p. 521.) As Dr. Albers coincides almost entirely with Dr. Boehm respecting the anatomy of these parts, we shall content ourselves with referring our readers to our previous article on the subject. Dr. Albers's essay adds little to what was already supplied by Boehm, but it is of importance as corroborative of his views.

Peyer's patches vary exceedingly both in number and degree of development; they are sometimes so indistinct as to be scarcely perceptible, and at other times they form prominent projecting surfaces. They are greatly under the influence of disease, not only in point of development, but also in point of number. Scoutteten‡ holds not only that their number is variable in different individuals, but that they may be increased or diminished by individual habit of body. Fohmann and Von Halen§ are of the same opinion, and maintain that various conditions of the body may produce a development of Peyer's patches, and cite pregnancy as a case in point. It is a generally received opinion that they are more frequent in youth than in old age, and the idea that their number and development are influenced by disease is by no means uncommon.

We are not prepared to say in how far the views of Boehm and Albers respecting the intestinal glands may be considered as correct. The subject is one, however, well deserving the attention of the physiologist and pathologist; and it appears to us a matter of surprise, considering the great importance which has for some years been attached to diseases of the intestinal mucous surface, that such an investigation had not been sooner entered upon; and, we feel assured that a work either in confirmation or in refutation of the views of the authors above mentioned would render an essential service to the science of medicine.

* *Anatomisch-physiologische Abhandlungen*, von Karl A. Rudolphi. Berlin, 1802.
—Lieberkühn, *Dissertationes Anatomicæ*, curâ J. Sheldon. London, 1782.

† *De Glandularum Intestinalium structura penitiori commentatio anatomica*. Dissert. Inaug. Anatom. Auctore Lud. Boehm.—Berolini, 1835.

‡ *Journ. Complement. du Diction. des Sciences Med.*, 1829.

§ *De Glandulis Conglomeratis Specimen Inaugurale*. Leod. 1830.

ART. XV.

An Essay on Pyrexia, or Symptomatic Fever, as illustrative of the Nature of Fever in general. By HENRY CLUTTERBUCK, M.D.—London, 1837. Pp. 136.

THIS essay, the production of one of the most eminent of the physicians of London, has disappointed us. The vagueness of the title-page pervades most of the subsequent pages, and we close the book with a very indistinct idea of the author's precise intention in publishing it.

Several years ago, (about thirty,) and before the birth of the theory of Broussais, according to which every fever is symptomatic, and symptomatic of inflammation of the mucous membrane of the stomach and intestines, Dr. Clutterbuck published his views on the same subject; first in an inaugural thesis at Glasgow, and subsequently in an enlarged form, in an "Inquiry," which appeared three years afterward. According to these views, which are also maintained in the present essay, every fever is really symptomatic, although, with the same peculiar employment of words which leads to the use of pyrexia as the synonym of symptomatic fever, Dr. Clutterbuck calls such fevers as are, according to his own theory, symptomatic of cerebral inflammation, *strictly idiopathic*. The terms, thus, as we conceive, misapplied, are employed in almost every page. Separating the fevers symptomatic of all other inflammations from what is called, and what the author calls, idiopathic fever, Dr. Clutterbuck ascribes the latter, not to inflammation of the gastro-intestinal mucous membrane, but to inflammation of the brain. According to both theories, there is no such thing as a fever to which the term idiopathic can be properly applied, that term being significative of a primary general disorder of the system; of which they do not admit the possibility. Dr. Clutterbuck retains the term, we do not know why: M. Broussais and his followers reject it with ridicule and scorn; looking upon the idea of an essential fever as an abstraction, a mere imagination, a figment of the mind.

The authority of such names as those of Broussais and Clutterbuck might well discourage opposition, if the great theorists were not so much opposed to one another. Their theory would be unassailable, if they were equally fortified by union in all the points of it. To them we have to ascribe the more willing admission made by modern practitioners of the existence of local inflammations in fevers, and the importance of attending to them. But this did not suffice them. Each, much impressed with the frequency as well as importance of these complications of fever, raised unto himself an inflammatory doctrine of fever, to the discountenance of the essentiality of fever itself. Both practical men, both good observers, the theories they built up are such, and rest on such proofs, that the established truth of either would inevitably destroy the other. This is a curious illustration of the difficulty of observation, and the fallacy of experience. That both the theorists are entirely wrong, and that each has through life been in full pursuit of one idea, and the idea of each wholly incorrect, is inconceivable. There must be some points on which each is right,—some neutral ground on which each is safe,—although each may deny the stability of that very territory. We sus-

pect, also, that there is some common ground in which they each and equally wander into error. Where they differ, each is to a certain degree right; where they agree, we apprehend that both are wrong. Such a spectacle must be infinitely diverting to those unskilled in professional niceties; although the high character of the combatants, as well as the seriousness of the question, involved in such disputes, has caused the medical profession to look on the contest with interest and respect.

Dr. Clutterbuck says *his* doctrine has been adopted "by not a few," with little reservation, and he claims for it the high merit of having materially contributed to improve the practice in fever, by dispelling the idea of debility being an essential part of the disease, and teaching that the debility arises from a previous state of vascular excitement "in the brain more especially." This is, indeed, no small merit, and we believe Dr. Clutterbuck to be justly entitled to it. We learn from the publications of the time, that, about the period when his *Inquiry* was published, wine and bark were indiscriminately given by many practitioners in every case of fever. The first part of the *Inquiry*, the work in which Dr. Clutterbuck's views were published, contained the general doctrine; the second part, which is to shew the application of the doctrine to the *varieties* of the disease, has not yet appeared. In the mean time we are not left to suppose that Dr. Clutterbuck has modified his original views, the truth of which, he says, has only become the more firmly fixed in his mind by subsequent reflection. The object of the essay now before us is "to endeavour to shew that a febrile state of the system, or what is technically denominated *symptomatic fever*, is always a *secondary* affection, the result of inflammation, and of no other cause." (*Introd.* p. vi.) We need scarcely observe, that any fever, which is properly a symptomatic fever, must be secondary; but the question is, whether or not the primary accident is always inflammation. For the opinion that it is always inflammation, Dr. Clutterbuck claims importance in relation to diagnosis; as it must lead the practitioner carefully to seek in every case for an inflammatory cause. This, it is evident, may be advantageous or not, as the truth of the theory is established or doubtful. A prudent practitioner will be careful not to overlook any existing inflammation; but the practitioner who always asserts that there *must* be inflammation going on somewhere, will scarcely, unless always right in his opinion, be so prudent in his measures as the welfare of the patient requires; although Dr. Clutterbuck does not forget to mention that the pyrexia, which he considers secondary, may materially affect the character of the inflammation, which, according to him, is the primary disease; and although he admits that the pyrexia is *sometimes* the more immediate cause of the fatal termination, and that, even in brain-fever, a secondary inflammation of some other organ may supervene, and constitute the essential part of the disease.

As regards the subject of Dr. Clutterbuck's essay, the *symptomatic* fevers; the very name implies that they are of *secondary* origin. They must be symptomatic of something, or not symptomatic at all. But the main assertion involved is, that the febrile state, whenever secondary, is symptomatic of inflammation alone; and that, when no local inflammation can be anywhere detected, as in fevers in which the sensorium is

more or less affected, there is always inflammation present, and the brain is the organ inflamed.

It is impossible not to feel a conviction in the outset, that Dr. Clutterbuck's opinions, as well as those of M. Broussais, who would say just as much of the gastro-intestinal mucous membrane, are opposed by undeniable experience of cases in which neither the symptoms, from the beginning to the decline of the malady, nor the treatment found successful, indicate the presence of a state that can, with any propriety, be termed inflammatory. But no one can suppose such cases to have escaped Dr. Clutterbuck's attention; and this it is which gives a peculiar interest to the enquiry. Restricting the term symptomatic fever to fevers arising from all other inflammations, he considers idiopathic fever (which term he rather inconsistently retains, and often employs,) to be a local disease,—an inflammation of the brain; and he admits no other kinds of fever, excepting intermittent fever, which he looks upon as a distinct and specific disease; thus avoiding, as it appears to us, the inconvenient illustration which it affords, together with fever from the absorption of morbid poisons, of fever without local inflammation of any kind for its cause. Dr. Clutterbuck considers the prostration of strength, the "*viribus præsertim artuum imminutis*" of Cullen's definition, as not being a part of the pyrexia, or symptomatic fever, but only of the *proper* or *idiopathic* fever; which terms he so often introduces in this manner as to leave us in doubt whether or not he restricts them to inflammation of the brain. He gives a minute description of what he calls pyrexia, or symptomatic fever, without, however, making it very clear what he is really describing. After more than one perusal of the first section of the work, it leaves no distinct impression on the mind. We do not see, in the descriptions it professes to give, pictures of disease, but rather ingenious views, shaded by a one-sided theory. Nor can we wonder at Dr. Clutterbuck's ready imagination of the existence of inflammation of the brain in every case of fever not arising from some obvious inflammation existing elsewhere, when we find him disposed to consider the febrile condition induced by the use of mercury as so induced by a state bordering on inflammation of the internal surfaces of the blood-vessels; and even to look on pregnancy and menstruation, because often attended by febrile symptoms, as conditions having at least a great resemblance to inflammation. All our previous notions of pathology are confounded rather than corrected by the assertion, that "menstruation, in fact, in many females, is a state nothing short of actual uterine inflammation, and calls for an appropriate treatment." In like manner, Dr. Clutterbuck ascribes the febrile condition following a debauch in eating and drinking, the next day's sorrow of the debauchee, to inflammation of the mucous membrane of the œsophagus and stomach. In some of the previous pages, he points out, as diagnostic of what he calls "pyrexia, or symptomatic fever," heat of skin, frequency of pulse, and a furred tongue; and insists on idiopathic fever, or what is in his opinion always a brain-fever, being distinguished by the addition, or at least the presence, of "*pain in the head*, disordered sleep, prostration of muscular strength, and other disturbance in the state of the sensorial functions." The victim of hesternal conviviality suffers from all these; and we do not see why his case is not classed by Dr. Clutterbuck among brain-fevers.

The symptoms, which are decidedly febrile and symptomatic, are, however, considered by him as symptomatic of local inflammation; and he locates the inflammation, not in the brain, but in the œsophagus and stomach. We cannot imagine an opinion standing on a looser foundation. The force of imagination required to conceive the entity of fevers is mere feebleness compared with that which can be satisfied that there is any inflammation at all in such a case. There is a state of irritative fever; symptomatic doubtless, but not of inflammation.

Dr. Clutterbuck lays particular stress on the relief of symptomatic fever, as a means of acting upon the original inflammation, of which, according to him, fever is always symptomatic. He seems to ascribe tubercles in the lungs, in almost every instance, to an inflammatory origin; and he considers the relief of the hectic fever as the most important circumstance in the treatment of that malady. The following extract sufficiently illustrates the views he takes of this subject.

“A febrile state of body argues the existence of inflammation, equally in the weak as in the strong. The intrinsic nature of the disease is the same, and the treatment is governed by the same general principles. It is only necessary that the remedy should be properly adjusted to the existing circumstances; and, with this limitation, bloodletting will be found both as safe and as effectual in the one case as in the other.

“The case of hectic fever, as occurring in pulmonary consumption, serves to illustrate what has now been said. In the confirmed state of this disease, a cure is rarely to be expected from this or any other means. Yet much may be done to mitigate the violence of febrile action, (the hectic;) by doing which, much suffering is spared to the patient, and life often prolonged to a considerable extent. These important objects may be accomplished in many instances by bloodletting, repeated from time to time, but always in small quantities, as from three or four to five or six ounces at a time; the rule being, in such cases, so to draw blood as not sensibly to disturb the functions or feelings of the patient. When used with these precautions, and while a tolerable share of general health still remains, the good effects of small and repeated bleedings are often strikingly displayed, by their relieving all the most distressing feelings of the patient, inducing quiet sleep, and restoring appetite, if lost. And I may further add, that, if the disease admits of a cure, (as undoubtedly is sometimes, though rarely, the case,) it is most likely to be accomplished by such means; and this I have seen in different instances. I consider it, however, to be essential to this mode of treating pulmonary consumption, whether undertaken with a palliative or curative intention, that the patient should be allowed to take food, either animal or vegetable, as his habits and inclinations may lead him. And it is no small recommendation of this practice, that not only is appetite usually excited by such small bleedings, but animal food may then be taken without producing that feverish excitement which it is otherwise apt to do.” (P. 97.)

In inflammation of the mucous membranes, although Dr. Clutterbuck observes that they have a tendency to spontaneous subsidence, by an increased secretion of mucus, and although the disease may be prolonged if bloodletting is carried so far as to weaken the system considerably, he does not admit that there is anything so depressing in the nature of the inflammation as to preclude the use of that remedy altogether; but, on the contrary, when the symptoms are severe, considers bloodletting as useful, and even as necessary, in the treatment of catarrh, diarrhœa, and other inflammations of the mucous membrane, if recent, as in inflammation of any other structure.

At the outset of ordinary idiopathic fever, which we are to recollect is, according to Dr. Clutterbuck's views, brain-fever, or fever dependent

on inflammation of the brain, he deems the administration of emetics, several times repeated, at intervals of a few hours, highly useful, on the principle of counter-irritation. He questions the good effects of large doses of tartarised antimony; but places considerable reliance on digitalis, which, by its specific power of lessening the frequency of the pulse, and thereby diminishing animal heat, he thinks particularly calculated to subdue febrile action; particularly in the hectic fever of consumption, in which, he says, "it commonly diminishes, and sometimes subdues altogether, the febrile symptoms. Under its beneficial operation, the night-sweats disappear, the sleep becomes calm and refreshing, and the appetite is restored. The progress of the disease towards its usual fatal termination is often arrested for a time; and, more than this, a cure is sometimes thus effected, especially when the circumstances admit of its being used in combination with small and repeated *bleedings*, as already mentioned." (P. 105.)

Dr. Clutterbuck is of opinion that opium, in the generality of cases, rather aggravates than diminishes febrile action, whilst it also checks the secretions and masks the symptoms. Of nitre, the acetate of ammonia, and the citrate of potass, he speaks slightly; more so than, we think, these convenient and often-used remedies deserve. He lays just weight on the regulation of the food, drink, air, and exercise of the patient in all cases of symptomatic fever; observes that many chronic inflammations, when accompanied with a febrile state of body, will yield to persevering abstinence, where a more active treatment has been useless. Pulmonary consumption has, he thinks, given way, in many instances, to the continued use of a milk and vegetable diet; and he thinks he has seen cancer arrested in its course, or its symptoms much mitigated, by the adoption of Dr. Lambe's system of vegetable diet.

Dr. Clutterbuck devotes several pages to endeavouring to shew that the able authors of the *Elements of Medicine*, Drs. Bright and Addison, differ very little from him respecting the nature of the fevers usually called idiopathic. To us, those respectable authors appear to concede no more to Dr. Clutterbuck than the majority of British pathologists would cheerfully do. They would doubtless acknowledge, as we do, the respect due to any opinions entertained by a physician so experienced, and of so high a reputation as a judicious practitioner. They would probably abjure the notion of mere debility in the commencement of fever; and would search for and recognize the presence of local inflammation whenever it existed, and, in consequence of its presence, or without it, in consequence of the presence of great vascular excitement alone, would promptly bleed the patient. But it is equally evident that they think the inflammation, when present, a secondary phenomenon in the order of the occurrence, preceded by several febrile phenomena; and that they believe these phenomena, even when indicating disturbance of the cerebral functions, may be associated with a morbid condition of the brain which is yet not inflammation. It would seem little more than a question of terms, if the head-symptoms always kept pace with the increasing severity of the febrile symptoms; or if the increase so often observable after the fever has continued for ten days were not frequently found to keep pace with, perhaps to depend upon, the development of inflammation in some other organ, without sufficient evidence, or any evidence, of the brain itself

being also inflamed. That there are many cases of fever, also, which run through their whole course with sufficient evidence of a morbid condition of many functions, not only of the sensorium, but of other parts of the system, in which it is no more reasonable to assume the existence of inflammation of the brain than of other organs, the uninflamed state of which is more easily verified, is, we suppose, unquestionable; and that cases of fever may be fatal either without exhibiting any signs of inflammation on examination after death, or such marks of it as are inadequate to account for death taking place, equally so. Our own observation is entirely baffled, if there are not many cases of simple fever, properly so called, cases of general disturbance without a primary inflammatory cause, in the course of which, and in the space of several weeks, irritations of the intestines, of the brain, and of the lungs successively appear, without inflammation, and without the invariable appearance of cerebral irritation as the first link in the chain, however early it may appear. When inflammation is decidedly present in fever, too, the progress and character of the fever are not, we presume, always or often found to be those of a simple symptomatic fever, wherever the inflammation may be; but are apparently influenced by some unknown causes, productive, in different epidemics, of more or less depression of the vital powers, and corresponding inability to bear lowering measures. All these are points which have been ably demonstrated by many careful pathologists; and such as we should have expected to find weight with more exclusive theorists. Although we are inclined to believe that practical benefits have arisen from the views taken by Dr. Clutterbuck, and those entertained by M. Broussais, (we particularly allude to the greater attention drawn towards inflammations occurring in fever,) we should much question the general soundness and safety of any practical measures whatever, *generally* applied to fever, and without especial regard to many or all of the above circumstances. The adaptation of the remedial means to the different cases met with in practice, according to the preponderance of the local or general symptoms, is one of the most delicate points which the physician is called upon to determine; and doubtless he will be most likely to determine accurately whose theory does not indispose him to scrutinize the nice evidence of the combination before him.

During the thirty years which have intervened between the publication of Dr. Clutterbuck's Inquiry and the present Essay, ample evidence has been collected, in all countries, of the variable types of fevers, to which Sydenham, long before, directed especial attention. They were insisted upon, with much force, by Dr. Wilson Philip as soon as the enquiry appeared, and few practitioners are now wholly inobservant of them. We cannot suspect Dr. Clutterbuck of overlooking them; but we think that both his theory and the practice it inculcates are somewhat dangerously opposed to the caution required in the treatment of fevers. In private practice, the want of such caution passes almost unobserved; but in public institutions the error has been shown in its worst consequences again and again. If the older practitioners erred with wine and bark, the moderns have not always been free from error with the lancet and leeches; and in both cases the hurtful practice arose from erroneous theories, from conclusions which became false when they became general. It may amuse as well as instruct the inexperienced reader to be told that

Dr. Beddoes, who pointed out in strong terms the too great exclusiveness of Dr. Clutterbuck's theory, fell himself into the gastro-enteritic error, and with quite as little discrimination advocated depletory measures directed to the abdomen, particularly by leeches in "relays of dozens;" as it were foreshadowing the theory and the practice of the school of Broussais. Thus do medical reasoners for ever seem to be following one another in circles, intersected by other circles, tangent in error or in truth; like unto blindfolded men, propelling a load of theory, with strenuous efforts to preserve a straight line, but deviating to the right or left, or retrograde, as the inequality of the course determine.

ART. XVI.

1. *Die Pest des Orients; wie sie entsteht und verhütet wird, drei Bücher.* Von Dr. C. J. LORINSER. Berlin, 1837. 8vo. pp. 461.

The Oriental Plague; its Cause, and the Means of preventing it, in three Books. By Dr. C. J. LORINSER.

2. *De Peste Antoniniana Commentatio.* Scripsit JUST. FRID. CAR. HECKER, M.D.—Berolini, 1835.

An Essay on the Plague which prevailed in the Reign of Antoninus. By Dr. J. F. C. HECKER, Professor of the History of Medicine in the University of Berlin, &c.—Berlin, 1835. 8vo. pp. 29.

ALTHOUGH works of the kind now before us afford no results of immediate practical application, they must be regarded as highly important in relation to our general knowledge of epidemic diseases. The investigation of the history of former or distant plagues may throw light upon those with which we are ourselves visited or threatened. The contemplation of such visitations on the great scale, and when the mind of the enquirer is freed from the many causes which disturb the judgment when he himself is a witness of the scene, is likely to lead to views and principles, both in regard to pathology and etiology, of a more enlarged and comprehensive character. Therefore it is that we are always gratified by the publication of treatises like the present, which, whatever their defects, bear the impress of honesty of purpose, industry, and learning. As the thick volume of Dr. Lorinser is valuable chiefly as a work of reference to persons engaged in similar enquiries, we shall merely indicate its general character and contents in this place. Of the matter contained in Dr. Hecker's classical little essay we shall give a brief outline and analysis, as it is at once interesting and in many respects novel, although conversant with times so remote.

I. It is with regret that Dr. Lorinser views the apathy with which the physicians of the present day regard the fearful scourge of the Plague, and it becomes one of his chief objects in the work before us to vindicate its claims to occupy a share of their attention commensurate with the deadliness of the malady. We do not perceive, however, that the author has enjoyed any peculiar advantages of studying this fatal disease; on the contrary, it appears that he has not had the opportunity of observing

it personally; but he calculates upon the attention of his readers, chiefly, from the great importance which ought to be attached to the consideration of all measures which tend to limit its ravages and prevent its irruption into Europe, and because of his having enjoyed the opportunity of inspecting some of the numerous quarantine establishments, which are scattered over the Turco-Austrian and Turco-Russian frontiers.

Dr. Lorinser is evidently a man of considerable knowledge, and well conversant with all the standard works relating to the plague. His first book is devoted to a critical review of the preceding original writers on this disease, from Thucydides and the Greeks down to Dr. Patrick Russell. From the consideration of these works he seeks to demonstrate that Egypt has always been the birthplace of the plague, from which, as from a centre of infection, it has invaded the neighbouring countries.

In his second book he discusses the topographical character of Egypt, and takes occasion to express his conviction that the plague is at first an endemic disease, dependent upon the particular physical nature of the country, which acquires its contagious properties only at a later stage. This opinion accords with that of many of the French physicians who accompanied Napoleon's expedition, and likewise with that of the commission despatched to Egypt by the French government in 1828, for the express purpose of ascertaining its cause. It has been attempted by some to prove that the origin of the plague dates from the period at which the Egyptians ceased to embalm their dead, and that its cause is to be sought for in the putrid emanations arising from the bodies which have been disinterred by the waters of the Nile during the season of inundation. Dr. Lorinser is not satisfied as to this being an influential cause of the disease, but he believes that its periodic origin will be found to be owing to the unhealthy vapours which arise from the plains of the Delta after the return of the waters to their accustomed channel, whether they be contaminated by the corruption of animal or vegetable matter.

Two conditions are necessary for the propagation of the plague; the disease must in the first place be produced by the unhealthy emanations of the flooded plains, and secondly there must exist a peculiar state of the atmosphere favorable to its extension to countries too remote to suffer primarily from the poisonous vapours of the dank, reeking plains of Egypt. In the cities of Syria and Turkey it is not uncommon to observe the plague raging with violence for some weeks or months, and then gradually to die away, again to break forth with increased energy at some future period. But it does not follow that on each occasion a new focus of infection has been introduced from Egypt; it suffices that the seeds of infection have become latent, owing to the disappearance of that peculiar state of the atmosphere which is necessary to call into activity the principle of infection, and remained so till they are again vivified by some new atmospheric change.

In the third book we are brought to the consideration of those measures which have been put in practice by various European nations to limit the ravages of the disease. It contains a good account of the arrangements made by the consuls in the Levant to guard as much as possible against the danger of shipping infected goods, or of granting passports to suspected individuals; and also a long and very full description of the various quarantine establishments on the European shores of the Mediterranean,

with a detail of the procedure of disinfection. We have besides a full account of the sanatory cordon established along the Turkish frontiers of Russia and Austria, with some interesting details as to the practical efficacy of the system.

The great want in Dr. Lorinser's work is the absence of all original ideas. It forms a good compilation, in many respects excellent, and evinces much labour and patience, but would be decidedly improved by the curtailment of about two-thirds of its pages. Its contents, in so far as they relate to quarantine establishments, are calculated more for the middle of last century than for the present day.

II. The leading principle which Dr. Hecker has followed out, as well in the present essay as in his larger works on the Black Death, the Dancing Mania, and the Sweating Sickness (so admirably translated by Dr. Babington,) is, that an epidemic disease is the natural result of the compound influences exerted by the customs, diet, and general mode of living of the people whom it invades, and not a sudden inexplicable outburst: so that Horace's rule, *nemo repente fuit turpissimus*, applies to things as well as persons, and more years are required to arm an epidemic than to finish a scoundrel. Now, as every thing in this world is transitional, (the most opposite states being linked together by a thousand grades of approximation,) an unique condition of mankind has rarely, if ever, been witnessed; and hence diseases, whether epidemic or sporadic, which have been called unexampled, or extinct, are still to be detected, in a somewhat modified state, by the acute observer. Thus the dancing mania of the middle ages still survives in the Italian tarantula and even in St. Vitus's dance; sweating fevers are still seen in central Germany, particularly on the Maine, which have an evident resemblance to the English sweating sickness; and the *morbis cardiacus* of the ancients is nothing more than a scorbutic carditis, such as Dr. Crichton and Seidlitz have seen at Petersburg. (*See Brit. and For. Med. Rev.*, Vol. I. 259.)

Dr. Hecker is of opinion that the way was prepared for the pestilence which he describes, by extraordinary disturbances as well in the order of nature as in the frame of society. During the reign of Marcus Aurelius Antoninus and Lucius Verus, (the latter of whom reigned from A.D. 161 to 169, the former from 161 to 180,) tempests, earthquakes, inundations and scarcity of provisions combined in reducing whole nations to extreme misery. Vast clouds of locusts streaming from Asia, laid waste the fields of Europe: active warfare raged in the East, Illyria, Italy, and Gaul; and an increasing depravity of manners lent an additional shade to this gloomy picture.

Origin of the Pestilence. Preceded by these forerunners a plague broke out which must be numbered among the greatest in the annals of mankind, whether we consider its enormous mortality or the extent of the regions which it invaded. It is said to have first appeared at the city of Seleucia in Mesopotamia. The story runs that when Avidius Cassius had taken Seleucia, some of his soldiers rushed into the temple of Apollo Comeus, and searched a small aperture with the hope of finding something precious; on which the primitive infection burst forth from the secret hiding-places of the Chaldeans, and spread death and contagion from the

confines of Persia to the Rhine and Gaul. Dr. Hecker thinks that the soldiers were infected by means of clothes, which perhaps were placed in the temple for that very purpose. The city was taken by Avidius Cassius A.D. 165.

Progress of the Pestilence. The disease had soon spread over the immense space from the Tigris to the Alps, as it broke out at Rome immediately after the triumph of the Emperors; the date of this ceremony, indeed, is placed by Eusebius as late as A.D. 168, but our author thinks it more probable that it took place in A.D. 166. It is certain that the pestilence delayed the war against the Marcomanni for three years; and in the year 168 it raged most violently at Rome, (but whether for the second or the third time is uncertain,) as we learn from the life of Galen. He was scared away from Rome by the plague, and retired into Campania; but not thinking himself safe even there, he sailed from Brundisium to Pergamus. About the end of the same year, or the beginning of the following one, (169,) he was recalled to Rome by the Emperors, and then set out for Aquileia; the pestilence having probably now subsided, so that greater military expeditions could be equipped. The assembling so many troops seems to have caused a recrudescence of the epidemic, yet not such as to have put a stop to the campaign, or obstructed the ordinary course of affairs. It had not entirely ceased, however, when Galen wrote his fifth book *de medendi methodo*, as appears from his expression “*ὅν ἐτι ποτὶ παύσεσθαι*,”—would that at length it might cease! Now Galen wrote, as the learned agree, in his old age, and after A.D. 180. It was in this year that Marcus Aurelius was carried off, and, as the words used by Julius Capitolinus would lead one to suppose, by the pestilence; “on the seventh day he was much depressed, and would see only his son, whom he dismissed immediately, lest he should be infected; (septimo die gravatus est, et solum filium admisit, quem statim dimisit, ne in eum morbus transiret.)”

Nature of the Disease. The first symptom mentioned is a pustular eruption terminating in sores and crusts; sometimes, however, it was papular, and ended in desquamation; in the former case the cutis vera was affected, in the latter the epidermis alone. Galen calls it *ἐξανθήμα μέλανα*, a black eruption; the blackness showing that the disease was of a malignant and putrid type. The eruption was critical, those patients who had none, or only an imperfect form, being in the greatest danger. Our author declares this pestilent exanthema not to have been the small-pox, as that disease did not appear in Europe till the sixth century; a fact for the proofs of which he refers to an essay of his, entitled *die Pest im sechsten Jahrhundert* (the plague in the sixth century,) in the *Annalen der gesammten Heilkunde*, vol. x. p. 1.

Another symptom was a violent cough, with hoarseness, and fetid breath. One patient, a young man, with a bad cough and ulceration of the trachea, brought up a crust, i. e. a shred of lymph. Thucydides too, who described a pestilence of the same kind, mentions that the patients after sneezing were attacked with hoarseness and violent coughing. Galen's epidemic was likewise attended with a pestilential redness of the tongue, mouth, and fauces; and a fatal diarrhœa. On the whole, the disease seems to have possessed some features in common with both the

small-pox and malignant scarlatina as observed in recent times; as in these, the mucous membranes of the bronchi and bowels participated with the skin in the morbid affection. Dr. Hecker however considers it as a peculiar disease, a true specimen of the plague of the ancients, of which he promises a full history at a future time.

ART. XVII.

On a new Mode of Treatment employed in the Cure of various Forms of Ulcer and granulating Wounds. By F. C. SKEY, F.R.S., Assistant Surgeon to St. Bartholomew's Hospital.—London, 1837. 8vo. pp. 85.

As its title leads us to expect, this treatise is almost exclusively practical. Whatever temptation the subject of the pathology of ulceration may have offered to its author, he has almost entirely resisted; with the exception of some few remarks which connect themselves with the supposed *modus operandi* of the treatment recommended. The work is divided into four parts: the first, containing some general remarks on ulceration and ulcers; the second, on some of the medical properties of opium, the remedy proposed for them; the third, on the general treatment of ulcers and cicatrizing wounds; and the fourth, cases illustrative of the effects of the treatment proposed. We shall shortly analyze each part. Practical men (and the book is not intended for others,) know what is meant by an ulcer; so that we need not puzzle them with definitions. The classification adopted by Mr. Skey is that of Sir E. Home, and is a very useful one,—viz. ulcers characterized by excess, and those characterized by torpidity of action. The processes which constitute the healing of an ulcer,—exudation of lymph, its organization and cicatrization,—are dependent for their activity on the activity of the general circulation, supposing them to be uninfluenced by art; for, of course, local action may be excited by local means, without the participation of the general system. As a general rule, subject, however, to important exceptions, an ulcer may be regarded as a good criterion of the vital energies; these energies diminishing proportionally according to the distance from the trunk. Mr. Skey broaches the old question, why ulcers on the legs are proportionally so much more frequent than in other parts? and he agrees with no explanation of this fact which has hitherto been offered. His reasons, however, against the usual explanation of what are termed varicose ulcers are very unsatisfactory; because “ulcers frequently exist without varix, and varix without ulcer, and both these forms of disease occur more frequently alone than do the two conjointly,” is, supposing the statement to be a fact, no reason why varix may not be one of the frequent causes of ulcers. Might it not with as much justice be said, we have frequently extravasations of blood without blows, and blows without extravasations of blood, and both circumstances occur more frequently alone than do the two conjointly; therefore, blows cannot cause extravasations of blood. Both the blow and the varix (or the conditions essentially connected with the varix,) are mechanical causes, or are assumed to be such in the theory of their action; and, in one case, the extravasation, in the other the impeded circulation, is supposed to be thus mecha-

nically produced. But Mr. Skey cannot see how varicose veins can act as any impediment to the arterial capillaries which transmit the blood to them. "If," he says, "the size of the vena saphena were diminished instead of increased, I could understand the cause of obstruction in the small veins supplying it; and I should suppose, except for the contrary authority of Sir E. Home, that, the larger the trunk of a vein, the greater the facility afforded for the transmission of blood into it from the smaller branches; and that, instead of obstruction of the capillaries, there would be every encouragement afforded to the arterial blood to advance." Can Mr. Skey, when he considers the anatomical structure of varicose veins, and the laws which govern the circulation, as far as we are cognizant of them, still maintain that an enlarged vein, probably deprived of its valves, or possessing them in such a condition as renders them incapable of performing their functions, affords greater facility for the transmission of blood into it? As reasonably might he talk of the increased contractility of an over-distended bladder. The effect of pressure in mitigating the evil but adds evidence to the great probability of the theory usually applied to the explanation of varicose ulcers.

We should not have devoted so much space to the consideration of this subject, but, when well-received and apparently rational explanations of morbid phenomena are attacked, the reasons for so doing should be satisfactory, and, if possible, the substitute should be equally so. Mr. Skey refers the cause farther back in the circulation,—i. e. "to the capillary system, *which wants power for transmission*;" and he goes on to say, that "it appears to me that nearly all the phenomena of disease in those parts of the body remote from the heart, denoting this languid action, may be referred to this system. The fault is not with the veins, but with the arteries, and this transmission is by them effected so languidly as in a great degree to deprive the smaller veins of the influence of their contractility. Thus, the blood accumulates in the larger venous trunks, which become knotted and convoluted to a degree quite surprising." We give Mr. Skey's hypothesis in his own words. No very potent argument is adduced for its truth; but, as we are not the advocates of any one exclusive mode by which varix may be produced, or on which the various processes of ulcer depend, we can imagine that, in certain cases, the fault may originate with the arterial capillaries, whilst in others we can conceive a similar state of these vessels as the result of some mechanical obstacle, such as a varicose vein, &c.

To this condition of the arterial capillaries, the author, and with plausibility, ascribes the weak granulations of distant parts of the body. Inflammation, which is essential to ulceration, is not essential to constitute an ulcer; so that depletion, which may be of service in the former case, is only so in the latter when, from causes foreign to the disease itself, some degree of inflammation has been superadded. The reproductive power of ulcer is quickly exhausted, if allowed to fall below the standard of health. The form presented by the ulcers "too weak to carry on the actions necessary for their recovery," is that of large, whitish, bloodless, insensible granulations; the smaller the granulations, the greater is their manifestation of health. In the former, there is need of artificial excitement. In one form of irritable ulcer, there is a deficiency of secretion of the solid material of reparation, and a tendency to excess

of organization. The granulations are small and florid, bleed readily, and are very sensitive to the touch; in the other form, the ulceration partakes of the phagedænic character. Mr. Skey regards "every form of ulcer not caused by external injury, that is, interrupted in its healing by causes not external, as, in one sense, constitutional; that is to say, they take the type of the general health, and are remediable by internal therapeutical or dietetic agency." He does not include those forms of ulcer which may be justly termed *specific*.

In reference to the *treatment*, which forms the subject of the next chapter, Mr. Skey says, "although I conceive that it will be found applicable to every form of ulcer dependent upon weakness,—and in this catalogue I should include more than nine-tenths of those affecting the lower limbs, and a large proportion of ulcers situated on any other part of the body,—yet there is none in which it will be found so striking as in that most frequent form of the chronic or callous ulcer affecting the legs of old persons."

We cannot pass entirely without notice the remarks which, in this part of his essay, the author has made on Mr. Baynton's long-tried and very valuable treatment of certain forms of ulcers. He believes that Mr. Baynton had no very distinct notion of the principle of the application of his treatment; and says, that the moderate and uniform pressure recommended does not produce its good effect "*by supporting the granulations* by stimulating the functions of the skin," because healthy granulations require no support: however plausible the idea, there is nothing in the structure of a healthy granulation that demands the assistance of art, to the end of rendering it efficient for the purposes by which it is designed by nature. The effect of pressure applied to the surface of an ulcer is to excite to action, to rouse, to stimulate." The author appears to forget that the question is not, what healthy granulations demand, but what unhealthy granulations require. We are not compelled to believe that pressure is a stimulant, because unhealthy granulations assume an improved appearance under its use; but we know that the action of the heart, in propelling the blood from within outwards towards the skin, acts with a uniform pressure upon its interior surface. This pressure is, of course, resisted by the skin when entire, and the blood flows in its proper channels; but, when an aperture is made, there is no longer any resistance at that point, and consequently the blood flows out of it from all points of its circumference, because the internal pressure, being uniform, cannot act upon any particular point. Now, as in the case of an ulcer, the assistance afforded by the skin, which supports the vessels, is wanting, we can readily understand that the uniform and moderate support afforded by the adhesive plaster, as employed by Mr. Baynton, does actually in this respect simulate the function of the skin, and compensate for its absence as far as can be done by artificial means; and also that the plaster, in thus supporting the vessels, may have a distinct tendency to relieve inflammation by restoring the blood to its proper channels, and consequently to improve the appearance of granulations.

The second division of the essay is devoted to a consideration of some of the medical properties of Opium. Its acknowledged efficacy in senile gangrene, as first recommended by Pott, led Mr. Skey to reflect on its applicability to ulcers. In small doses, it promotes genial warmth, and,

by giving energy to the extreme arteries, maintains an equable balance throughout every part of the body. Its beneficial influence on the physical frame requires the previous existence of actual disease, the ravages of which demand assistance from the powers of renovation. But (and the author particularly and most judiciously insists on this point,) the effects above described are only obtained by the most moderate dose. In this dose, its powers as a stimulant are followed by no perceptible depression, either of the pulse or nervous system; and in such quantity only does the author recommend its use. The power possessed by it, when thus employed, he speaks of as one "rousing the dormant energies of local health through the means of the circulating system." To those who are disposed to contend that the influence of opium is more exercised upon certain organs, Mr. Skey replies,

"I deny the influence of opium upon either the brain, genital organs, or skin, when administered in doses of half a grain, night and morning, to adult subjects, with open and discharging wounds. These patients are ordinarily most tolerant of its influence, and, in the doses in which I recommend its use, are insusceptible of such eccentric action. I have been convinced of the truth of this assertion by repeated failure in the treatment by opium of cases of irritable and not ulcered legs, with dryness and desquamation of the cuticle. In these cases, I have frequently known the malady aggravated rather than relieved by opium. The dose should hold a relation to the state of the wound and of the constitution, and should always be an absolutely small one, although it may be comparatively large."

The former part of the above extract, which contains but the statement of what has fallen under the author's own observation, is well worthy of attention, and probably may be of very extensive application; but the latter, in which he justifies his conviction of its truth, is an instance—unfortunately too frequent in medical writings,—of the illogical inferences which may be derived from careless analogies. Opium, says Mr. Skey, may be continued with advantage, whilst the constitution requires artificial power. So long as there exists a drain on the circulation, the influence of opium on the general health will be found most beneficial. But he would wish that the conditions should be well defined in which its beneficial effects are or are not to be expected. "It is less efficient in florid or sanguineous temperaments; not that its influence is less marked, but because such habits are usually deranged by all narcotics. It is less efficient in youthful age or childhood, and it is objectionable in cases in which malignant disease is present in the system. Excluding these exceptions, all of which are subject to other exceptions, the wards of the metropolitan hospitals teem with cases of ulceration amenable to the treatment by opium, in which, perhaps, the disease is situated in the lower extremities." Of these, such a case as the following is described: that of a man beyond the middle of life, his nervous system exhausted, his leg or legs exhibiting, to any extent between the knee and the ankle, a dark red-brown discoloration of the skin, surrounding a deep excavated ulcer, immediately encircled by whitish elevated integument; the base of the ulcer being flattened or ragged, covered by a layer of watery lymph, and secreting a various quantity of fluid. There are no granulations, but an unhealthy and unorganized flake instead of them. Instead of Mr. Baynton's treatment, Mr. Skey recommends the internal, constitutional use of opium. He goes on to say that, "in many cases, a very

palpable effect is produced by eight drops of the tincture twice a day; but I rarely commence with a less dose than half a grain of the extract, night and morning. Age, sex, constitution, and idiosyncrasy, of course, require attention. But in every case, however favorable, I commence with this dose. On these conditions, it may be rapidly increased, if necessary, up to two grains night and morning." Opium thus given does not constipate. As a general rule in trying the effect of this remedy, all local treatment has been suspended, excepting the application of poultices or of some unstimulating ointment. It will be found most effective in cases of chronic ulcers, in old persons inclined by habit to spirituous drinks, and the leuco-phlegmatic who have not been similarly addicted. As it regards time, its effects have been least strikingly exhibited on the legs, and most so on the head and trunk.

Mr. Skey likewise informs us that he has extended the use of opium, in the form of the tincture, in doses of from ten to fifteen drops or more, with advantage in cases of common catarrh; and he also mentions a case of what he calls passive hyperemia of the nose, "which had existed for two years in a lady, aged fifty, of languid circulation and cold extremities, and of remarkably abstemious habits," which was successfully treated by opium. We are not able to speak, from our own experience, of the employment of opium in such cases, but we do not doubt its utility in many instances where the constitution is weak and the circulation languid. Its effect, our author tells us, is to arrest the troublesome defluxion from the nose which characterizes catarrh, and that "no reaction follows this treatment. The discharge does not recur with increased violence, and, if it return at all, is considerably lessened in quantity."—"The sensible effects of the medicine," he says, "are a general glow of warmth throughout the body, with a uniform degree of perspiration proportionate to the quantity taken. That the warmth of the skin is an indication of healthy action may be inferred from this fact, that it relieves itself, if in excess, by cutaneous transpiration."

Mr. Skey has communicated a few of the cases of local disease treated by the method above recommended. Of these, nine are cases of chronic ulceration of the legs, in three of which the veins were varicose; three of open bubo; two of phagedænic ulceration. In all these cases the sores assumed a healthy character immediately after commencing the use of opium, as already recommended. The two remaining cases are mentioned to shew the efficacy of the medicine in arresting pyalism.

As we ourselves have often derived advantage from the employment of opium in such cases, we are not disposed to examine very carefully what amount of benefit was derived, by constitutions exhausted by bodily fatigue and excess, in some of these cases, from rest, cleanliness, porter, and meat diet. Setting aside the effects of these remedies, there is evidence enough of the advantage obtained by the use of opium; and Mr. Skey deserves the thanks of the profession for having published his experience in its employment. The effects which all surgeons have frequently witnessed from the use of the drug in many forms of ulceration will dispose them to believe in its more extensive applicability, and to give it a fuller trial, according to the suggestions of Mr. Skey.

Having spoken favorably of this essay in a practical point of view, we can find nothing else to commend. What little pathology there is in it

is sadly unpathological; much of its reasoning is very illogical; and, generally speaking, its composition is so defective and so little indicative of a habit of clear and connected thought, that we have been obliged to read again and again in some instances, still remaining in doubt as to whether we have always correctly appreciated its author's meaning.

ART. XVIII.

First Principles of Medicine. By ARCHIBALD BILLING, M.D. A.M., Member of the Senate of the University of London, &c. &c. *Second Edition.*—London, 1837. 8vo. pp. 130.

AN idea, which must have been at some time or other entertained by every student accustomed to reflect on what he heard and saw, that, namely, of its being possible, out of the mass of ill-explained practical facts in medicine, to draw a clear continuous thread of medical principles, seems to have been the origin of this little work. The author did not allow his dissatisfaction with conventional explanations to exhale in general accusations of physic and physicians; but set to work to remove, if possible, the causes of what dissatisfied him.

"I visited," he says, "the different schools; and, though the students of each hinted, if they did not assert, that the other sects killed their patients, I found that, provided the physician of each was a man of talent and experience, the mortality was fairly balanced. I thought there must exist some true general principles by which the apparent inconsistencies of correct practice might be reconciled, and by which the contest between such systems as were essentially at variance might be decided. But, though there were to be found innumerable volumes of cases, and interminable heaps of insulated precedents, there was no treatise upon first principles. After twenty years of intense application to clinical practice, as a student, assistant, and professor, I ventured to publish what I had drawn up, condensed to 130 pages." (*Preface*, p. vi.)

We can imagine no more legitimate mode of producing a useful work than this; and Dr. Billing's opportunities of acquiring experience at the London Hospital, combined with the disposition manifested in these expressions, entitle his observations to great attention. Nor are we unmindful of the debt of gratitude owing to Dr. Billing for being one of the first, if not the very first, to practise *clinical* teaching, in the true sense of the word, in the London school, so early as 1822.

We meet in Dr. Billing, as in Dr. Latham, a witness of the negligence of the London students respecting the uses of the stethoscope; but those who were immovable by higher motives may, perhaps, be startled into studying it by Dr. Billing's admirably directed remark, that many affectionate parents "will soon begin the very simple process of applying their ear to the chest, and thus put the medical attendant to shame."

Every teacher of medicine, passing yearly in review the principal phenomena of disease, must find, after lecturing some years, that he becomes master, by the sole power of reflection, of some of the facts which during that time physiology and minute anatomy, advancing with slow but continual pace, have succeeded in developing or establishing. That Dr. Billing, therefore, an active as well as independent thinker, should find materials for half-a-dozen pages of prefatory reclamations, does in no

degree so much surprise us, as the good humour, little characteristic of reclamators, with which they are penned. Yet they comprehend important matters: the functions of nerves, the reflex principle, the theory of inflammation, the sounds of the heart, and the cholera; concerning all of which matters there has been much ink-shed. Thus, mortal men, using the intellect which God has given to them, make ever slow advances along the boundaries of knowledge, sometimes in column, and sometimes in equal line of march; and happy is it when, on lighting on some undiscovered truth, they do not turn round or aside, and contend with their followers or fellows, as is the wont of carnivorous creatures over their precarious food, disgracing the understanding and mocking the dignity of the human creature by the passions of the lower tribes of animals. It was a saying of Sir James Mackintosh, that, when asked in another world what he had been doing all his life-time in this, he should have no better answer to make than that "he had been talking." Of how many a physiologist the reply must be, "I have been fighting:" not from the calmness and silence of the study, with unwrinkled brow advancing, but with flushed cheek and soul-marks of intellectual blows.

Dr. Billing introduces the subject of the *First Principles of Medicine* by a brief survey of the leading facts in physiology. In speaking of the function of repair, he lays especial stress on inflammation, being the *notice* that repair is wanting, and also the process by which the spaces are prepared where the reparation (in a bone) is to take place, prepared by distention of the vessels, by sponginess induced in the part requiring repair; so that, if not stagnation, a sufficient retardation of the blood takes place "to allow of crystallization of bony matter." The use of the term inflammation will appear objectionable, perhaps, to many pathologists, who would admit that a peculiar condition of the part, accompanied by or passing on to inflammation, was the agent in giving the required notice. The state is probably a compound state, to be resolved into certain conditions of the nerves as well as of the blood-vessels.

Indeed, in other passages, Dr. Billing very distinctly explains that inflammation begins in the nerves, on which the capillaries depend for tone and energy. (P. 27.) In a subsequent page, (p. 35,) he seems to restrict this to *morbid* inflammation, in contradistinction to *healthy* inflammation. All this proves that our language is deficient in exact terms to express several morbid states of the blood-vessels, as well as of the nerves. It is not consonant with the impressions on the ordinary senses to admit, what Dr. Billing and others strenuously maintain, that in inflammation there is only diminished action of the arteries; that inflammation and congestion are but degrees of the same condition, &c. (P. 24.) The state of the arteries or of the capillaries, during inflammation, is not necessarily that of simple diminished action, *because* it is not a state of increased action. There is yet something undetected, and which determines the alterations of tissue that characterize inflammation.

Dr. B. maintains the elasticity of the arteries, and the non-vascularity of the circular fibres of the middle coat, which, allowing of separation laterally, and resisting dilatation, accommodate themselves to the elongation of the artery; in consequence of which the arteries are increased in capacity at each contraction of the heart, but by longitudinal stretching; whence arises the serpentine motion visible where the arteries

are at all loose. He does not admit the active power of dilatation which some physiologists ascribe to the heart: and he considers the right auricle to be filled by a constant pressure and an equable stream, not by suction, or any effect of vacuum and atmospheric pressure; observing, that there can be "no suction (no atmospheric pressure) during natural respiration, for the glottis is sufficient to admit a free current of air; it is only in croup or laboured respiration of any kind that there can be any effect of atmospheric pressure." (P. 12.) The capability of the heart to send blood all over the frame is, he thinks, intelligible by a reference to the hydrostatic principle of fluid in bent tubes finding its level.

Dr. Billing's theory of animal heat would seem to be identical with the celebrated one of Dr. Crawford. He expresses it somewhat oddly; and the paragraphs, it will be seen, are oddly printed. (We retain his types.)

"The ANIMAL HEAT has been accounted for in different ways by several ingenious physiologists; from the aggregate of their opinions and experiments I deduce, that *heat is extricated all over the frame, in the capillaries, by the action of the nerves, during the change of the blood, from scarlet arterial to purple venous; and also whilst it is changing in the lungs from purple to scarlet.*

"There is a perpetual deposition, by the capillary system, of *new matter* and decomposition of the *old* all over the frame, influenced by the nerves; in this decomposition there is a continual disengagement of carbon, which mixes with the blood returning to the heart, at the time it changes from scarlet to purple; this decomposition being effected by the *electric agency* of the nerves, produces *constant extrication of caloric*; again, in the lungs that *carbon is thrown off* and united with *oxygen*, during which *caloric* is again *set free*; so that we have in the LUNGS a CHARCOAL FIRE constantly burning, and in the OTHER PARTS A WOOD FIRE, the one producing *carbonic acid gas, the other carbon*; the food supplying through the circulation the *vegetable* (or what answers the same end, animal) *fuel*, from which the charcoal is prepared which is burned in the lungs.

"Thus the animal heat is kept up, the EVAPORATION of PERSPIRATION keeps the SURFACE COOL; but in inflammatory fevers where this is *deficient*, the body gets too *hot*; and in low fevers where the nervous influence is too low to keep up the full fire, the surface gets cooler than the natural standard." (P. 18.)

The phenomena of determination, blushing, &c. are explained on the principle, so strongly enforced by Dr. Parry, of diminished tonic. Diminished secretions, coexistent with increased quantity of blood in the secreting organ, are explained by the readier passage of the arterial blood through the enlarged capillary vessels to the veins, although with slower current, without obstruction, spasm, or error loci; a diminished supply being the consequence in the capillaries of which it is the office to secrete. This applies to that form of diminished secretion in which the heart retains its usual power; and the relaxation of the capillaries is ascribed to a want of nervous energy. Secretion may also be diminished from a failure in the injecting power.

In treating of the means of allaying inflammation and removing its consequences by means auxiliary to bleeding, Dr. Billing refers to the theory of capillary distension, with applications that may appear novel to some of his readers; as, for instance, when speaking of this use of mercury:

"I believe that mercury removes morbid growths by starving them, by contracting the capillaries, and not by increasing absorption, as is a commonly received opinion: it may be said that the swelling of the gums and fauces from mercury is a contradiction of their being starved; but, though swelled, they are worse nourished, are spongy,

weak, soft; as a limb may be swelled by dropsy, though actually emaciated as to its natural substance: another apparent objection to this idea of starvation is that mercury stops ulceration; but in many of our medical explanations we appear to 'blow hot and cold;' it stops ulceration, for the same reason in one case that it produces it in another; it contracts the capillaries; so that a healthy part is ulcerated by what contracts its nutrient capillaries *from* a natural state, an unhealthy ulcer is stopped by what contracts its relaxed capillaries *to* a natural state." (P. 39.)

In this quotation we have been obliged to take considerable liberties with the punctuation, which has evidently been quite overlooked in the correction of the press in Dr. Billing's book, and is so imperfect and capricious in every page as to confound and obscure the meaning, particularly to those to whom the subjects treated of are not familiar. This may seem a slight fault for a critic to notice, but it would be absolutely better to print a book without any stops at all than to point it with commas and semicolons thrown in at random, and constituting not stops, but hinderances. Such blemishes are always more to be regretted in proportion to the excellence of the publication, and are now so rare in works of respectability that their presence is seriously detrimental to a writer's reputation. We find no printer's name in any part of the book, and the author's time may be better engaged than in correcting proofs, but surely Mr. Highley should have some regard to sheets which issue from his shop, and some regard for those who publish with him.

An attempt is made to clear away the obscurity attached to the terms stimulant and sedative, as employed by many writers, and partly arising from the hypothetical notion that all sedatives became such in consequence of a primary stimulant operation.

"Stimulants produce stupor or confusion of ideas by overpowering the brain with arterial blood, from the *increased action* of the *heart* which they occasion; for, though arterial blood is the source from which the capillaries of the blood prepare or secrete the nervous influence, *over-injection* diminishes secretion (as in the kidneys, &c.); but mere increased action of the heart is not sufficient to produce the over-injection, the *stimulant* itself being circulated to the brain at the same time, *expends* by its action on it, the *nervous influence* which it meets with there; besides that, it interferes with the generation of more, so that the capillaries, from the exhaustion of nervous influence, become more *distensible*: the proximate cause, then, of *coma* from *stimulants* is *congestion*, or *plethora*.

"Sedatives, on the contrary, *diminish* the *injection* of the brain, at the same time *repressing* the *nervous influence*; so that the proximate cause of *coma* from *sedatives* is *inanition*.

"This will explain one cause of the confusion of terms: sedatives are sometimes miscalled stimulants when they relieve the coma of stimulants, or the drowsiness of fatigue, or other plethora, because this relief is called rousing or awakening, as by tobacco-snuff, digitalis, green tea. A stimulant, wine, given to a person fatigued produces sleep.

"By the term *sedative* is *not* to be understood a *putting to sleep*, but whatever has for a time the effect upon the nervous system as if it had been refreshed by sleep; for by checking the expenditure of nervous influence in the organs, there is more of it left at the disposal of the brain, but this of course has not the restorative effect of sleep (which does not diminish the supply of arterial blood to the brain by checking the action of the heart); on the contrary, though the ideas are rendered free at first, exhaustion will at length produce the coma of inanition." (P. 47.)

Dr. Billing considers the proximate cause of fever to be an "injured state of the brain and nerves," and to this injury he gives the name of inflammation. He acknowledges that we do not always see proofs of this

after death, but ascribes this circumstance to the nature of the nervous tissue. "This injury of the nerves (fever) may," he says, "be produced either by disease of some part in the body, as traced above, or by a poisonous matter introduced from without in the form of effluvia, the effect of which poison is to debilitate the nervous system, and consequently all other parts as depending upon it for energy." (P. 92.) Of this kind of theory and language we have already had to express our dissatisfaction. Dr. Billing's remarks on the treatment of fevers are, however, indicative of much experience and sound judgment; and his observations on the neuroses contain ingenious and interesting matter for reflection. But we must accuse him of being the worst *book-maker* that we ever met with. No table of contents, no division into chapters or sections, no title-pages, and no index, guide the reader through what thus appear desultory and unconnected pages. Still another edition is required, in which these things may be corrected, the printing attended to, and something more of form given to the whole work.

ART. XIX.

Elémens de Zoologie; ou Leçons sur l'Anatomie, la Physiologie, la Classification, et les Mœurs des Animaux. By M. H. MILNE-EDWARDS, D.M., &c.

Elements of Zoology; or Lectures on the Anatomy, Physiology, Classification, and Habits of Animals. By Dr. H. MILNE-EDWARDS.—Paris, 1834-7. 8vo. pp. 1066.

THE recent appearance of the last part of this valuable work leads us to take the opportunity of bringing it before our readers, and strongly urging it upon their attention. Its author must be known to all of them as a countryman of our own, and as one of the most distinguished naturalists of the present day. Though professedly an elementary work, it is easy to trace in it the pen of a master in the science; and we know of no book which we can with more confidence recommend to those of our younger friends who are desirous of acquiring such a knowledge of zoology as may aid them in more profound and scientific researches in Comparative Anatomy and Physiology. It is not the intention of the writer to detail the minutiae of genera and species, but rather to point out the peculiarities of structure which characterize each principal subdivision of the animal kingdom; and this is done in a manner at once clear, concise, and attractive. The anatomical and zoological descriptions are illustrated by more than five hundred woodcuts, many of which will bear a comparison with the best that have been recently produced in this country; and the cheapness of the volume is by no means its least recommendation. We shall be very glad to see the general diffusion of works like this lead to an increased attention to natural history among the members of our profession in this country; and we are persuaded that the student who devotes to such rational employment a portion of that time which is generally given to idle and frivolous amusement, will find no reason to regret his choice in after life, nor will he usually experience any difficulty in combining a moderate share of such pursuits with the faithful and zealous discharge of his more strictly professional duties.

ART. XX.

The Teeth a Test of Age, considered with Reference to the Factory Children. Addressed to the Members of both Houses of Parliament.
By EDWIN SAUNDERS.—London, 1837. 8vo. pp. 76.

THIS is a clever little pamphlet upon a very important subject. Of all the means hitherto proposed for ascertaining the age of children, there is not one, taken by itself, but in a great proportion of cases would be useless; and accordingly we find that deception has been practised to a great extent, in the manufacturing districts, both by the employers and the employed, and the recent benevolent parliamentary enactments thereby evaded. The interval allowed by these enactments, between the age of helpless infancy and that when hard and continuous labour is to commence, is short enough; yet, even this small period, which is all that can be looked to for training the children of the manufacturing districts to such habits, tastes, and feelings as may render them honest, industrious, intelligent, and happy, is neglected and encroached upon. We know from eye-witnesses that children are even now, after all that has been said and written on the subject, doomed to work at an age when their physical development proves them to be incapable; and the premature deaths of thousands of these unhappy beings demonstrate what must of necessity be the consequence. There is but one remedy which would strike at the root of the evil at once, and that is a system of national education, similar in some respects to that which is now in operation in some parts of the continent. This, with the present mode of registration, would not only ensure a sufficient time for the development of the body, but would convert what is now a mere automaton into a thinking, reasonable being. In the mean time, it is important to examine every hint which is thrown out towards rendering the end in view more certain; and we think the observations of Mr. Saunders, as he himself says, "cannot be unworthy the attention of the legislature, in its humane endeavours to succour and protect 35,000 little sufferers from injustice and oppression."

The question to be decided is, whether the Teeth obey certain fixed laws in their progressive eruption, so as to render the age of an individual, up to a certain period, ascertainable from an inspection of them?

"An acquaintance with the manner of their production, their structure, and the mode and order of their eruption, would certainly not discourage the suggestion: on the contrary, their great density of structure, unique and nearly independent mode of formation, together with their almost total want of sympathy with those constitutional changes and variations in the health in which the more highly organized structures partake, would induce the expectation of a greater uniformity of development of the teeth than of any other parts of the frame. And the idea would receive additional confirmation from the well-known fact that, amongst the lower animals, by common consent, it is regarded as the most certain and constant of all proofs on the subject that can be obtained: it is, therefore, not unreasonable to suppose that, making adequate deductions on account of the interruptions to normality of development, which a more artificial mode of existence opposes, the same regularity would obtain in the human species." (P. 27.)

It would be natural to suppose, *a priori*, that diseases affecting the development of the body would also, in some degree, affect that of the

teeth; but the author assures us this is not the case, or at least not to such an extent as materially to affect the correctness of the results. He contends that the formative process is never entirely stopped by causes acting on other portions of the animal economy; that disease may exist in one part, retard the full development of the physical powers, but that still the teeth, as if belonging to another and separate system, are equally well developed as in cases where all the functions are normal; that, in fact, they are developed according to a law of their own. It will be seen from this that the test cannot supersede the other means used for the same purpose,—viz. the ascertaining the state of health, constitution, temperament, &c. These alone can give any indication of physical strength. The only point gained is the knowledge of certain signs connected with dentition, which afford a criterion of age.

It is only necessary, according to the present Factories' Bill, to ascertain two epochs,—viz. nine and thirteen years of age; and accordingly the author has confined himself, in his investigations, to these periods, although tables could be formed equally applicable to the other periods.

The following is an instance showing the superiority of this test.

	HEIGHT.		AGE.		INCISOR TEETH.		MOLAR TEETH.
	Feet.	Inches.	Years.	Months.	Central.	Lateral.	Anterior.
James Jacques.....	4	0 $\frac{3}{4}$	8	4	3	0	4
John Sims	3	7 $\frac{1}{4}$	8	7	3	2	4

"Here, then, was a difference of *five inches and a half* in height in favour of the youngest boy, who was by *three months* the junior of the other. It need scarcely be observed, that the elder and shorter boy was by far the strongest and most capable of exertion: his pulse was strong and regular, while that of the latter child was small and frequent, indicating a low degree of animal power. Judging, then, from the height in this case, the elder boy, who was active and shrewd, and displayed great physical and mental energy, would have been pronounced too young for labour; while the other, *who was junior by three months*, but who exceeded him in stature by *five inches and a half*, would have been subjected to a distressing and injurious amount of exertion. It will be seen, however, that the appearances of the mouth indicated the true state of the case; so that, relying upon these, the shorter child must have been the elder." (P. 54.)

The author thinks he is justified in laying it down as a law, that both teeth are to be considered as emerged from the gums, wherever the tooth on one side is freely developed. It is necessary to direct attention to this, since it will make a great difference in the results, if unattended to.

Upwards of 1000 children have been examined by Mr. Saunders in the various large schools in London, and elaborate tables show the dental development at the two epochs; the results are summed up in the following manner:

"Thus, then, it appears that, of 708 children of nine years of age, 389 would have been pronounced, on an application of this test, to be near the completion of their ninth year; that is, they presented the full developments of that age. But, on the principle already stated, that of reckoning the fourth teeth as present where the three are fully developed, a still larger majority will be obtained, and, instead of 389, the proportion will be as follows:—Of 708 children, no less a number than 530 will be fully nine years of age. What, then, are the deviations exhibited by the remaining 178? They are the following:—126 would be pronounced eight

years and six months, and the remaining 52 eight years of age; so that the extreme deviations are only twelve months, and these only in the inconsiderable proportion (when compared with the results obtained by other criteria,) of 52 in 708.

"Again, of 338 children of thirteen years of age, no less than 294 might have been pronounced, with confidence, to be of that age. The remaining 44 would have been considered as follows:—Thirty-six in their thirteenth, and eight near the completion of their twelfth year." (P. 76.)

Such is an outline of the evidence adduced by Mr. Saunders; and it is, in our opinion, quite sufficient to establish the superiority of the dental test over every other yet proposed. The subject is well worthy the consideration, not only of the government, as connected with the Factory Bill, but also of the medical jurist, to whom tables such as those drawn up by the author would, in many cases, prove of essential service.

ART. XXI.

Bidrag til Bornekoppernes og Vaccinationens Historie i Danmark, og om de sidste herskende Koppe-Epidemier. Af JOH. CHR. W. WENDT, M.D. —Kiøbenhavn, 1836.

Contributions to the History of the Small-pox and of Vaccination in Denmark; also Observations on the last prevailing Epidemics of Small-pox. By J. C. WENDT, M.D.—Copenhagen, 1836. 8vo. pp. vi. 72.

THE author of the above treatise had previously published an account of the small-pox and of the progress of vaccination in the Danish States. In the interval of the two publications, the author has acquired more experience,—has learned to estimate very highly the importance of vaccination,—and has acquired the conviction that the influence of time and of other circumstances is such as to diminish the protecting power of vaccination, so that it may require to be repeated more than once. Epidemics of small-pox have, during this interval, been prevalent; a system of quarantine, and a mode of treatment corresponding to it, have been adopted, and again given up. As the author (between May 15, 1835, and March 31, 1836,) in his capacity of superintendent physician to the hospitals of Copenhagen, was engaged in the treatment of 1389 cases of variola, his opportunities for collecting materials worthy of communication on the subject of this treatise must have been considerable. In the epidemic of variola which commenced in March, 1828, and which prevailed, with a short interval, until July, 1830, 562 patients were received and treated in the hospital; of whom, 111 had the true variola. Of these there died 29,—in the proportion therefore of 1 in 4: 84 had not been vaccinated, and of these 24 died,—in the proportion of 1 in $3\frac{1}{2}$. One of them did not know if he had been vaccinated; and 29 were specified as vaccinated, of whom only 4 died,—i. e. 1 in 7. This confirms the fact that the previous vaccination, even if it had not produced any modification in the outward form of the disease, had materially influenced its mortality.

Among the patients who had been vaccinated, there was only one of four and a half years of age: all the others were older, and most of them had been vaccinated in the early period, when vaccination was extensively employed. The younger the patients were, so much the more mild

was the variola. None of the vaccinated children died, or had true small-pox.

In August, 1832, commenced the most violent epidemic of small-pox which has prevailed in Copenhagen since the introduction of vaccination. Until the end of the year 1834, as many as 1045 patients were received into the small-pox hospital, of whom 45 died; among them was a female, who died nineteen days after the eruption of the pock, and sixteen days subsequent to her delivery. The proportion of deaths was 1 in 24. The number of individuals not vaccinated, and of those who were vaccinated eight days previous to the breaking out of the small-pox was 147; of whom 34 died,—i. e. 1 in 4. Eight hundred and ninety-eight had been vaccinated, and of these 10 only died,—i. e. 1 in 90. Of 179 patients who had the true disease, 119 had not been vaccinated; of these there died 34, or 1 in $3\frac{1}{2}$; 60 had been vaccinated, and 10 of them died, or 1 in 6; which confirms the protecting power of vaccination. In both epidemics, the efficacy of vaccination was seen; but experience proved that in many cases the protecting power was weakened after many years; and this was shown by the modification of the pock in infants and adults; for the disease was only seen in its true form in such as had been vaccinated and had completed their fourteenth year, and of those who had been vaccinated none died of small-pox before they were twenty-three years of age. Again, the pock was often so far modified that it never burst; and, of those who were revaccinated, not one had the disease.

On the 1st of May, 1835, the quarantine against the disease was terminated, although the disease itself continued to advance. In one hospital, between the 15th May and the end of the year 1835, the author treated—vaccinated patients, 1043; non-vaccinated, 123. In addition to these were 31, in whom no traces of vaccination or of true pock could be discovered, making together a total of 1197. Although the pustules of those patients who had been vaccinated were generally of the milder kind (varioid), still there were occasionally cases which almost completely exhibited the true matured pustule, and even confluent in a high degree. Of the whole number of the 1043 vaccinated, there died 47; three of whom were in their eighteenth and nineteenth years: none were younger than this. Of the 123 who were not vaccinated, partly because they had had the small-pox, 51 died; i. e. five children beneath a year old; two within ten years of age; the other 44 between their twenty-fifth and fifty-second years. Of the 31 cases of small-pox in which previous vaccination or small-pox was quite uncertain, eight died; none being less than ten years of age, nor beyond forty-three, excepting one individual, aged fifty-three. Of the vaccinated there died about one in 22; whilst, of those who had not been vaccinated, about one-third fell a sacrifice to the disease. It must also be remarked, that the small-pox patients received into the hospital of which we have just spoken, had usually, at the period of reception, reached an advanced stage of the disease; some of them having lain days, or even weeks, in their miserable abodes, and others being even at the point of death. If those who were dying when brought to the hospital, together with those who lost their lives by morbid complications of the small-pox, are subtracted, it would leave the proportion of 58 deaths in 1149 patients who were treated, which must be regarded as a comparatively small mortality.

Revaccination is introduced into the Danish army, as an effectual means of diminishing the contagion of variola, and of securing against the fatal consequences of the disease. The result in the year 1835 was as follows:

Age.	Revaccinated with effect.	Revaccinated without effect.
1—10 years.	33 individuals.	1 individual.
10—20	216	82
20—25	2175	998
25—30	191	76
30—40	123	43
40—50	18	8

It is very important, both in vaccination and revaccination, that the operation should be performed with great exactness, and with lymph transmitted through healthy subjects. Winslow received the first lymph from London, on July 6, 1801; and since that time it has been transplanted from individual to individual, and, as may be learned from the experiments and their results during the long epidemics of small-pox, it has continued unweakened in its protecting power. In respect to that question, so often asked, Whether it is fitting to take the matter originally from the cow? the opinion of the author is, that (since the erroneous notion that vaccination protected during the whole life has been given up,) the circumstances have in so far become changed, that, from the acknowledged advantages derived from vaccination, there is so great a demand for the matter, that it must be necessary in some instances to obtain it from the cow itself. He reports that Dr. Ritter, the superintendent of the institution for vaccination in Kiel, in consequence of a royal command, had undertaken several journeys to those places in Holstein where the cows had the pock; and that he had sent to the Sanatory College in Copenhagen some of the vaccine matter which he had taken from the cow, which succeeded well. Dr. Ritter has remarked that the vaccinations which he performed with the matter of the cow-pock, in the years 1824, 1826, 1829, 1830, and 1832, upon children, produced larger pocks than those produced in the ordinary manner; and that they were surrounded by a strongly inflamed areola. The cicatrices, also, which remained after these, were more marked and deeper than in the common form. Herhold says, in a letter of the 6th of December, 1834, addressed to the author, "that he entertains no doubt that vaccination from young cows might be the most appropriate method, both to renew the vaccine matter and to provide the requisite quantity of it for revaccination."

From his collected experience the author draws the following conclusions: That the true or inoculated small-pox, which has run through its course, does not certainly protect; that the same individual (though such a case is rare,) may be a second time affected with the disease; that such is more frequently the case after vaccination, although the disease is then usually the varioloid; that the protecting power of a good vaccination generally secures for a certain time, perhaps for six years, and that a person who has been properly vaccinated always has a milder form of the disease; that, on those accounts, revaccination should be performed. The time at which the protecting power of the vaccine terminates cannot be correctly estimated: it depends on the susceptibility of

the system of each individual for the small-pox. The revaccination must therefore be undertaken when an epidemic of small-pox prevails; and, should not the vaccination answer at this time, it must be repeated at a longer or shorter interval. It is also certain that a fatal termination of the disease is rare when the vaccination and revaccination have been perfectly performed and no causes tend to prevent its favorable operation; and, consequently, that the discovery of Jenner is of great advantage to mankind.

The very general prevalence of epidemic small-pox throughout England of late years, and the now generally acknowledged inefficacy of vaccination to guard against it in a large proportion of cases, renders every contribution to our knowledge, on this subject, of importance in the present state of the enquiry. We have on this account deviated, on the present occasion, from our invariable custom of writing our notices of books from the original. Dr. Wendt's book not having reached us, we have translated the above brief analysis of it, by Dr. VON SCHÖNBERG, of Copenhagen, from *Smidt's Jahrbucher*, which we give without comment. On the same grounds of utility, we shall give here, rather than in our department of SELECTIONS, the following Report from a late Number of the *Medicinische Zeitung*.

Results of Revaccination of the Prussian Army in 1836. By Dr. SCHLESIER.

"IN this year, 42,124 individuals were revaccinated. Of this number, former cicatrices were distinct in 32,635, indistinct in 6,645, and none were apparent in 2,844. In the revaccinations of this year, regular pustules were produced in 18,136 cases, irregular in 9,940, and in 14,048 the inoculation was at first unsuccessful, but, on repetition, succeeded in 1,569, and remained abortive in 8,205 cases. The number of pustules on each individual varied from one to thirty; of those on whom regular pustules were produced, 7,311 had from one to five pustules; 5,647 from six to ten; 4,418 from eleven to twenty; and 760 from twenty-one to thirty.

"Of cases revaccinated in 1836, or at an earlier period, 14 had during this year an attack of varicella, and 8 of the varioloid disease; but there was no case of genuine small-pox.

"The inoculation was in general performed with fresh matter taken from the arms of infants; sometimes it was necessary to draw the matter from the arm of adults, and only very seldom was it necessary to have recourse to preserved matter. The surgeons of different corps differed in opinion as to the superior or inferior efficacy of matter drawn from the infant or adult, but direct experiment seemed to shew very little difference in their respective powers of producing pustules.

"Compared with former years, the results of the revaccination of 1836 seem to lead to the conclusion that the susceptibility to the vaccine disease, and consequently also to small-pox, is becoming annually greater. Of 42,124 individuals revaccinated in 1836, genuine pustules were produced in 18,136; in 1833, 48,478 individuals were revaccinated, and of these 15,269 with success; in 1834, of 44,454 cases, 16,679 were revaccinated successfully; and, in 1835, of 39,192 cases, success was obtained in 15,315. Or, of 100 revaccinations of each year, 31 were successful in 1833, 37 in 1834, 39 in 1835, and 43 in 1836.

"In 1833, 4,161 were revaccinated unsuccessfully, but a repetition of the inoculation was successful in 784; of 4,830 unsuccessful cases in 1834, the repetition was successful in 866; in 1835, in 9,411 unsuccessful cases, it succeeded in 1465; and 1836, in 1,569 out of 9,744." *Medicinische Zeitung*, 1837. No. 20 and 21.

The results of the revaccination of 1834 are given in our Third Number.

PART SECOND

Bibliographical Notices.

ART. I.—*Transactions of the Philosophical and Literary Society of Leeds; consisting of Papers read before the Society.* Vol. I. Part I. —London, 1837. 8vo. pp. 201.

WE are glad to find that the Philosophical and Literary Society of Leeds is beginning to follow the example of some of the older institutions of a similar kind in this country, (for instance, that of the town of Manchester,) in publishing a selection of the papers read at the different meetings of the members. This is highly desirable in all institutions, the object of which is the dissemination of useful knowledge. We trust that other towns will follow the examples now set them by Leeds as well as Manchester; they will thus prove the means of recording much valuable information, which otherwise may be lost to the public, or at most be only partially disseminated through the neighbourhood of the local Society. A proof of the justice of this remark is offered by the volume before us, which contains several admirable papers, some of which were read as long ago as 1824-25. The nature of our Journal permits us to notice only those which are somewhat connected with our profession; but we may remark, in passing, that the others are not of inferior merit.

The papers which we feel ourselves more particularly called upon to notice are descriptive of the anatomy of three interesting genera of invertebrated animals. The first of these papers, on the Internal Anatomy of the *Limaces* found in the neighbourhood of Leeds, by Mr. NUNNELEY, is exceeding well written, and comprehends the complete anatomy of four species of *Limax*, accompanied by etchings of the parts described, which do equal honour to the anatomist and to the artist. The anatomy of the nervous system is beautifully executed, and, as we can state from our own observation, faithfully dissected and delineated; as are also the alimentary canal and the exterior of the animals themselves. We should, however, have been better pleased had the author been more minute in the descriptions he has given of the different structures, which we feel confident he could well have been.

The second paper, by Mr. TEALE, is on the Anatomy of the *Actinia coriacea*, or Sea Anemone, a curious genus of animals, which has excited much interest among naturalists. The muscular system of this species, with the digestive and reproductive organs, are well described. It is with much satisfaction that we find Mr. Teale has observed the existence of cilia on the Tentacula, and has noticed also the direction of the currents they produce; although, as the author remarks, they had previously been noticed by Dr. Sharpey, and are since described by him in the article *Cilia*, in the Cyclopædia of Anatomy and Physiology. With

regard to the much-disputed subject of the existence of a nervous system in the Actinia, Mr. Teale appears to have been as much unable to detect it as preceding anatomists; although a supposed nervous system in Actinia was long ago described by Spix. Mr. Teale has however observed, as also had M. Blainville, the existence of a grey pulpy cord, within the substance of the lip or mouth, which might probably be the nervous system; but he has been unable to satisfy himself that this is really the case, in consequence of its softness, which rendered him unable to trace it. Altogether, the paper has contributed much to our knowledge of this curious class of animals.

The next, and last, paper we are enabled to notice in this interesting volume is on the *Alcyonella stagnorum* of Lamark, a species of zoophyte found in the vicinity of Leeds, and now for the first time recorded as a British species. The general characters of this species, with its digestive and reproductive apparatus, are well described; and the paper is very creditable to its author as a naturalist.—On the whole, this first volume of the Literary and Philosophical Society of Leeds gives great promise of the future, and we hope that the example set by it will not be lost sight of by other Societies of a similar nature.

ART. II.—*Die Gerichtlich-chemischen Untersuchungen. Eine praktische Auleitung für Aerzte: entworfen vom Dr. C. GUSSEROW, prakt. Ärzte zu Berlin.*—Berlin, 1836.

Researches in Medico-legal Chemistry: designed as a Guide for Medical Practitioners. By Dr. C. GUSSEROW.—Berlin, 1836. 8vo. pp. 76.

DR. GUSSEROW's treatise is entirely confined to the subject of chemical toxicology; and we may truly say of it, that it is one of the best which has yet appeared to serve as a guide for the detection of poisons by chemical analysis. The processes for the discovery of individual poisons are preceded by a well-written introduction, in which clear and concise rules are given for determining whether any unknown substance, presented for examination, be really a poison; and, if so, to what class it belongs. The author follows throughout the rigorous laws of induction, and teaches the experimentalist to arrive, almost unconsciously, at an exact knowledge of the particular nature of the substance which he is engaged in examining. These rules of generalization, which must always be followed in practice, are usually omitted in works on toxicology; and we are therefore pleased to find this deficiency so well made up in the unpretending treatise before us.

Dr. Gusserow confines his remarks to the analysis of those poisons which are most commonly met with, and which in general call for medico-legal examination. The reagents which he employs and recommends are for the most part such as may be easily obtained by the experimentalist. The requisite cautions are given for their use; and, above all things, our author advises that their purity should be determined before they are employed in any analytical process. We may observe, that, throughout, the tests which he uses are perfectly adequate to the satisfactory detection of the particular poison; but here and there he introduces

a few which are not only superfluous, but which may prove sometimes injurious in the hands of those who are not very expert in these matters. Thus, in the analysis of mineral poisons, the use of the hydrosulphuret of ammonia might have been omitted altogether. The sulphuretted hydrogen gas answers every purpose; it is easily made; it does not itself colour the solution to which it is added; and, what is of greater importance, the gas will not redissolve some precipitates which are readily taken up by the hydrosulphuret. The use of lime-water as a test for arsenious acid is entirely exploded in this country, because there are other reagents which afford better and more certain evidence. The subsequent reduction of arsenite of lime with boracic acid and charcoal, or with oxalate of lime, is a more complex process than the method commonly pursued in this country, by the employment of black flux. The use of the "hydrogen test" for the reduction of arsenic, under all its combinations, does not appear to have yet reached Germany.

The plans recommended for the detection of poisons in mixtures containing organic matter are clear and satisfactory. They may, we think, throughout be safely relied on, since they are evidently derived from an extensive practical acquaintance with the subject of toxicological analysis. An easy and simple method for detecting opium, or, in other words, its constituent principles, morphia and meconic acid, is yet a desideratum in toxicology. We find each experimentalist recommending a different plan; and some of these processes, from their tediousness, suffice to discourage many from attempting the experiment. These considerations have induced us to extract Gusserow's method for the detection of morphia and meconic acid in mixtures abounding in organic matter.

"a. The solid contents of the stomach, and other substances, must be sliced, bruised, and treated with successive quantities of distilled water. The different liquids are to be collected, and the solid residue pressed. Too large a quantity of water should not be employed in this preliminary proceeding.

"b. The solid residue, after compression, must be treated with an excess of diluted muriatic acid. Distilled water is then to be added, and the whole allowed to digest for from eight to sixteen hours. At the end of this time the liquid may be poured off, and the residue again well washed and pressed.

"c. The liquids obtained by *a* and *b* are now to be mixed, and evaporated to perfect dryness in a water bath. All organic solutions suspected to contain opium may be treated with diluted muriatic acid, and, after filtration, at once evaporated to dryness.

"d. The dry mass, *c*, must be digested in two successive portions of pure boiling alcohol; the solutions filtered, and then evaporated to the consistency of syrup. Whatever residue is left on the filter should be saved for the subsequent extraction of meconic acid.

"e. The alcoholic solution thus obtained is now to be treated with liquor ammoniæ in excess, and set aside. If opium were present, morphia would be slowly deposited in a confusedly crystalline form. The crystals may be obtained pure by redissolving the deposit in alcohol, and allowing this solution to evaporate spontaneously. The mother liquor may be made to yield an additional crop by evaporating it to dryness, and treating it with a small quantity of alcohol. If the original alcoholic solution, *e*, should not yield any deposit with ammonia, it should be evaporated to dryness, and digested in the same way in a very small quantity of pure alcohol." (P. 72.)

"For the extraction of meconic acid, the residue mentioned under *d* is to be digested in a dilute solution of ammonia, filtered, and the excess of alkali neutralized by

acetic acid. Acetate of lead is now to be added until there is no further precipitation. The precipitate, which is the meconate of lead, is to be collected on a filter, well washed, and then boiled for some time with very dilute sulphuric acid. On filtering this liquid, the sulphate of lead remains on the filter, while the solution of meconic acid passes through." (P. 75.)

This process appears to us to be far better than any which has yet been published. We are aware that the method for separating meconic acid presents nothing particularly new; but the whole description seems to us so clear that we think there are few who would not, with a little care, be able to demonstrate by it the presence of opium in the most complex mixture in which that poison may present itself for examination.

We recommend this treatise strongly to those of our readers who are acquainted with the German language. So far as it extends, it is one of the best works which has yet appeared on the subject of which it treats.

ART. III.—*A Treatise on the Influenza of 1837; containing an Analysis of one hundred Cases observed at Birmingham.* By P. BLAKISTON, M.A. Med. Lic. Cantab., Physician to the Birmingham Dispensary.—London, 1837. 8vo. pp. 60.

THIS is the best treatise that we have yet seen on the subject of last year's epidemic. "It lays no claim to be considered as a history of the disease; . . . no attempt has been made to institute a comparison between this complaint and the epidemic of former years; . . . the author has confined himself to the simple task of recording *facts*, which he has endeavoured to observe accurately and to report faithfully; deducing such conclusions alone as appear incontestibly to result from their analysis." (*Pref.*) We think that Dr. Blakiston has fulfilled his promises, and attained his object; and his work will be useful to the future historian of the epidemic.

The following conclusions are regarded by the author as resulting from his investigations:

"1. The influenza, as observed at Birmingham, is an affection of the nervous system, with its concomitant derangements in the organs of digestion, circulation, &c. commonly known under the name of Nervous Fever; accompanied, throughout its whole course, by irritation of the pulmonary mucous membrane.

"2. This irritation not unfrequently amounted to congestion, and even to inflammation.

"3. The influenza was modified by pre-existing disease, more particularly by chronic bronchitis, the subjects of which were rendered liable to the acute form of that disease.

"4. Neither locality, previous habits, or diet, acted as predisposing causes.

"5. In simple, uncomplicated cases, mild treatment alone was sufficient.

"6. When bronchitis was present, counter-irritation, and large doses of ætherial tincture of lobelia, repeated at short intervals, seemed useful.

"7. Venesection was always counter-indicated.

"8. It was often necessary to have recourse to diffusible stimulants at the commencement of the complaint, and to administer tonic medicines in an early stage of it.

"9. It only proved fatal in those cases where the persons it attacked had been enfeebled by old age or chronic disease." (P. 59.)

ART. IV.—*On the use of Auscultation and Percussion in the Diagnosis of Diseases of the Organs of Respiration and Circulation.* By JULIUS WOLFF, M.D., Member of the Royal College of Göttingen, &c.—*London*, 1837. 8vo. pp. 200.

WE can only account for the appearance of this work by attributing it to the author's unacquaintance with the state of our literature on the subjects of which it treats. As it contains and professes to contain nothing original, and only gives us again what we already possess in many different forms in our language, we fear its author will not be long in discovering that, like other things which are superfluous, it will neither be sought after nor valued. It is, however, due to the author to state, that his work contains a fair compendium of the present state of our knowledge of auscultation, although conveyed in a cold and unattractive manner. We wish that Dr. Wolff, instead of wasting his industry in such a supererogatory labour, had employed his time in giving to British readers—what would have been really useful to them—some of the many classical works of his countrymen which are yet untranslated.

ART. V. *Hints to Mothers for the Management of Health, during the period of Pregnancy and in the Lying-in Room; with an Exposure of Popular Errors in connexion with those Subjects.* By THOMAS BULL, M.D., Physician-Accoucheur to the Finsbury Midwifery Institution.—*London*, 1837. 12mo. pp. 174.

THE nature of this little volume is very accurately pointed out in the title; and we are bound to say, after an attentive perusal of its contents, that it is admirably calculated to fulfil the object contemplated by its author. We are not acquainted with any medical work addressed to non-professional readers which is conceived in a better spirit, or executed in a more satisfactory manner, than these “Hints;” and we are quite sure that they will be of essential service to the class of persons for whom they are designed. The work is marked throughout by good sense and good taste; and, moreover, evinces, on the part of its author, a familiar and thoroughly practical acquaintance with his subject. We recommend it to our readers; and they will confer a benefit on their new-married patients by recommending it to them. We ourselves thank Dr. Bull for this very seasonable addition to our books on Hygiene. We marked here and there a few inaccuracies of language, such as *lay* for *lie*, and one or two nursery vulgarisms, which the author will do well to correct in the next edition. We should not have noticed these immaterial blemishes in a work of so much merit, but from our earnest desire to see the members of our profession more attentive to accuracy and elegance of style than they commonly are.

ART. VI.—*The Nature and Treatment of Diseases of the Ear.* By Dr. WILLIAM KRAMER. *Translated from the German, with the latest Improvements of the Author.* By J. R. BENNETT, M.D., &c.—London, 1837. 8vo. pp. 306.

WE gave so full a review of the original of this work (*British and Foreign Medical Review*, No. V. Jan. 1837,) on its publication, that it is quite unnecessary to say one word, in this place, of its excellence. It is decidedly the best book extant on diseases of the ear, and Dr. Bennett has conferred a great benefit on the profession in this country, by rendering it accessible to every reader. The translation is very well executed. It may be mentioned, as a proof of the great merits of Dr. Kramer's treatise, that it was translated and in preparation for publication, by another gentleman, at the time when the present translation was announced as being in the press. Although we are sorry that our friend was forestalled by Dr. Bennett, the more so as we believe he would have illustrated the original text to a greater extent, by annotations from his own experience, still we are thankful to the author of the present version, which is very creditable to his learning and industry. In these and other signs, we think we see clear indications of a new era in acoustic surgery in this country, when learning and science shall assert their rights, and the despicable ignorance and impudent empiricism which have hitherto prevailed shall be reduced to their proper level.

ART. VII.—*Elements of Anatomy.* By JONES QUAIN, M.D. *Fourth Edition, revised and enlarged. Illustrated with Steel Plates and numerous Engravings on Wood.*—London, 1837. 8vo. Part I. pp. 491.

OF all the manuals of anatomy for the use of students that have yet come before us, Dr. Quain's is certainly the best; and the present edition, owing to the numerous and beautiful plates and woodcuts with which it is for the first time illustrated, is very superior to the preceding. In its present form, the work, when complete, will constitute a most elegant volume, and is calculated, no less for the practitioner who wishes to refresh his anatomical knowledge, than for the student and general reader.

ART. VIII.—*Elements of Chemistry, including the recent Discoveries and Doctrines of the Science.* By the late EDWARD TURNER, M.D. *Sixth Edition, enlarged and revised.* By JUSTUS LIEBIG, Professor of Chemistry in the University of Gressen, and WILTON G. TURNER.—London, 1837. 8vo. Part I. pp. 410.

THIS is a greatly improved edition of the admirable work of the late accomplished professor of chemistry in University College. It is prepared under the superintendence of the author's brother, and is enriched by much new matter by himself and by the celebrated chemist, Liebig. The latter has undertaken the department of organic chemistry, which has been entirely re-written for the present edition.

PART THIRD.

Selections from the Foreign Journals.**ANATOMY AND PHYSIOLOGY.***On the Anatomy of the Eyelids.* By DR. ZEIS, of Dresden.

[It is not so much from the complexity or from the beautiful construction of the eyelids, that their anatomy is regarded with interest, as from their liability to many diseases, some of which are attended with great pain, and are even apt to prove fatal to vision. Our ophthalmological readers are well aware of the numerous and various structures of which the eyelids are composed. In the present article, Dr. Zeis has with great care reexamined several of these structures, and has communicated some new views concerning them, which we regard as important. We shall abridge Dr. Z.'s paper, and add a few comments of our own.]

1. *Skin.* The skin of the eyelids is very thin, destitute of hairs, and, according to Dr. Zeis, presents no trace of sebaceous follicles. Certainly it differs widely in this last respect from the skin of the nose, where the follicles are very evident; but we cannot suppose the skin of the eyelids to be destitute of sebaceous follicles, and we presume that the albuminous tumours, which are not unfrequently met with in this situation, are enlargements of these organs.

2. *Cilia.* We have already (vol. II. p. 454, and Plate II. fig. 2,) given an account of the structure of the hair-follicles and hair-glands, according to Gurlt. Dr. Zeis's researches on the cilia have led him to conclusions somewhat different.

The small whitish bodies, described by Dr. Zeis as hair-glands, lie nearly equidistant from the bulbs of the hairs and their exit through the skin, and surround each eyelash, he says, like a wreath. They are visible to the naked eye, but under the microscope they are seen to consist of a number of little grains. Eble, in his work entitled *Lehre von den Haaren*, represents these bodies, but speaks of them as merely particles of fat, and does not seem to regard them as peculiarly belonging to the hairs. Zeis contends that fat exists only under the skin, whereas these bodies are in the cutis, and within the hair follicles. That Eble has not been sufficiently attentive to them, is evident from his omitting them even when describing and figuring the whiskers of the lower animals, which he regards as the normal, or most perfect, hairs. The roots of the large whiskers on the snout of the ox are surrounded, according to Zeis, each by a very firm fibrous bag, filled with a reddish fluid, and including the hair bulb. If this bag or follicle is slit open in all its length, the little yellowish bodies are discovered surrounding the whiskers, exactly as they do the hairs on the body of the ox, which are not contained within any bag. A transverse section is given by Zeis, exhibiting the whisker, surrounded by a wreath of glands, the whole being contained within the fibrous follicle or hair-bag. The bristles near the eyebrow of the hog are also contained within such follicles, but Zeis could not detect any glands within the follicles in this animal.

The small hairs, which (with the exception of the palms and soles) cover every part of the human body, are destitute of the little glands, and their bulbs are placed merely in small mucous follicles, very similar to the sebaceous cryptæ. But the glands are found surrounding the hairs of the head, those of the beard, and the cilia. In man the glands are generally nearer to the surface, say about the exterior third or fourth of the bulb; but in the hairs of the body of the ox, generally in the middle between the bulb and the exit of the hair from the epidermis. From the regularity of their presence, as well as their being composed of little grains or acini, Zeis

rejects the idea that these bodies are particles of fat, and considers them of a glandular nature. They may be destined either for the nourishment of the hairs, or for furnishing them with an oily varnish.

Gurlt represents these hair-glands, but with several material differences from the views of Zeis. The former represents the acini as much smaller than they appear to the latter. He asserts that in the neighbourhood of the root of each hair, there are generally two, rarely only one, gland. Had he made a section perpendicular to the shaft of the hair, Zeis thinks he would have immediately discovered his error upon this point, for the glands encircle the hair like a wreath. Zeis thinks Gurlt must have made his sections always in the diameter of the hair-follicle, that is, longitudinally. Zeis has not been able to perceive the duct which Gurlt figures going from each gland, and conducting the secretion of the gland into the cavity of the hair-follicle. Gurlt compares the hair-glands to the sebaceous glands, which he calls inversions of the skin. But whatever be their function, whether they contribute to the growth of the hair or serve merely to give it an oily varnish, it is plain they are of a much more complicated structure than the sebaceous follicles, even on the supposition, to which Zeis inclines, that they have no such slender duct as Gurlt represents. A most material point of difference between the representations of these two authors is, that Gurlt figures the glands as lying without, Zeis within the hair-capsule.

3. *Ligamenta Tarsæ Lata.* Winslow described these ligaments as stretching from the edge of the orbit to that of the tarsi, with which he thought they formed a complete layer of the eyelids. Zinn could not trace the ligaments all the way to the tarsi. Haller doubted altogether of their ligamentous texture, and Zeis regards them as merely cellular substance.

4. *Meibomian Glands.* It is generally stated in anatomical works, that the acini, of which each row of the Meibomian glands is composed, communicate, so that their secretion passes from one to another, till it exudes on the edge of the eyelids. A very different idea was adopted by Morgagni, who maintained that each row of acini contained within it an excretory canal, along which the secretion of the whole acini is conveyed. This view is also that of Dr. Zeis, who establishes it by numerous observations both in the lower animals and in the human subject. He states that the canal in man is exceedingly narrow, and its parietes very thin. In brutes, its cavity is comparatively ample, especially in ruminants; the aperture by which it penetrates through the skin is always much contracted. He thinks that in man the canal must give off ramifications or side-branches, as the acini often lie apart from one another: and observes that the rows do not run close together, much less one heaped upon the other, as Soemmering represents them, but in separate lines, and even, in the same row, the acini generally distinct one from another.

The Meibomian glands are evidently conglomerate; and, notwithstanding Müller's assertion regarding the simplicity of their structure in the dog, in no animal are they mere follicles or inversions of the skin. In the dog, the acini are very minute; but still evident under a high magnifying power.* As they appear in the calf, Zeis compares these glands to the pancreas. They present, he says, in that animal, lobes, lobules, and acini. They are comparatively shorter in brutes than in man; and are generally of about the same length in the upper and lower eyelids; whereas in the human subject they are much longer in the upper than in the lower eyelid.

5. *Tarsi.* Anatomists tell us, that the Meibomian glands lie between the tarsi or cartilages of the eyelids, and the conjunctiva. Some say the glands are lodged in furrows, formed on the internal surface of the tarsi. If we look at the edge of the eyelids, it is plain that the apertures of the Meibomian canals are in the substance of the tarsus, and not between the conjunctiva and the cartilage. Dr. Zeis goes farther, however, than this. He maintains, that, in man, the Meibomian glands are entirely lodged in the substance of the tarsi; and that in very few of

* This we state on our own authority. The fact is not noticed by Zeis.—REV.

the lower animals is there any cartilage at all in the eyelids. He says, that on making sections of the human eyelids, either perpendicular or transverse, it is evident that the Meibomian glands are on all sides surrounded by cartilage, and not nearer the posterior than they are to the anterior surface of the tarsus.

In the lower eyelid of the human subject, Dr. Zeis says, he has never been able to discover a true cartilaginous lamina. He thinks the Meibomian glands are lodged there, as they are in brutes, merely in dense cellular substance. In the upper eyelid of the sow, there is a cartilage; but there is none in the eyelids of any other of the lower animals. When we take the eyelid of a calf or of an ox between our fingers, the edge of the eyelid feels so firm and hard, that, without knowing the fact, we should never doubt of the existence of a tarsus. The eyelid of the horse, again, feels soft and relaxed. On dissection, it is manifest that the degree of firmness or of softness depends merely on the state of the cellular substance.

The question naturally arises, Why are the eyelids of man, and especially his upper eyelid, provided with so peculiar a part as the tarsus, of which the eyelids of brutes are deprived? Dr. Zeis is of opinion, that this contrivance is not so much for protecting the eye of man, as for allowing him the widest possible opening of the *fissura palpebrarum*. Were it not for the tarsi, whenever we opened our eyes, the *fissura palpebrarum* would become shortened from angle to angle, and we would thereby lose the advantage, which we at present possess, of seeing objects placed laterally, by a simple movement of the eyes from side to side. Brutes possess this lateral movement in a very imperfect degree; but for this they find a compensation in their neck, which is generally long and moveable. When their eyes are open, little more than their cornea is exposed, and very little of their sclerotica, so that a lateral movement of their eyes would hide their cornea behind the angles of their eyelids. Their *fissura palpebrarum* assumes, then, almost a circular form when their eyes are open. A tarsus would prevent this, as we may observe it to do in the sow. Man's compensation for the shortness and limited mobility of his neck is the tarsi, by means of which his *fissura palpebrarum* is elongated, his eye allowed to turn advantageously from side to side, and his lateral field of vision increased.

Ammon's Zeitschrift. B. II. 3ter u. 4ter. Heft.

Microscopical Researches on the ultimate Structure of the Nerves, and the central Parts of the Nervous System. By Dr. BERRES, Professor of Anatomy at Vienna.

[CIRCUMSTANCES unnecessary to mention here have hitherto prevented us from laying before our readers an account of Ehrenberg's discoveries respecting the microscopical structure of the brain and nerves. The following abridgment of an important paper by Berres, contains an extension of some of Ehrenberg's views, which we hope soon to give at length.]

The forms in which the nervous substance presents itself to us under the magnifying glass, can only be compared to those of canals and vesicles; but, whether it be really hollow, and thus adapted for the reception of more delicate matter or fluid, or whether it does, in fact, contain fluids or an albuminous substance in a half-coagulated state, is still involved in great obscurity. In his numerous researches, Dr. B. only saw, in the larger canals and vesicles, when they were exposed to heat, phenomena strengthening the supposition that they were receptacles for delicate fluids, some of them, under these circumstances, expanded to double their usual diameter. Dr. B. only calls the structural elements of the nerve which are distinctly visible under a glass magnifying 750 times, canals and vesicles, because the shapes of the latter are those which they nearest resemble. The relative position and connexion of these elements, and the diameter of the canals and vesicles, are very diverse; and it is this diversity on which Dr. B. has founded a division of these structures into three natural classes, and of each class into several orders.

This new classification, if proved correct, will be of great value, inasmuch as it

professes to indicate the *function* of nerves, the use of which is unknown, and expresses their analogy with others of the same class. All the nerves of the human body may be divided, as to their ultimate structure, according to Dr. B., into three classes: *1st Class.* Nervous structures composed of canals swollen at intervals, like a string of beads, (*tubuli moniliformes.*) *2d Class.* Ditto, composed of delicate canals, with vesicles attached, (*tubuli baccati.*) *3d Class.* Ditto, of intussuscepted, invaginated canals, (*tubuli invaginati.*) Of the first class, are the nerves of the particular senses; of the second, the nerves of sensation; of the third, the nerves of motion. After a long description of his classes and orders, our author arrives at the important result, that in all the higher organs of sense, may be demonstrated, firstly, a nerve of a particular sense; secondly, a nerve of sensation; and, thirdly, one or more nerves of motion.

Other alleged discoveries of Dr. B. are the following: The structure of the brain and of the spinal marrow is similar to that of the first class of nerves. The nerves of a particular sensation and the sympathetic seem to run into the nervous centres without alteration or interruption; but those of the other classes lose their tegumentary membranes as they approach the latter, and change, thus, their structural arrangements, whilst the canals which compose them terminate by interlacing with the canals and vesicles proper to the brain; all these form together the higher organization of the nervous centres.

Oest. Med. Jahrb. Vol. ix.

On the Microscopic Structure of the Brain, and of the Tissues intermixed with it.
By Dr. LUIGI CALORI.

AFTER describing the three principal forms of the ultimate structure of the brain observed by anatomists, viz. the tubular of Ehrenberg, the fibrous or cylindrical of Alex. Monro and Fontana, and the globular of Della Torre, he mentions his own observations, which have led him to conclude that all these forms may be produced and rendered apparent by attention to certain particulars. The most uniform appearance is the globular; but the shape varies much with the mode of preparing the brain for examination, and with the degree of illumination to which it is subjected under the microscope, as well as with the magnifying power: nevertheless the globular is most probably the real form of the nervous matter.

The tissues that are intermingled with the nervous substance and give it support, are very fine and transparent cellular substance, and the blood-vessels pointed out by Prochaska, of which Calori asserts that no one has hitherto given a full and perfect description. That the vessels of the medullary and cortical portions are not identical is clear from their difference of caliber and distribution, and hence Gall was wrong in asserting that the latter was the source of the former. These vessels form a remarkably fine network, quite disproportionate to the size of the primary vessel, and similar to the distribution of the vessels on the surface of the Allantoid membrane. The nervous matter is united to these vessels within whose ramifications it is collected; and, if really globular, it approaches most closely to the adipose tissue of Malpighi in its general appearance.

Bullettino delle Scienze Med. de Bologna. Sett. 1836.

On the Fibrous Membrane beneath the Pleura Pulmonalis. By M. BAZIN.

AFTER quoting the opinion of Colombo, the pupil and successor of Vesalius, that the pleura is composed of two laminae; between which the blood-vessels and nerves are distributed, the author of this memoir states that the researches of succeeding anatomists have led to a different conclusion, in consequence of their having been confined to the pleura costalis. His own extended investigations into the structure of the respiratory organs in the series of vertebrated animals, have led him to the conclusion that the lungs, like other organs, possess a proper capsule. He remarks that capsular envelopes are found to exist in three states: 1, a fibrous network with large interstices; frequently taken for cellular tissue; 2, a complete

fibrous or sclerous membrane, whose density and thickness may be variable; 3, an osseous plate. The capsule of the lungs in most animals presents the first condition, that which exists in man; but in the elephant, a distinct fibrous membrane is met with, consisting of bundles of parallel fibres interlacing at certain points with others, like the muscular coat of the bladder. The lung of a panther, which died of phthisis, presented M. Bazin with a hypertrophied condition of this membrane, which in the healthy state of this animal is not thicker than in man.

Annales d'Anatomie et de Physiologie. No. I. 1837.

Examination of the Bodies embalmed by SIGNOR TRANCHINA.

IN our Fourth Number (vol. II. p. 543,) we gave a short account of Tranchina's discovery of a method of preserving dead bodies by the injection of a solution or mixture of arsenic into the arteries. Two bodies were thus embalmed, three days after death, at the military hospital of Naples; and the following is the account of the Medical Faculty of the University appointed by his majesty to examine and report thereon.

The two bodies, injected on May 15, 1835, at the military hospital, and dissected on December 11, 1835, exhibited the following appearances:

The integuments were wrinkled and the flesh dried and shrivelled. The skin of one was livid-red, and blackish-red of the other. There was no mark of decomposition in either; they were only covered with mould.

The brain and cerebellum were in their natural state, firm and elastic, but diminished in size. The cerebral tissue was wonderfully preserved; the cortical substance alone had become brownish.

The heart was in its natural state. The lungs were somewhat dried and softened; but, when cut into, they gave no signs of putrefaction, nor any disagreeable smell.

The abdominal viscera were rather dry and flaccid, of a dark colour, but inodorous. The feces and urine, in their respective receptacles, gave no proofs of putridity.

The arteries were empty, shriveled, and contained saline concretions. The veins contained liquid uncorrupted blood, that readily followed the incisions.

Filiatre Sebezio. March, 1836.

On the Action of Strychnia on the Nervous System. By Dr. H. STANNIUS.

IN the following experiments, the nitrate of strychnia was preferred, on account of its greater solubility, to the other preparations of that alcaloid. Dr. Stannius introduced, beneath the integuments of strong healthy frogs, a few drops of a concentrated solution of the nitrate, and the following effects were generally observed: The animal continued to comport itself as usual for a short time, but, in from five to fifteen minutes, it became more quiet, taking at intervals short leaps, till one tremendous bound succeeded, followed instantly by the most energetic tetanic convulsions, which continued without interruption for about a minute. They then generally ceased; but the slightest shaking of the table, the least touch, or even the vibration of the air caused by speaking, sufficed to reproduce them instantly. This acute sensibility gradually diminished; which consequence Dr. Stannius ascribed to the absorption of a larger quantity of the poison. In analyzing these effects, we observe, firstly, an affection of the voluntary muscles; and, secondly, a greatly increased susceptibility to all external impressions. These phenomena may be owing, firstly, to the action of the poison on the brain; secondly, to its action on the extremities of the centripetal nerves, which, by reflecting it upon the brain and spinal marrow, might produce the anomalous phenomena; or, lastly, it may be owing to the direct action of the poison on the spinal marrow. The first supposition is negatived by experiments, which shew that the destruction of the brain does not prevent the occurrence of tetanic convulsions after application of the poison, and by others in which the spinal cord was divided above the origin of the

nerves of the posterior extremities, and in which convulsions nevertheless ensued, both in the anterior and posterior half of the body. Secondly, does strychnia act primarily upon the extremities of the nerves through the instrumentality of the blood?

The following experiments may be regarded as answering this question in the negative: the spinal column of a frog was divided immediately above the origin of the nerves of the posterior extremities; the posterior portion of the column was then carefully separated from the soft parts, and the blood-vessels supplying it cut, so as to leave it in connexion with the other parts of the body solely through the nerves of the posterior extremities. These nerves were then freed from all adhesion to the adjacent vessels, muscles, &c. in that part of their course which lies between the spinal column and thighs; so that in this manner the posterior portion of the spinal marrow, and, to some extent, the nerves arising from it, were completely isolated from the blood-vessels. The blood-vessels and nerves retained their normal situation in the extremities, in which the circulation continued to be carried on, and mechanical irritation of the toes of the posterior extremities, by inducing the animal to retract them, shewed that the functions of the posterior half of the medulla continued to be performed. If the poison, therefore, act upon the extremities of the nerves, and its action be through them reflected upon the spinal cord, we should still expect tetanic convulsions both in the anterior and posterior half of the body. But this is not the case; the posterior extremities are not convulsed, although they still contract on the application of mechanical irritation. We thus arrive, by negative proof, at the conclusion that strychnia acts primarily upon the spinal marrow, and that from it result both the convulsions and increased excitability.

Here the question arises, Do these convulsions suppose also an alteration in the centripetal nerves, or is the excitability of the spinal marrow increased to such an extent that, without any change in these nerves, common impressions suffice to produce the most fearful spasmodic paroxysms? To decide this question the following experiments were instituted: the spinal canal of a frog was laid open, and the posterior roots of the nerves of the posterior extremities cut; the animal was then poisoned, and the usual phenomena appeared: the posterior extremities were fully convulsed, but mechanical irritation had no effect upon them, and the convulsions in the posterior extremities ceased as soon as the cord was divided above the origin of their nerves. Again: the spinal canal of a frog was laid open, and the posterior roots of the nerves of the posterior extremities cut across. The cord was then divided above the origin of these nerves, and the animal poisoned. No convulsions followed in the posterior extremities, nor did irritation of the spinal ends of the cut posterior roots produce them. These experiments were repeated several times with like results; and hence Dr. Stannius infers that the centripetal nerves are necessary for the appearance of tetanic convulsions, and that increased susceptibility to external impressions is not confined to the spinal marrow; as, in that case, irritation of the spinal ends of the cut posterior roots, or of the posterior aspect of the spinal cord itself, would give rise to the peculiar tetanic convulsions. We know, from the above experiments, that the poison is devoid of influence upon the extremities of the centripetal nerves, and we thus necessarily arrive at the conclusion that the centripetal nerves receive from the spinal cord an increase of their excitability; and that, thus charged, they react upon the medulla and occasion the peculiar convulsions.

In experiments with strychnia, the blood has generally been regarded as the vehicle of the poison. This opinion derives confirmation from the facts that some time is required after the introduction of the poison beneath the integument for its action to become apparent, and that no effect follows its immediate application to the spinal cord or nerves. Dr. Stannius was unable to recognize any alteration in the appearance of the blood of poisoned frogs by means of the microscope, and he disproved the opinion that the act of respiration is necessary to impart to blood already charged with the poison, the power of acting upon the central organs of the nervous systems, by shewing that the same symptoms occurred in frogs which had previously been deprived of their lungs.

Professor Müller has lately called attention to the fact, that a solution of the aqueous extract of opium is possessed of a purely local action upon nerves with which it is brought in contact. To ascertain in how far strychnia possesses the same property, Dr. Stannius prepared three posterior extremities of frogs. The extremities of the nerves of one were steeped in a solution of opium; those of a second in a concentrated solution of strychnia; and those of the third in pure water. The nerves of the first shewed no susceptibility of mechanical irritation beyond a quarter of an hour; those of the second retained their excitability for an hour; and those of the third, for several hours. The stimulus of galvanism continued to affect the extremities of the nerves of the first for three-quarters of an hour; those of the second for an hour and a half; whilst those of the third, at the end of two hours and a half, still retained some excitability.

Müller's Archiv. Heft ii. 1837.

Experiments on Animals with the Blood of Cholera Patients.

By Dr. NAMIAS, of Venice.

UPON opening the body of a man, aged fifty, who had died in the cold stage of cholera after twenty-four hours' illness, at the Hospital of St. Daniel, the blood in the cavities of the heart was found to be black and congealed together, with one or two polypous concretions. A portion of the congealed blood, of the size of a strawberry, was inserted, without causing much suffering, under the skin of the thigh of an old and fat rabbit. The fur was shaved off, and an incision having been made through the skin, it was separated by the handle of the scalpel from the cellular tissue beneath; and into the cavity thus formed the blood was introduced, and the wound was then carefully closed with sutures. This mode of inoculation was followed in all the experiments.

Experiment 1. Five days after the operation the rabbit appeared ill, its evacuations were less solid than usual, and a whitish glutinous matter was observed upon the ground. The animal was found dead on the eighth day. The blood of the heart was black and grumous, with some fibrinous concretions; the bladder was full of urine; the injected blood had pervaded the surrounding tissues; the lips of the wound had their normal consistence; the internal surface of the whole of the skin was covered with blue spots of ecchymosis; but the rest of the organs exhibited no alteration.

Experiment 2. An equal quantity of the blood of this rabbit was injected in a similar manner into the thigh of a grey female rabbit. It was found dead in twenty-four hours afterwards, with the same appearance of the whitish matter upon the ground. The body when examined exhibited similar results as in the former experiment. The author here judiciously remarks that the rapid death of the second rabbit arose not only from its weaker age and sex, but also from the blood of an animal of the same species being more readily absorbed than human blood.

Experiments 3 and 4. The blood of the last rabbit was inserted into two others that died in six days with the same marks of disease. These rabbits were much larger than the former.

The next experiments were performed with the blood of a man who died in the cold stage after twelve hours' illness. The blood was black and grumous, but there were no concretions.

Experiment 5. This was a fat old grey rabbit of the male sex. It died five days after the operation. There was no whitish matter on the floor; the bladder was empty; there were many brown spots on the inner surface of the skin. The state of the blood and of the wound that was not of a kind to affect the health of the animal, was the same as in the other cases.

Experiment 6. Performed with the blood of the last on a male rabbit two pound and a quarter in weight. It died in forty hours. Bladder full; blood black and fluid; the usual spots on the skin. In this instance also, the poison transmitted through a similar animal acted more rapidly than when taken directly from man.

Experiment 7. Another rabbit, inoculated with the blood of the last, was found dead in six days.

Experiments 8 and 9. In these two experiments, for the sake of greater accuracy, the doctor weighed both the rabbits and the blood. Into one, weighing three pound and a half, he injected ten grains of blood from the last experiment; and into the other, weighing three pounds, he inserted eight grains of the same blood. Both died in six days.

The next experiments were undertaken with the view of ascertaining whether human blood taken from persons not dead of cholera would produce the like results. Three rabbits were inoculated: one, of two pounds and a quarter, with seven grains of black fluid stinking blood from the heart of a patient dead of gangrene of the bowels: a month afterwards the animal was well and vigorous. Another, of three pounds and a quarter, with the blood of an aneurismatic patient, of which twelve grains were used; and the third, of two pound two ounces, with fourteen grains of the same blood, which by its quantity only would probably be fatal: on the twenty-sixth day afterwards the larger rabbit was sprightly and vigorous, the smaller dull and out of condition, with a large wound on the seat of the inoculation.

Dr. Namias considering the reaction in cholera as a beneficial effort of the vital powers to eliminate from the body matters that are incongruous with its tissues, to restore the deranged state of the sanguiferous system, and to renew the exhausted nervous energy, was inclined to believe that the blood of persons dying in this stage would be less injurious than that from persons in the pulseless state; and some experiments that he undertook with Professor Rima seemed to confirm this notion. Twelve grains of the blood of a female, dead after three days' illness with imperfect reaction, were inserted in the usual way into two rabbits, and a third was made to swallow a scruple of the same blood. All three survived, but a great number of spots, produced by the drying of a white glutinous matter, were found in the hutch of the two former. The other appeared to vomit the blood it had swallowed, and for some days after it vomited a similar white substance. Four scruples were injected under the skin at the back of the neck in a little dog. This blood had been taken from a woman dead of cholera in seventy-two hours after incomplete reaction. For the first two days the dog passed along with natural feces many small white worms; on the third day he vomited milk he had lapped; and he died early on the fourth day. Incipient suppuration was observed under the separated skin; the bladder was quite empty; the heart contained very fluid blood in small quantity; and there was no evident cause of death.

Bullettino delle Scienze Mediche. September, 1836.

An Experimental Examination into the Opinions of Sir CHARLES BELL relative to the anatomical and physiological Characters of the Spinal Marrow. By HENRY H. SMITH, M.D., resident Physician in the Pennsylvania Hospital.

BELIEVING that the experiments of Sir Charles Bell on this subject demanded a closer examination, particularly as many of them seemed to be based on somewhat supposititious grounds, I repeated a number, and especially those made to prove the connexion of the middle column with the function of respiration. In order to conduct my examination in as impartial a manner as possible, I in each instance pursued the following order: first, to read only the experiment as stated by the author; and, second, to repeat it as nearly as possible in the same way; always, after the first operation, dissecting the portion operated on, so as to make myself sure of its correctness.

Commencing with his experiments on the anterior and posterior columns of the spinal marrow, I found, after having repeated them several times in various ways, that they all tended to confirm the opinion which he had expressed of their function. Passing over, therefore, the detail of these experiments, (which, as they proved nothing new, might not repay the perusal,) let us proceed to examine the grounds on which he has founded his opinion of the existence of a middle column.

Experiment 1st. A good-sized kitten was struck lightly on the head, so as to stun it slightly, but without affecting its respiration. The skin was then dissected

back, and the muscles removed from the spinous process of the sixth, seventh, and eighth dorsal vertebrae. The points of the scissors being then carefully introduced, the processes were removed, and the canal opened for about two inches. The cord was now raised upon the forceps, and a thin, flat instrument, somewhat similar to a gum lancet, introduced on the right side at the lateral fissure, and carried through to the opposite side. The action of the abdominal muscles, as well as the intercostals, were closely watched by a friend during this operation, but no change in their movements was visible. The instrument, being withdrawn, was again introduced with its flat side to the sacrum, and pushed through so as to separate, for the space of two lines, the anterior from the posterior columns; but without having any effect on the muscles of respiration. By this mode of operating, the anterior column was of course separated from the posterior, but remained otherwise uninjured; while, at the same time, any middle portion must necessarily have been divided. The instrument being removed, the animal continued breathing as readily, apparently, as if unhurt.

Experiment 2d. By the same operation, the spinal marrow was exposed at the upper dorsal vertebra. The same instrument was in like manner introduced between the columns here, and allowed to remain some minutes, but without producing any change in the action of the muscles. The animal becoming faint from loss of blood, the instrument was removed, respiration continuing easy. Death was now instantly caused by the insertion of the knife between the occiput and atlas.

Experiment 3d. The spinal marrow of another kitten was exposed between the last cervical and first dorsal vertebrae, and immediately divided with the scissors in a transverse direction. This paralyzed the lower extremities, but produced no material change in the action of the abdominal muscles, except that their expansion and contraction was not quite so full as before. The change was, however, very slight; but it was very evident here that the abdominal muscles did not act by themselves, but that they were materially influenced by the diaphragm, as they expanded and contracted at the same instant that it ascended and descended. The cord was afterwards divided at the fifth cervical vertebra, but without any very material change, except that it rendered the action of the diaphragm more plain and completely paralyzed the intercostal muscles.

Experiment 4th. The cord was exposed in the spinal canal of another kitten, and divided at the middle dorsal vertebra. In this instance the abdominal muscles were affected in the same manner as in a former experiment, although the effect upon respiration was rather more marked in this instance, as the operation was performed more easily, and the animal was allowed to live for a longer time. At the close of some minutes, respiration still continuing nearly natural, life was destroyed by the division of the cord at the first cervical vertebra.

These experiments were repeated twice on other kittens, and with the same results.

Experiment 5th. By a transverse incision on the occiput, the skin of a kitten two months old was divided, and then dissected forward to the crown of the head, and backwards to the sixth cervical vertebra. The spinous processes of the last three cervical vertebrae were now carefully removed with the scissors, and the cord exposed. The roots of the seventh, sixth, and fifth nerves were next cut, when respiration, which before went on naturally, became slightly laboured. The roots of the three next nerves were then also divided, when the action of the diaphragm ceased entirely, and likewise that of the abdominal and intercostal muscles. The animal, however, did not die instantaneously, but continued gasping for near a minute, when death took place; but, previous to this event, motion was observed in the extremities and tail, which was increased on touching the cord. Sensation was not tried, as the time was too short between the division of the roots and the death of the animal; but it no doubt remained, as well as motion.

Had there been in the spinal cord any middle portion especially devoted to respiration, as Sir Charles supposed, the division of it would have been caused by Experiment 1st, as, owing to the manner of operating, its division was ensured,

while at the same time the other columns would remain uninjured; thus preventing the action of the muscles supplied by them from being interrupted. According, however, to his theory, this division ought to have arrested the action of the respiratory muscles; whilst, on the contrary, they were not all affected by it! If, again, we admit that an injury to the upper part of the spine does affect respiration, then we must acknowledge that it does so through the medium of the anterior cord and roots; for Experiments 3d and 4th shew that the division of these portions rendered respiration laborious to a slight degree, though it did not stop it entirely. We are, therefore, I think, justified in believing that there is no distinct middle column devoted to respiration; but that, so far as the muscles *are* influenced by nerves below the fifth cervical vertebra, they receive that influence through the anterior roots. Did not these experiments induce us to believe this, the fourth alone would almost do so, as we there see that the division of the upper cervical nerves not only stopped the action of the diaphragm, but also that of the abdominal and intercostal muscles. That the cessation of motion in these muscles was not caused by injury to the anterior column, is apparent from the motion observed in the hind legs after the operation, as well as from the impossibility of such a mishap occurring from the way in which the operation was performed. From these results, I think, we must conclude that the diaphragm is the only proper respiratory muscle, and that the abdominal and intercostal ones are only secondary and mechanical in their action.

The American Journal of the Medical Sciences. August, 1837.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

DIFFERENT VIEWS OF THE NATURE AND TREATMENT OF TETANUS.

I. *A Case of Tetanus, confirmatory of the Views of BELLINGERI.* By SIGNOR MOROTTI FRANCESCO, of Vercelli.

[We give this case as we find it in the original Journal, with the observations of the author. We will only remark that, whether the case is confirmatory or not of the views of the celebrated Italian physiologist, the dependence of particular symptoms observed during life on particular organic changes observed after death, is by no means logically proven. The case is nevertheless interesting, as establishing the locality of the disease viewed as a whole.]

Dominico Pozzo, æt. twenty-three, of a strong constitution, after working hard and perspiring profusely in the hayfield, was seized in the evening of August 19 with rigors and cold sweats. The next day he had fever, with violent pain in the forehead and vertex, drawing the head downwards and forwards, and stitches in the side during inspiration; and on the day following, a pain down his neck in addition to the other symptoms. On the 27th, notwithstanding a general bleeding and leeches to the throat, the symptoms increased, and the patient perceived a drawing of the whole trunk forwards. The head was bent to the chest, the facial muscles were contracted, the lower extremities were spasmodically bent and drawn towards each other, and the motions of abduction and elevation of the upper arms were painful and difficult, but the forearms and hands moved easily in every direction. Received in this state into the hospital at Vercelli, he was bled largely. The blood was rather buffed. The next day, 28th, the emprostotonos was found to be changed to episthotonos; head and neck drawn back and a little to the left side, pain in the occiput, which by degrees became more piercing, that in the head having ceased; sight, hearing, and intellect unimpaired, speech hesitating and slow, features drawn and convulsed into a risus sardonius, lower extremities spasmodically extended, upper arms slightly extended and abducted, respiration difficult, with abundant expectoration, pulse slow and contracted. An inflammation of the cerebellum being suspected, he was treated accordingly.

31st. Trunk curved backwards, with acute pain and severe spasms of the head

and neck, *alæ nasi* drawn upwards, lips drawn apart, lower jaw almost closed with trismus, pulse hard and very frequent.

September 1. Opisthotonos, pain in occiput and neck less, but extending all down the spine, heat of skin natural, head and chest bathed in sweat, pulse variable. In the evening, stupor, opisthotonos, sensation impaired, legs widely extended, but allowing flexion till a fresh spasm extended them. Death at midnight.

Examination thirty-two hours after Death. No stiffness nor curvature of neck or trunk. *Pia mater* and arachnoid generally injected, some serum in the cavity of the arachnoid, and a little effused blood between the convolutions of both hemispheres. Substance of the brain hard and resistant to the scalpel, and somewhat engorged, a little serum in left ventricle. State of cerebellum was the same. The membranes of the spinal cord were injected, and the posterior columns of the cord were soft as pap, and covered with a puriform serum; the anterior columns presented evident marks of inflammation, and their medullary matter was as hard as that of the cerebral lobes. The left pleura was inflamed and adherent, and the right lung, hepatized in various parts, floated in a large quantity of extravasated blood.

In this case it is evident that the inflammation began in the cerebral lobes and their membranes, as was indicated by the pain in the head, which, setting in on the first day, extended at length to the neck, and probably to the anterior columns only of the cord. To this inflammation, continued till the eighth day, must be attributed the emprosthotonos which appeared on that day, and the state of adduction of the extremities; thus confirming the opinion of Bellingeri that the cerebral hemispheres preside over the motions of flexion and adduction, and that when they, or the anterior columns of the spinal cord, are irritated, emprosthotonos is the result.

On the ninth day the disease attacked the cerebellum, as was proved by the occipital pains, which began on that day, and by the change from emprosthotonos to opisthotonos, with convulsive movements of extension and abduction of the extremities. The backward curvature of the trunk on the eleventh day appears to be owing to the supervening inflammation of the posterior columns of the cord as revealed by the dissection; and the general opisthotonos to the affection of the cerebellum and cord together. This again confirms the opinion of Bellingeri that the cerebellum and posterior columns determine the movements of extension and abduction; and that their irritation produces opisthotonos.

Annali di Medicina. Gennajo, 1835.

II. *Case of Tetanus, with Remarks.* By Dr. BRUGGEMANN, of Magdeburg.

ON entering the hospital one morning, Dr. Bruggemann remarked a young man pacing the ward, apparently wrapped in meditation, with his face directed towards the ceiling, but with something comical in his expression; which, together with his inattention to surrounding objects, led Dr. B. at first to consider him as insane. On questioning him, however, it appeared that, on the 8th of November, he had experienced some difficulty in deglutition, with slight pains in the neck; on the 9th, he complained of difficulty in masticating, and could not open his mouth fully; on the 10th, there was stiffness of the neck; and, on the 11th, (the day in question,) the free motion of the limbs was impeded, and he felt uncomfortable whilst lying, sitting, or even standing, and found himself most at ease when walking. The face was flushed, the pupils enlarged, and the mouth capable of being opened only about half an inch. The masseteric muscles felt hard; deglutition was difficult; he suffered from thirst and hunger. The arms were fixed in a semiflexed position; he could not bend the body forwards; and the dorsal muscles felt hard to the touch. The respiration was free; pulse 110, small, and soft; temperature of skin natural. Every few seconds he suffered from violent shooting pains, which passed like an electric shock from the spine over the chest, and were accompanied by a convulsive twitching of the muscles, particularly those of the back, neck, and face.

Twelve cupping-glasses were applied along the spine, and forty leeches to the

nape of the neck; six ounces of blood were drawn from the arm, and two grains of calomel given every two hours. During the night, the shooting pains were more severe; the patient had profuse acid perspiration, but the other symptoms were mitigated. On the 12th, he had an alkaline bath; opium was added to the calomel, and a blister was applied to the nape of the neck. On the 13th, the pains were somewhat relieved, but the other symptoms continued much the same. The bath was repeated, and two grains of opium were given every two hours, alternately with a decoction of bark containing muriatic acid. The patient complained of hunger and thirst, the respiration was anxious and accelerated, and the pulse at the wrist could not be counted; the skin was moist, and the patient was affected with giddiness. At six, there was some delirium, which was followed, at nine, by extreme anxiety; the limbs were then powerless and relaxed, the respiration slow, pulse 160; the sphincters paralyzed. Death followed in about an hour.

On opening the spinal column, about thirty-six hours after death, some dark-coloured blood escaped, and an extravasation was found between the second and third cervical vertebrae. A second extravasation extended from the seventh cervical to the first lumbar vertebra, and the whole spinal canal contained a large quantity of dark half-coagulated blood. The vessels of the spinal dura mater were every where injected, and the vasa spinalia were gorged with blood. The substance of the spinal marrow was normal. The superficial cerebral vessels were gorged with blood, but the large vascular branches in the base of the cranium were empty.

After detailing this case, Dr. Bruggemann proceeds to theorize on the cause of tetanus; and, dismissing inflammation as unsatisfactory, seems rather inclined to acknowledge rheumatism as one of its most frequent causes. As the results of his own experience, he would recommend the following mode of treatment, founded on the conviction that the disease *must* be curable, could we only have sufficient boldness to stand firm and carry through some energetic plan of treatment. In the first place, he would commence giving opium in doses of three or four grains, repeated every quarter of an hour till sleep was induced, which is the great point to be gained. On the patient awakening, the most powerful stimulants and tonics, such as wine and quinine, are to be administered; to be followed, should the convulsions still continue, with repeated doses of opium till sleep again ensues; to be again succeeded by stimulants and tonics.

Wochenschrift für die gesammte Heilkunde. Nos. 19, 20, and 21. 1837.

III. *Tetanus caused by exposure to Wet, and cured by a Moral Impression (?)* By Dr. FERRUS.

DR. FERRUS relates a case of a young man, who, having danced a whole evening in company with his sweetheart, and having drunk much, lay down on the turf, and slept during a storm, to which he was completely exposed. On awaking, he felt much pain in his limbs, and had a good deal of fever during the night. The next morning, trismus came on, and was followed by other tetanic symptoms. These soon became so severe as to prevent his lying down in bed; a convulsion of the whole body occurred nearly every minute; there was complete inability to swallow and to void urine; total absence of sleep. The treatment consisted in bleeding from the arm, cupping and the application of the actual cautery along the spine; opium was given in large doses, by means of a canula inserted into the mouth. These remedies produced no effect; the patient continued to get worse, and on the seventh day the physician left him in despair to the care of the priest and the notary. On leaving the house, he met the young woman to whom his patient was attached, and who was in the utmost distress at being prevented from visiting him by the interference of his mother, who was opposed to their union. By the assistance of the kind priest he succeeded in procuring an interview between the lovers, which was followed by the happiest effects. Abundant tears were shed; the convulsions diminished, so that in the evening he could be placed in bed, when he obtained three hours' sleep; there was an abundant secretion of urine, but the

bowels remained obstinately constipated for a considerable time. The patient now continued to recover gradually, and was finally restored to health.

Journal des Connaissances Medico-Chirurgicales. May, 1837.

IV. *Of the Successful Employment of Colchicum in Tetanus, in Hayti.*

By WILLIAM G. SMITH, M.D., of Port au Prince.

IN Hayti, as in all warm climates, tetanus prevails to a considerable extent, but it would seem most confined to the natives, as I have never known a case to occur in an European, or any stranger whatever. It also appears, that persons in this country, from the age of five years to thirty, or thirty-five, are most predisposed to attacks of this disease. In the course of little more than two years and a half, nine cases of tetanus have fallen under my observation, and the greater number of these were under my own care. Women do not seem to be so susceptible of tetanus in any of its varieties as men: of the nine cases, only one having occurred in a female. Of the nine cases, two were traumatic, and were induced by very slight wounds, the remaining seven were idiopathic, no direct cause being present.

Traumatic Trismus . . . 2 males, under the age of 20, 1 died, 1 cured.

Idiopathic Opisthotonos { 3 males, under the age of 30, 1 died, 2 cured.

{ 1 female, under the age of 40, died.

{ 2 males, under the age of 25, cured.

Idiopathic Pleurosthotonos . 1 male, aged 15, died.

Total, 9: cured, 5; deaths, 4.

The last four cases of tetanus which have come under my care, were treated with the *Colchicum Autumnale*, and three out of the four recovered. The following is the general course I pursue, in the treatment of the disease: on being called to a case of tetanus my attention is first turned to the actual state of the bowels; constipation in this disease is at least a grave symptom, and should be relieved by injections. I prefer some emollient fluid for this purpose; because I believe that in almost all cases, there is, conjoined with costiveness, some degree of intestinal irritation. I commenced by administering an enema of lbj. decoction of either flaxseed or the okra, (*Ketmia gombo*,) cum. ol. ricini, ℥ij. or ℥iv. for an adult. After this, fifty or sixty leeches are applied to the spine, from the neck to the sacrum; in my particular practice, I have made choice of the scarificators and cupping-glasses, and the result has been most satisfactory. When the muscles of the jaws and neck are affected, as is most usually the case, leeches are also applied to the mastoid processes; the moment the cups or leeches, whichever may have been used, are removed, cloths wrung out of a strong solution of the muriate of ammonia are constantly applied to the whole of the vertebral column, the back and neck. Internally I administer the vinous tinct. of colchicum, commencing with 3ss.; the dose is increased every half-hour, and repeated until emesis or catharsis has been produced. As soon as one or other of these effects is obtained, this remedy is to be discontinued; if there afterwards occur any colic or griping, with exhaustion, as in all probability there will, I am in the habit of giving the spt. mindereri 3ss. every hour, with the addition of one-fourth of a grain of acetate of morphine, in solution. If the surface be cold, and there be symptoms of collapse, warm applications are made to the extremities and the axilla; and the muriated solution is discontinued. The seeds of the colchicum appear more permanent in effect than the bulb. I accordingly make a tincture as follows:—R. Sem. colch. sicc. ℥ij.; Vin. albi. hisp. lbj. Infund. &c.

All the cases of tetanus which have fallen under my care have been, with two exceptions, treated as above stated, with a very happy result. The exceptions were the first two cases I treated in this island, and in which I employed opium, general venesection, terebinth, &c.; the patients were put in a warm bath, but on being taken out of the bath, the spasms became more alarming, the limbs more rigid, and in one instance, death immediately followed. I shall never again treat tetanus, while in the West Indies, as recommended in the books. I was first

induced to employ the colchicum for the cure of tetanus from the accounts given by Mr. Haden, of London, of its good effects in chronic rheumatism, and inflammatory diseases, as well as from previous experience, while resident Physician at the House of Refuge in New York. Dr. Ives, of that city, frequently employed the tincture in rheumatism. The effect I invariably observed it to produce led me to believe that the colchicum possessed qualities both as diaphoretic, diuretic, and actively cathartic, besides being antispasmodic and anodyne. I am now certain that in tetanus, it acts in a manner altogether peculiar. In tetanus there is obstinate constipation, violent spasms, muscular rigidity, with pain, often retention or suppression of urine. Now the colchicum, when genuine, appears to combine in itself sufficient qualities to control several of these symptoms at once. It, however, does not invariably promote vomiting; this may depend on its genuineness, but it almost invariably acts upon the kidneys, the skin, and the bowels.

The Jamaica Physical Journal, for June, 1836.

V. Tetanus cured by Acetate of Morphine, applied endermically.

By Dr. HENRY.

THIS was the case of a young man, over whom passed the wheel of a carriage, by which two ribs and several of the metacarpal bones of the right hand were fractured, with great laceration of the soft parts. He was bled, and the fractured parts replaced. Until the fifteenth day, all went on favorably, when there suddenly occurred the symptoms of tetanus; fixed look, stupor, trismus, contraction of the muscles of the neck and superior part of the thorax, &c. A grain and a half of acetate of morphine was spread upon the surface of the wound, and an ounce of syrup of morphine administered internally, by means of a tube passed through the interval left by a tooth which had been extracted. The symptoms ceased during the following day, when the same remedies were employed; the next day, they were omitted, when the symptoms returned more formidably than before; the patient sitting upon his bed with his head almost between his legs, and being unable to lift it, (*Emprosthotonos*.) An abraded surface was made by ammonia, and two grains of acetate of morphine were applied to it; the application was continued, and the patient recovered.

Bulletin de l'Academie Royale de Médecine. No. 2. 1836.

On the present Predominant Views regarding Acute Rheumatism.

By Dr. HENRY ZERONI, of Mannheim.

THIS is an important paper, and its chief object is to direct attention to the fallacy of Bouillaud's views concerning the nature of acute rheumatism, and, consequently, likewise to the danger of pursuing this author's mode of treatment. Judging from an attentive perusal of Bouillaud's work, the following maxims appear to form the basis of his theory of rheumatism:

1. Acute rheumatism has its seat in the synovial membrane of the joints; it is an inflammatory disease, characterized by the same symptoms which denote other inflammations,—namely, swelling, heat, pain, redness, ulceration, and effusion.

2. In most cases it is accompanied by pericarditis or endocarditis, as proved during life by the stethoscope, and after death by dissection.

3. The fever which accompanies acute rheumatism is strictly inflammatory, and depends partly upon the cardiac affection, and partly upon inflammation of the lining membrane of the arteries.

4. That fever, which continues sometimes after the pain has left, depends upon endocarditis.

5. The fatal termination, when such ensues, depends upon complication with pericarditis, endocarditis, pleuritis, or some other disease.

6. The duration of the disease, when treated according to Bouillaud's method, is from eight to fourteen days.

7. This method consists chiefly in withdrawing blood. On the first evening, sixteen ounces are drawn from the arm; next day, two venesections, each of six-

teen ounces, are made, and sixteen ounces more are taken by leeches and cupping-glasses. On the day following, another venesection of sixteen ounces is made, and sixteen ounces additional are taken by leeches; and, should the pain still continue, another venesection is made on the fifth day, which is generally sufficient to stop the progress of the disease.

8. If twelve pounds of blood have been taken at short intervals, enough has been done to cure acute rheumatism in from ten to fourteen days.

These views Dr. Zeroni considers as erroneous, and the practice founded upon them as most detrimental to the patient. After quoting the opinions of Galen, Sydenham, and Stoll upon the nature and treatment of acute rheumatism, he proceeds to give the following cases, as illustrative of the evil effects of Bouillaud's method.

CASE I. A young man, between twenty and thirty years of age, was admitted into the hospital, on the 19th March, 1836, on account of a moderate rheumatic fever, unaccompanied by any local affection. Some gastric symptoms being present, he was ordered an emetic, and small doses of ammonia and tartrate of antimony were prescribed. Notwithstanding these remedies, the joints of the extremities became affected; but the accompanying inflammatory swelling was by no means violent. On the 24th, he complained of oppression over the whole chest, which continued to increase in spite of the application of twelve leeches and a blister, and continued doses of tartar emetic: the pulse was quick and hard.—Some improvement was obtained on the morning of the 25th, by a bleeding of sixteen ounces and the exhibition of nitre; but, towards evening, the state of the patient became much worse: the pulse was again hard and frequent, the oppression was more concentrated upon the left side of the chest, and was attended by burning, shooting pains, which prevented the patient from assuming the horizontal position, and caused him the utmost anxiety. The local articular inflammations had increased, and the heart beat violently. V.S. of thirty-two ounces, two grains of calomel every two hours, and mustard poultices to the calves, were followed by abatement of the symptoms. The anxiety and restlessness, however, soon returned; and, between the 25th and 29th, six pounds of blood were drawn at different times, fifty leeches were applied to the præcordial region, a seton was passed in the space between the fifth and sixth rib, and calomel, digitalis, opium, and ipecacuanha administered internally. The symptoms continued to get worse, and the patient died on the 12th of April. Dissection shewed inflammation and thickening of the pericardium, and effusion of serum into its sac.

CASE II. A man, aged twenty-eight, predisposed to rheumatic attacks, but otherwise healthy, was seized with rheumatic fever, in consequence of exposure to cold and wet. He was put upon low diet, and mild diaphoretics were ordered. On the fourth day, the joints became affected, and leeches were applied to them; the diseased joints were afterwards wrapped in oilskin, and a grain of tartar emetic ordered every hour, with warm lemonade *ad libitum*. On the following day, the patient made no complaints of the previously affected joints; but some of the other articulations had been attacked, and were so much swollen as to impede motion. They were wrapped in oilskin, and the patient was largely bled. The improvement was striking: the pain was removed from all the joints, except the knees, to which twenty leeches were applied. At night the patient was quiet and composed; but, after two hours' sleep, he suddenly awoke with great anxiety, dyspnœa, pain in the præcordial region, palpitations, and irregular pulse. This state continued till morning: percussion gave a dull sound over the præcordial region, and the respiratory murmur was absent. The articulations were free from pain, the skin was moist, thirst not excessive, bowels regular, urine high coloured. The patient was again bled, two grains of calomel were ordered every two hours, and mustard poultices were applied to the wrists and ankles. These measures were followed by some relief, but towards night the symptoms again became worse, and the application of eighteen leeches and a large blister to the chest brought no relief. Next morning, the pain in the præcordial region was excessive; the dyspnœa was greater, the pulse irregular. Another venesection,

and other measures, had no beneficial result, and the patient died on the ninth day. Dissection shewed the pericardium filled with a troubled reddish fluid, and its inner surface covered with false membranes.

CASE III. A woman, aged forty, who had previously suffered from rheumatism, was attacked with the acute form of the disease. Notwithstanding repeated general bleeding, the articular affections changed their locality five times in six days; and all the joints, both of the upper and lower extremities, became in their turn the seat of acute pain, and swelling to such an extent as to prevent motion. On the seventh day, there was great diminution of all the symptoms, and the patient, for the first time since the commencement of the disease, was enabled to get some sleep. On awakening, she complained of restlessness and dyspnoea, and could not remain in the horizontal position. She soon began to complain of an acute pain in the præcordial region, which at times was so intense as to force her to cry out; there was dulness on percussion over that region, and no respiratory murmur was to be heard. The dyspnoea increased, and the beat of the heart became unequal and irregular. (V.S.; mustard poultices to the wrists and ankles; calomel and tartar emetic alternately.) These measures had no salutary effect; the patient went on from bad to worse, and died on the ninth day. On dissection, effusion was found in the pericardium, which was coated with effused lymph.

CASE IV. A young robust woman, aged twenty-two, was suddenly attacked with acute shooting pains, which extended down the back from the shoulders to the sacrum. The pain was so excessive as to forbid the slightest touch, and the fever was considerable. Before her admission on the 14th, she had been bled without relief: indeed, the pains seemed rather to become more intense, but there were no symptoms of myelitis. Venesection was twice repeated, leeches were applied, and tartar emetic, with extract of aconite, prescribed internally. Dyspnoea appeared, and the patient died on the 17th. On dissection, the dorsal muscles were found of a deep brownish-red colour, filled with blood, but otherwise normal; the dura mater of the lumbar portion of the spinal marrow had lost its transparency, and was somewhat injected, as was the whole of the spinal dura mater. The spinal marrow itself, the brain, pericardium, and pleura were healthy; the lungs were not over-filled with blood, but their tissue contained a yellowish, bloody, ichorous fluid.

Here then, says Dr. Zeroni, are the results obtained by Bouillaud's practice. It seems, according to this physician, that art must do everything, and that the powers of nature are to go for nothing; that the disease is always advancing towards a fatal termination, which must undoubtedly ensue, unless art strikes boldly in to arrest it. Pathological anatomy, and the opinion of the many, have decided on the inflammatory nature of acute rheumatism; but will experience, demands Dr. Zeroni, confirm this opinion? It must be remembered that these and the following remarks apply exclusively to that form of rheumatism which is characterized by wandering affections of the joints, accompanied by fever; those forms in which the affection remains stationary in one joint, with or without fever, or which are marked by old chronic pains, are excluded entirely.

Acute rheumatism tends to pursue a regular course, as regular almost as that of the eruptive fevers, and is thus shortly described by Dr. Zeroni:—A patient, after suffering for one, or two, or more days from fever, begins to complain of painful swelling of one or more joints, often to such an extent as to impede the motion of the affected limbs. The tongue is sometimes thickly coated, sometimes it is as white as chalk; there is great thirst and complete anorexia; the bowels are rather slow, the urine small in quantity, and troubled; the skin is moist, the pulse quick and soft. By and by other joints are attacked, generally with relief to those which had suffered first, but sometimes the disease extends to almost all the joints of the body, producing great distress and a state of mental excitement, which often passes into delirium, especially at night; which becomes insupportable, from want of sleep, pain, and profuse perspiration. At this period the chest is liable to be attacked; the patient complains of pain, especially in the cartilages of the ribs; there is excessive dyspnoea, accompanied by a short dry cough, and sometimes

palpitations. These symptoms may continue for three, four, or six days, during which the articular pains are less violent; these now again become worse, and gradually the thoracic symptoms and fever decrease. Gradually the pains in the joints disappear, the fever goes off completely, the appetite returns, and convalescence is established.

Such is the disease when left to pursue its natural course, unaffected by blood-letting, purging, narcotics, or diaphoretics; and the patient is generally reestablished in four or five weeks. But, when recourse is had to heroic remedies, if the patient escape the fate amply illustrated by the preceding cases, the disease is apt to fix itself upon one joint, and the patient becomes a martyr to the most agonizing pains, whilst the articulation runs through all the degrees of disorganization. In consequence, likewise, of neglect or unnecessary intermeddling, effusion into some of the principal cavities, or disorganization of the thoracic or abdominal viscera, may supervene; or the disease may become chronic, and last for months or years, producing in some cases a state of body difficult to be distinguished from hectic fever.

We shall now shortly give one or two cases as illustrative of the powers of nature in the cure of the disease.

CASE I. A strong robust man, who had already, at a former period, suffered from rheumatism, was again attacked in consequence of exposure to cold and wet. The articulations of the right arm and foot were affected; there was considerable fever, the tongue was slightly coated, and the bowels slow. This state continued with little change till the fifth day; the swellings in particular joints increasing, or diminishing, or occasionally shifting altogether. At night, after the pains in the articulations had somewhat diminished, the patient was suddenly attacked with acute pain in the chest and great dyspnoea, accompanied by a short, dry cough. The pulse was quick, hard, and contracted; the skin moist, the urine small in quantity. This state continued three days; the articulations then became more painful, and the thoracic symptoms were relieved. The swelling of the joints gradually disappeared, and in four weeks the patient was in the enjoyment of his previous good health.

CASE II. A strong robust woman, aged twenty-five, who had formerly had a rheumatic attack, was exposed to cold and wet, and on the day following was attacked with fever, pains in the neck and right side of chest, oppression in the chest, incapability of drawing a full breath, and short respiration. (Spirit. Mindereri ʒss. in Decoct. Alth. ʒvi.) Next day the application of a mustard poultice removed the pains from the neck, but it became more violent in the chest. The pain in the articulations of the right clavicle was almost insupportable, and the shooting pains in the chest so severe as to force the patient to cry out. Great dyspnoea and anxiety. (V.S. of four ounces, and nitre.) The bleeding produced relief, but an exacerbation soon followed. (A blister was applied.) Next day, the pains in the chest were most violent; great dyspnoea, anxiety, and debility, profuse perspirations, quick contracted pulse. (Poultices to the chest; Decoct. Alth. c. Aqua Laurocerasi.) No sleep. On the day following, the pains were less violent; the menses appeared fourteen days before the regular period, and slow but steady improvement followed.

These cases will suffice to shew Dr. Zeroni's practice: he gives other three cases, but our limits prevent us from noticing them. It appears, from the two we have quoted, that the alarming symptoms which seize the chest yield as readily, if not more so, to expectant medicine, as to the heroic practice of Bouillaud. In the second case, Dr. Zeroni was induced, by the alarming nature of the symptoms, against his better judgment, to order a small bleeding; but, in spite of the momentary improvement which succeeded, he was not long in perceiving his error, and was firm in resisting all inducement to repeat it. He regards bleeding in this disease as most hazardous practice, and affirms that most of the accidents which occur in its progress are owing far more to improper treatment than to the malignity of the disease, which, had it been left to itself, would have pursued a regular course, and ended happily. The swelling of the joints bears the same re-

lation to rheumatic fever which the eruption of small-pox bears to its fever, and it is as absurd to set about removing the swelling in the one case by heroic remedies as it would be to try and remove the eruption in the other case. Rheumatic pericarditis is, in general, he says, an artificial disease, and furnishes the anatomical school with a remarkable instance of one of its most violent and most dreaded inflammations being the consequence of powerful antiphlogistic remedies.

[Without expressing our adhesion to Dr. Zeroni's views, we certainly regard them as worthy of attentive consideration. It is, however, but justice to M. Bouillaud to observe, that the active depletory treatment was too long delayed in Dr. Z.'s cases; while we think that, if Dr. Z. had availed himself of calomel, opium, and colchicum, as administered in this country in such cases, the results would have been less lamentable in some of the first series of cases.]

Heidelberg Medicinische Annalen. Dritter Band. Erstes Heft. 1837.

On the Cause, Seat, and Nature of the Asiatic Cholera.

By Dr. BELLINGERI, of Turin.

[TOGETHER with the late Numbers of the *Bullettino delle Scienze Mediche*, we have received an appendix of Memoirs on the Cholera, that has now for more than two years been raging in Italy. After a careful perusal of these papers, we cannot find that the transalpine physicians have added much to our knowledge of the pathology of the disease, and certainly they have done nothing to improve its treatment. The first and most original article is by Bellingeri, of Turin, of which we have given an analysis below. The second is an ingenious attempt, by Dr. Pucinotti, of Florence, to prove that cholera is a contagious eruptive disease; which idea he was induced to entertain from observing that there were very few persons who did not, during the period of reaction, exhibit a peculiar eruption upon those parts of the body and limbs that had been most livid during the cold stage. The next paper is upon the cholera as it appeared in Florence, in 1835, by Dr. Petri. This physician stands almost alone in Italy as an anti-contagionist, although he believes in the infectious nature of the disease that made its first appearance in Florence immediately after the arrival of a ship from Marseilles, (at that time an infected port,) with two of the crew labouring under the disease. The persons most affected were the inmates of the Lunatic Asylum, of whom it destroyed about fifty, notwithstanding that every precaution was taken to prevent communication between the healthy and diseased. The fourth article is a Report of the Medical Committee of Turin, published by order of the Board of Health. Bellingeri was a member of this committee, and appears to have had a considerable share in the labour of drawing up the report; which opinion we have formed, not only from the similarity of style and diction in the two documents, but because that portion of the report which is devoted to the pathology of the cholera is quite in accordance with the views given by him in his own memoir, where, however, they are more fully developed, and made to apply to every form and variety of the disease.]

The occasional (exciting) cause of cholera is a principle that, like other contagions, is communicable from one individual to another, by immediate or mediate contact; inasmuch as it adheres to solid porous bodies, that serve as its vehicles; and it is also volatile, and diffusible in the atmosphere to sensible distances. The most conclusive proof of this diffusibility is the sour yeasty smell and the harsh metallic taste observed where many cholera patients are collected together, indicating the altered state of the cutaneous and pulmonary exhalations, to which the contagious quality adheres, and is thus transmitted with ease, and as it were by preference, through the medium of the respiratory organs. The miasm of cholera, however, requires for its development and epidemic propagation a certain morbid constitution of the atmosphere, which, although incapable of generating the contagious principle, favours its extension when produced, and induces a state in the human economy adapted to its reception. Hence, the most appropriate name for the disease is the *Pestilential Cholera*, as distinguishing it from the sporadic form, as well as indicating its mode of propagation. It was the immoderate rains, and

consequent unwholesome state of their food, particularly of the rice, that, debilitating the natives on the banks of the Ganges in 1817, superadded a contagious tendency to the cholera and other bilious diseases previously endemic in those regions. Like other contagious diseases, the cholera requires a certain predisposition in the individual for its reception, that experience has shewn not to be very common in Europe, even in those places where it has raged with the greatest violence.

The poison of cholera differs from all other miasmata in having a tendency, like prussic acid, to exhaust and destroy the vital principle, while the others act by irritating and stimulating it. At the invasion of the disease, the nervous system is the part primitively affected, and of this more especially the ganglionic portion, either in toto or in its principal divisions in the neck, chest, and abdomen. When the abdominal portion is affected, the disease is either confined to the digestive tube, constituting *cholérine*, or it radiates from the solar plexus to the thoracic and cervical divisions, and thence to the cerebro-spinal system, exhibiting itself in its most complete form. If the disease first lights upon the cervico-thoracic part of the ganglionic system, instantaneous death, or "ganglionic apoplexy," may result from the sudden exhaustion; or a more prolonged form of collapse, almost equally fatal, but unattended with vomiting or purging, and hence termed "dry cholera," may be induced. This, again, may transmit its influence to the abdominal ganglia, and by sympathy to the cerebro-spinal system; and thus at last the perfect form of the disease becomes established. The primary affection of the cerebro-spinal system is clearly indicated by headach, vertigo, &c., followed by loss of muscular power. The complaint, however, never stops here, but passes on, through the intercostal nerve, to every part of the body, completing the circle of diseased actions.

The most plausible explanation of these various forms of disease is to be sought in the different constitutions of the individuals, or in the mode by which the contagious miasm has penetrated the system. If it has been introduced by the saliva that has been swallowed, gastro-enteric symptoms will arise, in the form of cholérine at first, afterwards passing into the more severe species, according as the morbid matter extends its influence; if by the air respired, instant death, or "dry cholera," will arise, from the mass of the blood being poisoned at its source, and infecting every organ as it pervades it through the arteries; whereas, when the contagion insinuates itself through the skin or digestive organs, it is the veins and lymphatics that convey it, diluted and modified by their contents, back to the heart, to be still further modified and purified by the act of respiration; lastly, if the morbid matter be applied to the skin, either the digestive organs are first affected, through their sympathy with the skin, or else the cerebro-spinal system, particularly if much terror or depression of spirits be present.

The cholera is essentially and originally an affection of the ganglionic system or intercostal nerve, radiating to the heart and the whole of the sanguiferous organs, to the lungs, the digestive tube, and to the cerebro-spinal axis; and, from the debilitating nature of this affection, it may be denominated a "ganglionic paralysis."

In general, the course of the symptoms exhibits disturbance of the system in the invasion, of prostration in the cold stage, and of excitement with a tendency to inflammation during reaction. It is not true Asiatic cholera unless there is an affection of the heart and vascular system, which, in the fulminant cholera (*il colera fulminante*), with instant death, is a real paralysis of the heart. The affection of the lungs in the cold stage arises from the diminished supply of nervous influence from the intercostal nerve, by which cause the vital turgor of the lungs is destroyed, and the circulation through them is obstructed; as is proved by their state of flaccidity and congestion with black blood after death, although this latter state is partly owing to the want of power in the heart itself. The absence of the chemical action of the lungs upon the respired air, and the cause of the blueness of the skin, must be sought in the diminished or deficient influence of the pneumogastric nerve upon the lungs.

The morbid affection of the stomach and bowels is not necessary to constitute cholera; consequently, the essence of the disease cannot be seated in them. The cholera, however, is limited to these organs, beyond which it does not extend; either because, in this form of disease, there is not sufficient morbid matter to affect the whole body, or that, through an idiosyncrasy of the individual, it is not absorbed: indeed, the vomiting and diarrhoea may be considered as critical discharges of a salutary nature, serving to eliminate the material cause of the complaint. Even the discharges in real cholera, when not excessive, may have the same result. During the cold stage, the digestive tube is in a state of irritation, with a tendency to inflammation, as indicated by the burning at the stomach and desire for cold drinks, &c., and confirmed by dissection, that exhibits the stomach and bowels congested with black blood, which lays the foundation for a real inflammation during the reaction. The typhoid period follows the reaction, or immediately succeeds the cold stage, and is produced by the congestion in the arteries, as well as in the veins of the head, of the black blood that has not yet been thoroughly arterialized, and therefore is incapable of properly supporting the functions of the brain.

The following is Bellingeri's summary of his views:—The ganglionic paralysis of the intercostal nerve first induces a disturbance of the heart and vascular system, that may be either of an irritative or adynamic character: this causes venous congestion, chiefly of the brain and heart, that, if very great, produces death—if moderate, prepares the succeeding period of reaction, which latter, if mild, carries off the disease in perspiration, but, if severe, requires moderating by antiphlogistic measures. The affection of the intercostal, propagated to and diffused over the digestive tube, either in the invasion or cold stage, always produces irritation there, and, during the reaction, a real inflammation. The cerebro-spinal affection usually accompanies the Asiatic cholera, and is sometimes primary, sometimes secondary, and also sympathetic. In the invasion it is irritative; it is a state of collapse in the cold stage, and then of congestion, particularly in the typhoid period, the blood being thick and black.

Asiatic cholera, in its form and course, is analogous to intermittents in their different stages, especially to the fatal cold ague, (*perniciosa algida*), combined with the choleric form of ague. The typhoid period of cholera resembles ordinary typhus fever.

[Such are Bellingeri's views, and his treatment is in accordance with them; but, except in the use of bleeding or leeches, it is inert in the extreme, being confined to warmth externally applied, and diaphoretics and aromatics in the cold stage, and diluents during reaction.]

Appendice al Bullettino delle Scienze Mediche di Bologna. Fasc. i. 1836.

CASES OF SEVERE LESION OF THE BRAIN.

[THE following cases are very interesting. They sufficiently prove how little we know of the precise functions of the encephalon, and what an extent of observation and comparison of facts are necessary before we can pronounce, with any thing like certainty, on the subject.]

1. *Case of Fatal Disorganization of the Brain, without corresponding Derangement of the Intellectual and Moral Acts.* By G. W. BOERSTLER, of Lancaster, Ohio.

In August, 1833, I was called to see William Miller, a lad about eleven years old: he had just received a kick from a newly shod horse, which fractured the right superior portion of the os frontis, and the anterior portion of the right parietal bone. During the operation of removing the fractured bones, I found one portion, an inch and a half long, of an irregular triangular form, driven into the right anterior lobe of the cerebrum, to the depth of an inch: on removing it, about a table-spoonful of brain was discharged. The piece of bone, having its edges serrated, and being driven from before backwards, necessarily produced a very great

laceration of the meninges. The common integuments over the fracture were much contused and lacerated, and sloughed in the course of a few days, leaving exposed a very considerable portion of the skull and brain. I moulded to the convexity of the cranium wet pasteboards, and then saturated them with albumen, which, when dry, gave them considerable firmness: these I confined with the double-headed roller. I looked upon these precautionary measures as important; for I feared *hernia cerebri*: four days gave reality to those fears; *hernia* came on, but, after six days' perseverance, I succeeded in preventing any further protrusion. There was no compression, save by the fractured pieces, which were readily removed. The boy's faculties were not destroyed, but there was some intellectual confusion, from the time of the injury, during the operation, and for two hours after; from which time he recovered every faculty of the mind, and they continued vigorous for six weeks, and to within one hour of his death, which took place on the forty-third day. During all this period there was little apparent derangement in any of the organs, except a slight irritative fever, which supervened sixteen days after the injury, and continued to the termination of the case. So slight was this fever, that, in despite of all entreaties, the patient sat up every day, and frequently walked to the window and withdrew the curtain, in order to see the boys play in the streets, in which he took deep interest, frequently laughing at their gambols. Four hours after death I proceeded to the examination, in the presence of Drs. Edwards, Ohr, and Newcomer. Upon removing the cranium, the *dura mater* presented strong marks of inflammation over the entire arch of the head, being deeply injected in parts, and having depositions of coagulable lymph in others. From the antero-inferior angle of the right parietal bone, in a line back to its junction with the occipital, the *dura mater* was disorganized in three points by ulceration. The space of the skull, previously occupied by the right anterior and middle lobes of the cerebrum, presented a perfect cavity, the hollow of which was filled with some sero-purulent matter, the lobes having been destroyed by suppuration: the third lobe was much disorganized. The left hemisphere was in a state of *ramollissement* down to the *corpus callosum*. It was so much softened that the slightest touch would remove portions; and, with the aid of a sponge, I wiped away its substance to near the *corpus callosum*, when it began to be firmer, but presented more the appearance of a homogeneous mass than of regular organization. The chiasm of the optic nerves, as well as their entire tract, was so soft as to yield to a slight touch with the handle of the scalpel, and the olfactory were in the same condition. The *corpus callosum*, *thalami nervorum opticorum*, and *tubercula quadrigemina* presented no pathological condition. The cerebellum and *medulla oblongata* were in a physiological state. The spinal column was not examined. This boy was remarkably intelligent. In my daily visits I held frequent conversations with him, and in all my observations I could not discover the slightest derangement of his intellectual faculties; no dulness of sensibility, no obtuseness of perception, no impairment of judgment, no want of memory, and, so far as mind is concerned, he gave no evidence of disease. His vision, audition, and voice were unimpaired.

We have here a case which presents that portion of the brain from which the nerves arise, in a physiological condition, and the general nervous apparatus in a sound state, fit for conveying impressions; whilst the organ upon which depends perception and the perfection of ideas is in a great degree lost, and what remains is in a highly pathological condition; yet we have all the manifestations of intellect, as if the *encephalon* were not required in those highest functions. His case contradicts the opinion of Sir Charles Bell, that disease of the general surface of the brain is always attended with derangement of the mind; and it is equally opposed to the views of Desmoulins, Gall, Spurzheim, and others, who contend that the seat of intellect is in the periphery of the brain or its convolutions. In like manner, the opinion of Magendie is contradicted, that the sense of sight is always destroyed by removal of the cerebral hemispheres; for here the right hemisphere was destroyed, and yet vision was perfect with either eye. Where, I would ask, were the functions of mind executed in this case? Intellection was performed,—

the moral faculties were exercised,—and that portion of the brain in which we believe those important and complicated actions are generated and perfected was either gone or in a highly pathological state.

2. *Case of Abscess of the Cerebellum, communicated to the Académie de Médecine, the 27th Sept. 1836.* By M. BOUVIER.

A man, fifteen years of age, had been subject for a length of time to a discharge from the ear, with deafness and frequent headach. He was suddenly seized with more severe headach in the left side of the head, vomiting, and disorder of mind. His symptoms were indeed so characteristic that a physician, who was consulted, pronounced him to be labouring under abscess in the head, and that death was almost certain.

He entered the Hôtel Dieu on the 15th of September, three weeks after the last exacerbation, when he complained of fixed pain in the head, which frequently caused him to cry out; sensibility in other respects obtuse; slow answers; somnolency; face pale, features sunken; look sad and anxious; a copious purulent discharge from the left ear; deafness of the same side; pulse slightly slower; vomiting; constipation; the movements of the limbs were preserved; an incomplete paralysis of the upper eyelid being alone observed.

These symptoms continued for the following days without any marked aggravation; and it seemed probable that the patient's life might still be prolonged for some time, when, on the 23d of September, after vomiting, accompanied by great agitation and violent outery, he suddenly fell into a state of complete collapse. Respiration became embarrassed, and he died eight days after his entrance into the hospital, with symptoms of asphyxia.

On examining the body, there was found, as had been foretold during life, a caries of the petrous portion of the temporal bone, and an abscess in the interior of the cranium. But, what was remarkable, the abscess occupied the left hemisphere of the cerebellum, although nothing led to the suspicion that there was any lesion of that organ. There was an extensive cavity, which invaded the two outer thirds of the left lobe of the cerebellum, and which contained several table-spoonfuls of pus, somewhat similar to that of an abscess. The substance forming its parietes was softened, and of a livid tint. The meatus auditorius was filled with reddish vegetations.

The caries occupied the base of the pars petrosa only; the labyrinth and auditory nerve were untouched. There was no perceptible communication between the internal abscess and the abscess of the caries. The disease of the bone, however, extended to the dura mater, in two very circumscribed points, at the upper and hind part of the pars petrosa. The dura mater opposite these points was deeply coloured; and the coloration extended to its inner surface, where it was in contact with the cerebellum.

The cerebral ventricles were, moreover, greatly distended by a limpid fluid, and the pia mater exhibited a decided injection under the anterior part of the cerebral lobes, chiefly on the left side.

"Two circumstances," says M. Bouvier, "give interest to this case. The first is the almost entire separation, by means of the dura mater, (which was scarcely affected,) between two lesions, one of which must have been the effect of the other; so that it is difficult to explain, merely by continuity of tissue, the transmission of the affection from the ear to the cerebellum.

"The second is the absence of all the symptoms which have been of late regarded as an effect of lesions of the cerebellum; such as augmentation of the general sensibility, loss of equilibrium, and excitation of the genital organs. Could this peculiarity be owing to the slow development of the affection, or to its not having extended sufficiently far from the side of the medulla oblongata?"

American Medical Intelligencer. April 1, 1837.

On the Efficacy of Sugar of Lead in Cases of Fever with Ulcers of the Intestines.
By Professor NASSE, of Bonn.

[THE treatment recommended in the following article has recently acquired a fresh interest from the recommendation of the same remedy in the Asiatic cholera.]

A fever has prevailed for some time at Bonn, attended by a peculiar appearance of the tongue; pain in the abdomen, especially on the right side, upon pressure; nervous symptoms, and a particular state of the pulse: it is also accompanied by diarrhœa, and, on examining those who had died of it, the termination of the small and the commencement of the large intestines have been found in a degenerated state. During the inflammatory stage of the disease, Dr. Nasse applied cupping-glasses to the abdomen, and in most cases more than once; in a few cases, when the pulse was hard, he bled from the arm; and in every case applied blisters to the abdomen. As soon as the diarrhœa made its appearance, sugar of lead was prescribed, generally without being combined with opium, and was continued until the fœces reassumed their natural appearance, and until the abdomen ceased to be pained by pressure. Dr. L. Jung, in his work *De Dothi-enteritide ejusque plumbo acetico sanandi ratione*, had already described the admirable operation of this medicine. Dr. Nasse gives a similar testimony to its value, and states that, in his own practice, it has, in his opinion, saved several lives, which would have been lost without it. Nineteen cases of the above fever were treated with sugar of lead; five in children, and fourteen in adults. Of these eighteen recovered. The lead was not administered till the fifth, eighth, or tenth day of the complaint, or even later, when cerebral congestion rendered necessary the application of leeches. The dose was one-fourth, one-third, and half a grain, from three to six times daily. Generally, two grains were sufficient to put a stop to the diarrhœa; but, in five cases, four grains were necessary. One of the patients, who had forty stools in twenty-four hours, and who suffered from tenesmus, took in all eight grains, at first without, and afterwards combined with opium, and at last with starch clysters. The young man, who died, took altogether ten grains: in his case, the diarrhœa yielded to it in a great degree, but the abdominal irritability and the tympanites were such as to induce its fatal termination. As soon as further doses of lead seemed unnecessary, a weak infusion of ipecacuanha was given. In some of the cases which recovered there had been petechiæ; and in some, too, the evacuations had been bloody; but the lead had been administered all the same. Opium was not generally ordered, on account of its effect upon the head. In eight cases, on account of weakness and fainting fits, the liq. ammon. carb. was had recourse to; in one, that of a child, quinine was given before the lead, but, the fever increasing, and blood passing off with the stools, the latter was prescribed, and with the best effect.

Med. Zeitung v. Ver. für Heilkunde in Pr. 1836. No. 23.

Moral Management of the Insane in America.

[THE following extracts from the Report of the Maclean Asylum, in Boston, for the year 1836, place in a striking light the benefits to be derived from a rational treatment of insanity. Both the treatment and the results correspond, in a striking manner, with those recorded in Dr. Macrobin's recent Report of the Aberdeen Asylum.]

The number that enjoyed the advantages of the institution during the year was one hundred and eighty-three, of whom one hundred and twelve were discharged on the 1st of January last. Of these, sixty-four had recovered; seven were convalescent: two much improved; five improved; nine not improved; fifteen were sent away by order of committee; ten died. Of the nine discharged, "not improved," five were hopeless cases of masturbation at the time of their admission; two were idiotic, and two had insufficient trial. Of the deaths, one was of phthisis pulmonalis; one of old age; one of mania with convulsions; one of dysentery; one of suicide; two of marasmus; one of fracture of the neck of the thigh bone; and two of acute inflammation of the mucous membrane of the bowels.

"Our amusements are various and numerous. We keep a carriage, two carioles,

one chaise and four horses, which are devoted almost exclusively to the use of the patients. Many of them ride every fair day, and have, the last year, ridden ten thousand miles. The males are also engaged at bowls, quoits, bass-ball, fishing, fancy painting, walking, dancing, reading, swinging, and throwing the ring, &c. Of the one hundred and three male patients who have been in the institution during the year, seventy have been engaged in out-door amusements, passing, in this way, three thousand five hundred and forty-one hours. Seventy-seven have walked ten thousand four hundred and thirty-one miles. Some have walked, individually, over one hundred and fifty miles per month. Twenty-four have occupied one hundred and nineteen hours in fishing.

"In our 'Labour Department' the patients have been equally active and interested. Seventy-seven of the males have engaged in manual labour, and have worked, allowing six hours per day, (more than which no patient has been asked to work,) one thousand seven hundred and ninety-eight days.

"Gardening, the cultivation of flowers, and farming, as usual, have occupied and interested many of the patients during the whole season. The tastes and wishes of each individual have been, in all cases, consulted as far as possible; and while some were engaged with the team, others would be equally ambitious to excel in planting, hoeing, or in displaying their taste in the arrangement of the flower-beds and borders. Thus their irritability was expended in healthy exercise and occupation, and instead of meeting them in the halls in tattered garments with oaths and imprecations, we are greeted in the walks with the affectionate grasp of friends, their countenances glowing with pleasure and contentment, and each commenting, in his own way, upon the business of the day.

"Nor has our labour resulted in mere amusement, as the harvest of our crops abundantly testifies. Our farm and lands, inclusive of all the grounds occupied by the buildings and courts, consist of twenty-five acres. We have raised, for the most part, vegetables enough of every kind to supply the institution for the year, and have cut hay sufficient to keep five horses and six cows, besides storing eighty barrels of apples and fifty bushels of pears. We have also made rose-water enough for medicinal and culinary purposes, and disposed of fifteen dollars' worth. The net profits of our farm and garden, for the past year, have been 500 dollars.

"In April we opened the dome of the male wing as a carpenter's shop for the patients, having previously secured the services of a judicious carpenter to superintend and work with them; and, although we were confident of success, our hopes have been more than realized. Not the least accident has occurred, although the patient's have not been restricted in the use of tools, and herein, as I conceive, our safety lies. The patients feeling themselves under no restriction, consider that they are placed upon their honour, and their self-respect being called into action, they would not forfeit the confidence and good opinion of the officers for any consideration. Give a man constant employment, treat him with uniform kindness and respect, and, however insane he may be, very little need be feared from him, either of mischief or violence.

"Fifty patients have worked in the shop, at six hours per day, and have been employed eleven hundred and fifty-one days; and made seven thousand two hundred and thirty-six boxes, which have been sold for 907.06 dollars.

"In cases of masturbation, we depend entirely on labour for restoration. During a residence of ten years in this asylum, I have never known a single case of masturbation to be cured, unless the patient engaged in regular labour. This is a very large and most unfortunate class of the insane. We seldom receive a case of this kind in its incipient stage. It is so insidious in its approach, that before the friends of the patient are aware of his situation, he is past recovery. Labour promises the only relief. More improvement in this class has been evinced the past year, than in all the others together, and work alone has effected it.

"The results in the female wing have been equally interesting. Fifty patients have been received. Of this number have recovered, 30. Convalescent, 8. Much improved, 5. Improved, 3. Died, 4. Total, 50.

"The Belknap Sewing Society continues its operation, and affords agreeable

occupation and diversion for its members. They continue their regular weekly meetings, which are held in the oval room of the mansion-house, or in one of the halls of the wing. In the absence of the presiding officer, the meeting is organized by choosing, on nomination, by a vote of a majority, one of the members to act as president *pro tem.*, whose duty it is to oversee the work and read some interesting story selected for the occasion. Their employment is piecing and quilting bed-coverings, and making and mending garments and furniture for the Institution and the patients. After the labours of the day are over, tea is passed round, and then the meeting adjourns. The account of each day's proceedings is recorded in the Society's book. It is sixteen months since the Society was organized, and the avails of their work have been, in cash, 112.96 dollars.

"In all our amusements and recreations, it is our intention to blend utility with labour or diversion. Thus, when we walk or ride, some object of interest is sought to visit; and in this respect the advantages of the locality of the institution are pre-eminent. It stands in the midst of the most interesting portion of New England, isolated from the noise and throng of business, but in full view of the capital and its beautiful environs. In these excursions the patients have uniformly conducted themselves with perfect propriety.

"Following out this plan, (of the combination of labour with utility and pleasure,) the Belknap Sewing Society is professedly and operatively benevolent. They furnish clothing for any of their members who may be needy, and sometimes purchase for themselves articles of taste and fancy; and they seek out and assist the afflicted and destitute of the neighbourhood. The poor widow, whose husband was killed in a sudden and shocking manner, last summer, by the railroad engine, was visited, and mourning was provided for herself and daughter at the expense of the Society. They called a special meeting, and deputed a member to purchase the articles necessary; and, with their accustomed promptness, made them with their own hands. I mention this, not as an act of charity worth naming, but as exemplifying the fruits of a system of moral management which is pursued, and to show that our patients are not excluded from society, and that there is scope enough for useful occupation even here. The making of the dresses for this widow and her daughter, for the time, engaged the united interest and attention of all. Diseased manifestations were quieted in the universal feeling of sympathy for that afflicted family. This being over, something else would be found to excite a similar interest, and a succession of objects, to engage their attention and to call into exercise the better feelings of their nature, has helped to do away, little by little, diseased impressions, and bring about, with many, the healthy and natural operations of the mind and body.

"Our social meetings, for diversion and recreation, continue to exert a benign influence on the convalescent. The weekly dances are continued with unabated interest, and the deportment of the patients, without a single exception, has been respectful and appropriate. Fifty-four of the males and fifty-two of the females have attended on these occasions. A sure guarantee against all improprieties is found in the constant attendance of both nurses and officers, who take an active part in the amusements. We assemble at an early hour of the evening, and the recreations consist in alternate dances and marches, with occasional songs, accompanied by the piano. At eight o'clock refreshments are served, and at nine the party 'breaks up.' For two or three days before the party, the females are engaged in preparing their dresses for the occasion, and for some days after they have a fund for remark in the events of the evening. The males also are found practising the figures of the dance, and perfecting themselves in the marches, during the week. The females have, besides, meetings every afternoon during the winter season, at which they read one hour, and pass another in practising upon the piano and in the exercise of dancing.

"Our religious meetings and exercises have been continued, and with all the success which the trial of last year led us to anticipate. Seventy-nine of the males and sixty-six females have attended family prayers. Not the least disturbance has been witnessed; but a great degree of solemnity, suited to the occasion, has uni-

versally been maintained, and the patients of both departments, with a few exceptions, depend as much upon being present at this exercise as upon their daily meals. The attendance at prayers is altogether a matter of choice.

"Our females, the past year, have ridden some thousands of miles, walked in the country 1,159 miles, walked in the garden 150 hours, folded and ironed clothes 1,025 hours, and assisted in domestic concerns 1,025 hours.

"In addition to the work before stated as having been done by the males, they have sawed, split, and piled all the wood for the whole establishment, viz. 200 cords; and have carted 100 cords from the wharf to the house. Work promises much; and it has been the aim of the Institution, the past year, to keep every patient employed in labour as far as possible. One patient has braided and sewed 100 palm-leaf hats."

American Medical Intelligencer. May 1, 1837.

On the Treatment of Delirium Tremens by Digitalis and Opium.

I. On the Infusion of Digitalis in Delirium Tremens. By Dr. MAGNUS HUSS, of Stockholm.

THE infusion, prepared with a drachm of the leaves to a pint of water, was administered in six cases, all of them men of strong constitution, of from twenty-four to thirty-three years of age. Two of the cases required bloodletting. In three, a table-spoonful of the infusion was administered every hour throughout the day only; in the other, a like quantity was exhibited every hour both day and night. In the former number, the disease yielded, and sleep ensued on the third day; in the latter number, sleep ensued in thirty-six hours; an equal quantity of the infusion was thus required in both cases. The patients awoke after a sleep of from six to ten hours, free from the disease, but labouring more or less under the effects of the medicine. In one patient the pulse had sunk to thirty-five beats, but in the other the rhythm was normal; in the whole number the pupils were contracted, and they complained of dryness of the mouth, burning in the throat, humming in the ears, heaviness in the head, great weakness, and nausea; which last symptom was so severe in one patient that for two days he vomited whatever he took.

Jahrbücher der Gesammten Med. B. xv. H. 1.

II. On the proper Dose of Opium in Delirium Tremens. By Dr. WEISSE.

A case of delirium tremens, in which one grain of opium, taken every hour up to fourteen doses, failed in effecting a cure, but removed the delirium. The friends became alarmed by observing that the patient squinted incessantly, saw double, and that the face became horribly distorted. These symptoms were attributed by Dr. Weisse to the opium producing too little effect; he accordingly ordered two-grain doses every two hours. After the second dose the patient slept soundly, and awoke quite well. Dr. W. observes, that when a patient labouring under delirium tremens is under the influence of opium, and his mind, instead of picturing to him men and devils, begins to busy itself with *insects*, he is about to fall asleep.

Zeitschrift f. d. Gesammte Med. B. v. H. 2. Hamburg, June, 1837.

New Mode of treating Empyema. By M. REYBARD.

M. REYBARD cites three cases of empyema. In the first case the patient, sixteen years of age, fell upon the side, which produced pleurisy, and which was followed by an effusion. The second, forty years of age, received, in a state of drunkenness, a blow from a knife on the anterior and superior part of the chest, from which resulted an effusion of blood. The third, aged seventeen years, had a spontaneous pleurisy of the right side: this was followed by an effusion, which, instead of being absorbed, collected in so great abundance that the heart was displaced. These three patients were operated on, and cured: the first in fifteen or twenty days, the second in thirty or thirty-five days, the third in four months. Besides the operation, the second had to sustain the action of detersive injections into the

pleural cavity. The third case was operated on twice; the first time in the intercostal space; but the effusion being reproduced, M. R. pierced a rib, and fixed a canula in it furnished with a valve. It is to this practice he attributes, in a great degree, the success he has obtained in the treatment of empyema.

This instrument consists of a tube of silver, furnished at the end with a piece of catgut, four or five inches long. That this catgut may perform the office of a valve, it should be held in warm water until the sides are softened. A plaster of diachylum, fastened near the external extremity of the tube, fixes the latter to the walls of the chest, and at the same time that it prevents the external air from penetrating into the cavity by the wound, it unites exactly the lips of the wound to the tube.

M. R. leaves the canula constantly in the wound, convinced of the double necessity of evacuating the effused fluid, and of leaving the chest open to prevent a new collection.

Bulletin de l'Académie Royale de Médecine. April, 1837.

Case of Dyscrasy, treated with Sugar. By Professor DUNGLISON.

WHEN sugar is added to venous blood out of the body, it immediately communicates a florid hue to it, in the same manner as many salts, which are presumed to have an alterative effect when administered internally. Some change is produced by all those alterative agencies, so that when the modified blood attains the capillaries it induces a new action in them, and breaks in upon the pathological catenation that constitutes the cachexia. When the capillary functions are morbidly affected, as in chronic cutaneous eruptions or ulcerations, there are two great methods in which we may reach the disease: the one is by the application of external remedies to the diseased capillaries; the other is by changing the impression made upon them internally by the fluid that circulates within them. Sugar (like arsenic, creosote, iodine, and other alteratives,) acts in the latter way.

The success which we have met with in the removal of inveterate eruptions from the administration of sugar, in the manner to be mentioned presently, has induced us to infer that the different alterative syrups, officinal and empirical, may be mainly, if not entirely, indebted for their efficacy to the sugar they contain.

Early in last November, a lady, from a distant city, came to Philadelphia, to place herself under the care of a distinguished professional friend, who advised her to consult the editor. Four years ago, her husband, who was a dissolute character, had contracted syphilis, and communicated it to her. She had applied to several physicians, but without experiencing entire relief. Ulcerations existed on various parts of the body; she had nodes on the tibia, &c.; added to which, she had been unable to sleep, in consequence of severe pains in the bones, for the eighteen months prior to her arrival in Philadelphia. In order that full opportunity might be afforded for recovery, she took a house, had her furniture sent to her, and determined to reside during the winter in this city. She had already taken various forms of mercurials, and amongst the rest had persisted, for a length of time, in the use of the solution of corrosive sublimate. The solution was, however, directed again, so that she should take one-sixteenth part of a grain of the bichloride three times a day; and, in addition to this, she was ordered to dissolve a pound of rock candy in a pint of water, and to take a wine-glassful of this solution four times a day. Under this course, the ulcers healed; the nodes disappeared; the osteocopi ceased; her nights were passed in comfort, and at the end of five weeks she was so well, that she determined to rejoin her family, quitted her house, removed her furniture, and went away in the middle of December with feelings of perfect recovery. She was recommended, however, to persevere in the plan advised. Since then we have not heard from her.

American Medical Intelligencer. April 15, 1837.

Curious Case of Absorption of Bone. By JOHN H. MARABLE, M.D.,
of Tennessee.

THE following singular case of absorption and reproduction of bone occurred in the person of a negro, who is now about ten years old. When about three years of age, a softness or absorption of the skull occurred in several places, say five or six. These would occasionally close, and be observed in other parts; and these changes have continued until within the last twelve months: at this time the nose, with all its bones, one of the maxillary bones, and those of one eye, are entirely absorbed and soft, so that the pulsations of the brain can be as distinctly felt as those of the wrists. His intellect is somewhat impaired, but not until lately. The eyeball of the side affected is protruded, or rather appears so, from the absorption of the bone. All these appearances I have examined within the last day or two. He has a great appetite, sleeps much, and can walk about pretty well. There is no doubt that at this moment more than half the bones of his skull and face are wholly absorbed. His condition is attributable to no known cause; he is of usual size, and the rest of his body (the head being a little too large,) is of the usual form and proportions. It is a rare case, and no relief is expected; but the master of the boy, as well as myself, would like to know if such a case has ever come under the care of Dr. McClellan; and, further, his opinion relative to it.

American Medical Intelligencer. April 15, 1837.

SURGERY.

On the Treatment of Erysipelas by raw Cotton. By M. REYNAUD.

CONSIDERABLE experience appears to have established the utility of cotton in the treatment of burns. So convinced is the author of its value, that he employs it in all cases, in whatever degree they may exist: for he has found it to calm the pain instantaneously both in superficial burns of the skin and in those which occupy the whole thickness of the cutis, as well as to act beneficially in the subsequent process of the cure. Being struck with the efficacy of cotton in burns, and noticing the analogy which there exists between the inflammation of the skin produced by an incandescent body and that of erysipelas, M. Reynaud employed cotton in the latter disease, and the constantly beneficial results which followed its use led him to permit their publication.

The local treatment of erysipelas is, as M. Reynaud observes, very various, and based on no fixed principles. Some of the means employed give rise to considerable pain, others are, on various accounts, objectionable. In their place, M. Reynaud would substitute cotton, as a substance soft to the touch, of a moderate price, always at hand, easy to apply and unattended with pain in its application, and the action of which is so certain that the surgeon may at will extinguish or again excite the inflammation, by continuing or discontinuing the application. In erysipelas, as in burns, the cotton calms pain as it were by a charm; a mild and moist warmth takes the place of the itching, the formication, the sharp and biting heat which so much increase the pain; the swelling gradually diminishes, the redness disappears, the skin becomes flaccid and wrinkled, and without becoming covered with those furfuraceous scales, which characterize the termination of erysipelas, and which sometimes continue during a long period. All that separates are a few slight layers of epidermis, and this is speedily effected. The general excitement ceases with the local phenomena, the fever diminishes, and in simple cases the organic functions return to their normal state, without the necessity of any other treatment. A circumstance of great advantage in the use of cotton is, that it is equally fitted for, and produces analogous effects, in all forms of erysipelas, whether idiopathic or traumatic, whatever may be its situation, on the face, body, or limbs; whatever may be the depth of the tissues which are affected; for in the cases which are collected, are some of phlegmonous erysipelas greatly amended and others entirely arrested by

the simple application of cotton. The result, M. Reynaud would thus explain,—that the cotton acts by exciting in the diseased part a moderate warmth, a sort of vapour bath which keeps up a constantly equable temperature, a proper degree of humidity, by keeping the diseased part from contact with air and light, two powerful excitants of the cutaneous system. Cotton does not suffice in all cases; no more than other remedies does it enable the surgeon to dispense with general means, but it lends to these a great assistance: it hastens the resolution, and when this termination cannot take place, it still serves to limit the inflammation, and to arrest its progress. The method of applying the cotton is very simple. Raw cotton which is well carded must be selected, in order that it may be free from all foreign substances which it sometimes contains. A layer sufficiently thick to protect the diseased part from the light and air must then be applied, taking care always that the cotton extends some inches beyond the limits of the inflammation. A compress and a few turns of a bandage will keep the cotton applied. A linen mask is well fitted for the face. The cotton should be removed every twenty-four hours to judge of its effects, or, if there is no contra-indication, it may be allowed to remain during the whole course of the treatment. If the cotton should adhere too strongly to the skin, in a case where there is slight exudation, it may be removed by applying over it an emollient poultice. The author has added to the previous remarks, several cases of various forms of erysipelas treated with cotton; to which it is unnecessary to call the attention, otherwise than to state that seven are cases of simple erysipelas of the face and extremities; that one is of traumatic erysipelas; the ninth of erysipelas complicated with a miliary eruption; four of phlegmonous and one of gangrenous erysipelas. If other surgeons meet with the same success which appears to have attended M Reynaud in the use of his remedy, they will have great reason to thank him for its recommendation.

Journal des Connaissances Medico-Chirurgicales. Fevrier, 1837.

Rhinoplastic Operation. By J. MASON WARREN, M.D., Boston.

[THE following operation is extremely creditable to the judgment and skill of Dr. Warren; and although it cannot now be considered as novel, its perusal cannot fail to be both interesting and instructive to the young surgeon. It is never useless to exhibit the triumphs of surgery to its votaries.]

J. T., twenty-eight years of age. Three years ago last spring, he received a violent blow on the nose, which dislocated the cartilage, driving it at the same time over to the left side. Some inflammation came on in the nose at the time of the accident, which very shortly subsided; and as he was out of town, and at a distance from medical advice, nothing was done to replace the cartilage, which remained in the situation into which it had been driven by the blow. In the following spring, a small red spot appeared on the right cheek just below the eye; this very soon increased in size, the inflammation gradually spread, first attacking the lip, and from thence extending to the nose, which became red, swollen, and finally ulcerated; and in the course of eighteen months the whole nose, cartilages, septum, bones, &c., were successively attacked, and finally completely destroyed. The ulceration had also extended to the cheek of the opposite side. Subsequently to this, cicatrization gradually took place, leaving the patient in the state in which I saw him, six months after his recovery from the disease.

At this period, the nose had entirely disappeared, leaving in the place it originally occupied an opening about an inch in diameter, bordered by a firm cicatrix; the septum of the nostrils was destroyed, and the two nasal cavities thus thrown into one; externally a small cicatrix descended from the lower and left edge of this opening to the angle of the mouth. In the course of the disease the four front teeth had been lost, and this, together with the absorption of the alveolar processes, had caused a sinking of the upper lip, which had fallen an inch below the level of the lower one. An opening also existed between the lip and upper jaw, through which a probe might be passed from the mouth into the nasal cavity. The sense of smell was quite lost, and he was subject to an occasional running of the tears over the face, arising from the too sudden contact of the air with the lachrymal ducts.

His case was certainly a hard one. A young man, in the prime of life, in other respects of a good face and appearance, was, by this frightful calamity, not only entirely cut off from society, but prevented from gaining the means of subsistence. Having determined to submit himself to an operation, it was thought expedient to delay it a few weeks, in order to watch the case a little, and prepare him for it by a course of diet and regimen. At the end of six weeks his health had materially improved, and the operation was performed on the 7th of September. A piece of pasteboard, cut in the shape of the letter V, with a projection from its base, corresponding to the columna of the nose, was placed upon the forehead, and a trace made around it with the nitrate of silver. A trace was also made around the opening of the nasal fossa, at the points where it would be necessary to remove the integuments for planting the new skin taken from the forehead. This was done the night previous, in order to prevent any undue delay on the day of the operation. The head being firmly supported by two assistants, the incision was commenced between the eyebrows, and the flap of skin dissected up so as entirely to isolate it from the skin of the forehead, except where, for the purpose of nutrition, it was left adherent at the root of the nose. The incision on the left side between the eyebrows was extended a little farther down than on the right, the better to facilitate the twisting of the flap. This incision included the skin, subcutaneous cellular tissue, and a portion of the occipito-frontalis muscle, care being taken not to raise the periosteum, from fear of necrosis. The flap thus dissected and twisted round to the left side, was carefully wrapped in a compress of linen cloth, and, before the operation was proceeded farther in, attention was given to diminishing the large wound made in the scalp. Little hæmorrhage had taken place, and the temporal arteries, which had been cut, very soon retracted and ceased bleeding. The angles of the wound were first brought together by the twisted suture, two pins being employed on either side. Its edges between the eyebrows were also approximated in a similar manner; by this means the wound in the forehead was diminished at once to less than half its original size; it was still farther reduced by the use of a few strips of adhesive plaster, and a little scraped lint filled up the remainder of the wound. Some lint spread with cerate was spread over the whole surface, a pledget, and the whole secured by a bandage round the head.

The next object was to fix the borrowed skin in its place. For this purpose a short narrow knife, somewhat similar to a cataract knife, was used, and a strip of integument, a third of an inch in breadth, removed, including all that portion which had been at all indurated during the cicatrization of the ulcerations. The knife was also passed between the lip and upper jaw, in which existed, as before stated, an opening large enough to pass a probe, and the adhesions between the two, for the space of an inch, entirely cut away. This was done for the double purpose of giving the columna of the nose a more deep and firm adhesion, and, in the inflammation which would subsequently ensue, to close up the unnatural communication between the mouth and nasal cavity. The flap was now brought down into its place, its angles a little rounded with the scissors, the better to simulate the alæ of the nose, and the whole secured in its place by pins and points of the interrupted suture. From that portion of the skin which was to form the columna of the nose, the epidermic side was pared a little, so that it might form an adhesion not only underneath to the jaw, but on its sides to the quadrangular wound made for it in the upper lip. A little scraped lint was now placed under the ends of the pins, and a strip of oiled lint introduced into each nostril to prevent adhesion; another strip was placed upon the nose to preserve its temperature. The dressings were secured by a band of adhesive plaster fixed to the forehead above, and partially divided in the middle, so that it might descend on each side of the nose to the lip.

During the whole of this long and painful operation the patient kept up his courage, and not a cry was uttered, nor the least struggle made that could at all impede the motions of the operator. Not much blood was lost, and his strength was so little exhausted that he was able to run up stairs to his chamber. He was ordered to go to bed immediately, to keep perfectly quiet, and a watcher left with him, who had directions, in case of his falling asleep, to prevent him from either rolling over on his side, or raising his hand to the nose so as to derange the dressings; also to wake

him immediately should he breathe through the nose. To have arrow-root or gruel and lemonade, for nourishment.

September 10th. Passed a good night, slept well. A piece of cork was confined between the teeth, so as to keep the mouth open, it being hoped that this might prevent him from closing his lips during sleep and breathing through the nose.

11th. Quite as well. The introduction of the cork into the mouth had entirely effected its object, by preventing the passage of air through the nose.

12th. The first dressing took place. The dressings on the forehead, after being well soaked, were first removed. The angles of the wound were found to have united throughout, so that two of the pins were at once dispensed with. Union had also taken place in its lower part, just above and between the eyebrows; the remainder of the wound, that is, its central part, in which union by the first intention could not take place, was suppurating well, and filled with healthy granulations. The nose was next attended to. Upon the lint being removed, which had become very much hardened by the coagulated blood, it was found that entire union had taken place on both sides. The alæ of the nose and lower edges could not easily be seen without making use of too much violence in removing the dressings, which at present was not thought necessary. The columna was curved inwards, and the sutures concealed. The nose was of the natural colour and temperature, and the circulation through it seemed uninterrupted. Two strips of lint dipped in oil were laid over the cicatrix on each side of the nose, and no other dressings used. The patient was allowed to sit up a little, and to take any liquid food he might fancy.

One of the pins was removed from the forehead on the 13th, and another, the only remaining one, on the following day. The dossils of lint which had been placed in the nostrils still remained there, firmly caked in by the drying of the pus, blood, &c. These were not removed until the 19th, when their places were supplied by two pieces of hollow sound.

15th. The remaining pins were removed from the side of the nose, and the two sutures which confined the alæ; and on the 17th, ten days after the operation, the two ligatures, which confined the columna in its place, were also removed. At this period, the following was the state of the parts. The wound in the forehead, from the adhesion by the first intention which had taken place, and subsequent contraction had diminished to a third its original size, and the small triangular space which remained, together with that portion of the scalp from which the columna of the nose had been taken, was filled with healthy granulations. From the wound to the root of the nose was a lineal cicatrix two inches in length, and continuous with the cicatrix on the left side. Adhesion of the integuments had taken place on both sides of the nose; at the right alæ, however, the union was not quite so perfect as at the left; that is to say, the whole thickness of the skin did not appear to have united. To assist the union, the skin of the face which lay under it was slightly scarified with the point of a lancet. The columna of the nose was a little curved backward, and its edges had retracted inwards upon themselves. The inside of the nose was suppurating well, and at its upper part adhesion seemed to have taken place between the two bleeding surfaces which had been opposed to each other. The tip of the nose was well defined, and its edges were curved inwards so as well to simulate the natural appearance of the alæ; and just above the alæ, apparently from atmospheric pressure, a depression was taking place, forming their superior boundary. This was assisted by the patient making an occasional pressure with his fingers at these points. He feels well, has a good appetite, and sits up all day. He breathes freely through the tubes placed in the nostrils, which require to be daily removed in order to clear out any obstructions which may collect in them.

At the end of a month the wound in the forehead had contracted to about a quarter of its original size. Adhesion of the nose was perfect at all its points. The openings of the nostrils were regularly rounded, and simulated well the natural appearance. At the end of six weeks he was able to go out and walk about. At the end of two months it was thought time to proceed to the second operation, which was required to remove the twist existing at the root of the nose. The method usually adopted by operators has been to cut the pedicle, after sufficient union of the nose has taken place below to justify the separation of it from its source

of nutrition, and to fix it down at the root of the nose, in a transverse incision made for it at that point. To this method there are some serious objections. First, the danger of inflammation in separating the pedicle; second, of sloughing of the organ on the vessels being cut which have hitherto supplied it with blood; and lastly, the very perceptible transverse cicatrix left after the operation. The method resorted to in the present case is liable to none of these objections, except, perhaps, the first one, in which the danger is much diminished. An incision was made, commencing at the internal angle of the eye, and extending to that part of the base of the nose where adhesion had not been able to take place; a corresponding incision was also practised on the pedicle. The skin being well dissected up from its adhesion, a small portion of integument was removed from the upper angle of the wound, where it had become wrinkled from the twist in the pedicle. The edges were brought together by three points of the interrupted suture. The same operation was to be performed at a future day on the other side, where, however, the opening was of about half the size, and not so perceptible. Union took place, throughout, by the first intention. Some trouble was experienced, however, by the formation of a small abscess in the new cicatrix, which suppurated and discharged itself.

Four months after the operation the cicatrization had become complete at all points. The cicatrix in the forehead has become very small, and is gradually assuming the colour of the surrounding integuments. The scalp from which the columna was taken is lost in the hair. The nose is quite firm, of a good form, and the cicatrix on each side hardly perceptible; at the root of the nose on the left side, and at that portion which formed the pedicle, a small fissure still remains, which is for the present concealed by a strip of court plaster. The health of the patient has never been better, his sense of smell is returning, and the tears no longer run over the face, and he, as well as his friends, congratulate themselves both on the moral and physical effects of the operation. He is now able to make his appearance during the daytime, which he has not done before during the last two years, and no person would observe anything remarkable in the nose, without a minute examination, when it would be difficult to explain the remarkable anatomical changes which have taken place.

Boston Medical and Surgical Journal. March 8, 1837.

On the Causes which retard or prevent the Consolidation of Fractures.

By M. LOUIS FLEURY.

[THE following observations on the treatment of fracture are the more interesting, from the lively discussions that have recently taken place in this country on the subject. The French author takes no notice of these.]

M. F. remarks that, in order to ensure a perfect consolidation of a fracture, two indications must be fulfilled: 1st. Placing the broken extremities in contact. 2d. Keeping them in this position. The former is done with facility, but the latter, notwithstanding the means resorted to, presents some difficulties. In the present day, most surgeons think that complete immobility of a fractured limb, joined to strong pressure on the soft parts, are the best means of maintaining the fragments in position, and of obtaining a quick and regular consolidation. Unfortunately, these means are frequently unsuccessful; and, notwithstanding the care used in their application, the callus is frequently thrown out in an irregular manner, or perhaps never formed at all. What, then, are the causes which prevent or retard the consolidation of a fracture? Authors have enumerated a great number, but seem to have overlooked a very important one. Scrofulous and venereal affections, —old age, —rents in the periosteum, —formations of pus, —cold applications, —all, undoubtedly, exercise a prejudicial influence. But, by far the most frequent of all the causes, is the apparatus used with the view of favouring the consolidation, which it prevents by the compression it exercises upon the vessels of the limb; whether this compression is inevitable, as in the immoveable apparatus, or produced voluntarily by the surgeon.

If a fractured thigh be placed in a thick layer of soft materials, the effects of the

compression cannot be very appreciable; for, in this case, although the capillary circulation and the small arterial branches are more or less restricted, still the large vessels continue free. The same does not occur in the forearm or leg, where compression, ever so slight, interrupts the course of the blood, not only in the superficial vessels, but also in those which supply the fractured bone and periosteum. In order to obtain a rapid and regular consolidation, we must be careful not to apply more splints than are absolutely necessary, and not to bind these too tight by means of bandages. In following an opposite method, we wait sometimes, three, four, or six months for an union which has not yet commenced. It is then that the surgeon, eager at each dressing to reapply the apparatus with more care, that is to say, to augment the number of splints, surround the limb more exactly, &c., finds himself deceived; and the more he renews his efforts, by the same means, the greater is the distance separating him from his object.

The following case, one of four reported, exemplifies the beneficial results of this practice.

CASE. C. D., aged forty-one years, had a fall on the 15th of February, 1836, broke his right leg, and entered the same day the Hospital of St. Louis. The fracture was complete, situated immediately above the internal malleolus, and complicated with a deep excoriation and extensive ecchymosis. The wound was dressed with cerate, the rest of the leg covered with charpie dipped in the white of egg, and the ordinary apparatus for fractures of the leg immediately applied. The member was maintained in absolute repose during six weeks. On the 10th of April, the apparatus was removed, the wound was found cicatrized; the ecchymosis had disappeared, but the consolidation had not commenced. The apparatus was reapplied, and a more generous diet ordered. On the 30th of April, the consolidation was a little more advanced. The splints and anterior cushions were then removed, and the limb sprinkled with spirits of camphor. From this period the callus rapidly solidified; by the end of the month of May, it was very resistant, almost inappreciable to the touch, and the patient quitted the hospital.

Archives Générales de Médecine. August, 1837.

THE PROGRESS OF LITHOTRITY.

I. *On the existence of Lithotritry amongst the Arabians during the twelfth and thirteenth Centuries.* By C. F. MARTINS, M.D.

It appears from several passages in various Arabian writers, that, during the twelfth and thirteenth centuries, they were acquainted to a certain extent with lithotritry. In a work by the celebrated Arabian physician Azzarahvi (vulgarly designated Albucasis, who died at Cordova in 1107,) entitled "*Liber Theorices necnon Practices*," there is the following passage: "Accipiatur instrumentum, subtile quod nominant mashafarebia et suaviter intromittatur in virgam, nunc volve lapidem in media vesica, et si fuerit mollis frangitur."

In an Arabian work, entitled "*The Flower of Thoughts on precious Stones*," by Chehab-Eddin-ahmed-Ben-Joussouf-Teifaschy, we have the following observation:—

"One precious advantage of the diamond which Aristotle has spoken of, and which experience confirms, is its employment in calculous affections; when an individual is affected with stone, whether in the bladder or in the urethra, we take a small diamond, and fix it to the extremity of a small stalk of metal, either of copper or silver, and introduce it into the organ containing the stone, which can be pounded by frequent rubbing."

Ahmed-Ben-Abikhled, a physician known by the name of Ebn-al-Harrar, relates, in a work which he has written on calculous disorders, that he employed these means upon a domestic, who suffered from an urinary calculus of a very large size. "This man," says he, "would not submit to the operation of lithotomy; I therefore employed the other means indicated. I pounded the stone by rubbing and reduced it to a size sufficiently small to pass out with the urine."

Revue Médicale. July, 1837.

II. *Improved Lithotritic Instrument.* By M. CIVIALE.

The instrument which M. Civiale submitted to the Academy of Sciences differs from those which he had hitherto made use of, in the formation of the curved portion. Some difficulty was always experienced in seizing and fixing small stones and fragments of stones, and the operation often caused much fatigue to the patient. M. Civiale has obviated these inconveniences by increasing the surface of the curved part, and diminishing the quantity of material of which it was formed. In the old instrument, the size of the female branch was three lines, and that of the male two lines. In the new instrument the female branch is five lines and a half and the male four, so that the surface by which the calculus is seized is nearly doubled. The instrument is not too much weakened by loss of material, the urethra suffers no injury from increase of size, the bladder is less apt to be pinched, and the stone is less liable to escape. These advantages have been ascertained by experiment, so that we may safely regard the new instrument of M. Civiale as an improvement in the art of lithotomy.

La Presse Médicale. Tome i. 1837, p. 479.

III. *Lithotrity in a Child, aged fourteen Months.* By M. SEGALAS.

This child was aged fourteen months, and during seven months had manifested symptoms of stone; among others, frequent desire to micturate, violent pain, and particularly whilst making water, almost constant diarrhœa, and prolapsus of the rectum every time that the urine was evacuated. After having detected a calculus, M. Segalas introduced the "brise-pierre," without any other preparation than having dilated the urethra by a wax bougie, allowed to remain for a few minutes in the urethra. A calculus of a diameter of ten lines was seized, and broken by pressure and percussion combined. The calculus, which was composed of oxalate of lime, was entirely destroyed during four sittings. Neither of these was followed by any accident, and during their intervals, the child continued to go to school, to run and play as usual. He subsequently became extremely healthy.

Bull. de l'Acad. Roy. de Med. July, 1837.

On the Separation of the Epiphyses of Bones. By M. ROUX DE BRIGNOLLE.

THE possibility of the separation of the epiphyses of bones after the first few years of life, has been doubted by many eminent surgeons. M. Roux relates a number of cases which prove that these accidents may occur as late as the age of eighteen, and that the largest as well as the smallest bones are liable to them. He relates an instance of separation of the condyles of the femur in a boy of eleven years; of the inferior extremity of the radius, the styloid process of the ulnar, the head of the first phalanx of the thumb, and of the fore and middle fingers of the left hand, in a boy of eighteen years of age; of the humerus in a boy of twelve years; of the lower extremity of both radii in a boy of seventeen; of the head of the humerus in a boy of fifteen; and of the lower extremity of the radius in a man of twenty-two years of age. When the separated portions of bone are not replaced, inflammation, suppuration, intense pain, ardent fever, extensive and deep-seated abscesses, and sometimes even gangrene occur, especially when the larger bones are injured. When the displacement takes place in articulations of moderate size, the separation of the portions of bone is often inconsiderable; but union in that position causes deformity, and impedes and weakens the movements of the joint. In the smallest articulations, the displacement is never great, but, if the accident is neglected, motion is almost impossible, and the deformity very great. The union of the separated portions of bone is very rapid, whether they are restored to their proper place or not.

M. Roux gives the following directions for the treatment of these cases. The separated portions of bone are to be replaced as soon as possible, and retained in their situation by the same means which are resorted to in case of fracture; the treatment, however, does not last so long in separation of the epiphyses. If dis-

placement of the fragments produces pressure on the surrounding parts, and threatens tetanus, paralysis, or gangrene, and if the reduction cannot be effected in the usual way, deep incisions must be made, and the separated portions of bone be either replaced or removed. If the shaft of the bone project through the skin, the opening must be enlarged by incision, or the extremity of the bone removed. M. Roux admits internal as well as external causes of displacement, and terminates his letter by vindicating his claim to originality against M. Guérin.

La Presse Médicale. No. 55. July 12, 1837.

On the Effects of Pus in Contact with Osseous Tissue.

By M. MASLIEURAT LAGÉMARD.

THE case here reported was treated in the hospital of the Faculty at Paris, by M. Jules Cloquet. A woman, aged forty-two, was the subject of an abscess in the right axilla; the consequence of which was the formation of fistulous passages, extending upwards towards the clavicle. Whilst under treatment for this affection, she was suddenly attacked with acute inflammation of the left lung, which terminated fatally on the fourth day.

On inspection, a considerable part of the left lung was found infiltrated with pus, whilst the right lung was perfectly healthy and crepitant throughout. Beneath the anterior portion of the great pectoral muscle was discovered a large cavity, extending from the clavicle to the fifth rib, *not* communicating with the fistulous openings, and filled with well-formed pus. The part of the clavicle which was exposed to the action of the pus was denuded of periosteum, and presented a true ulceration with sharp edges, as if a portion of the bone had been removed with a chisel. The first, second, and fourth ribs presented a similar lesion; and in the third rib there was a complete solution of continuity to the extent of seven lines. The corresponding portions of the intercostal muscles were completely destroyed, and the pleura laid bare. This membrane was opaque and excessively thickened, and adhered closely to the opposite portion of the pleura pulmonalis. The adjacent pulmonary tissue was quite healthy.

The reporter supposes that this purulent deposit may have communicated in the first instance with the fistulous openings, and that, this communication being accidentally stopped, the absorption of the different tissues was the result of the consequent retention of the pus. It is worthy of remark, that there were strong adhesions between the pleura pulmonalis and the denuded portion of the pleura costalis, which was not the case elsewhere; so that, had perforation taken place, the pus would have escaped into the bronchi, and a cure might thus have been effected.

We are not acquainted with the conditions which influence the action of pus in the tissues with which it is in contact; nor can we explain why, in this case, the presence of the same fluid should have caused absorption of bone, and condensation of a serous membrane and of cellular tissue. The pus appeared to be well formed, and communication with the external air was prevented. We must therefore look for the specific cause in the peculiar constitution of the individual, and our ignorance on this point should induce us to attend most carefully to the practice, now so generally inculcated, of giving issue to purulent matter as soon as its existence is well ascertained.

Archives générales de Médecine. Mars, 1837.

On the external Use of Calomel in Inflammation of the Eyes.

By DR. FRICKE, of Hamburg.

THE author calls attention to a remedy, now fallen much into disuse, in cases of ophthalmia. He has employed it extensively, and the success with which its application in his practice has been attended makes him anxious to render it better known generally. The first cases which he selected for its use were of a chronic character, and such as had been subjected, without advantage, to other treatment; e.g. rheumatic, catarrhal, and scrofulous inflammations, attended with the various

changes which frequently accompany them. Subsequently he employed the remedy in other and milder forms of inflammation; the result of his observations being, that calomel, employed in this manner, is of the greatest utility. In cases of rheumatic inflammation of the eyes, in which, notwithstanding the most rational treatment, local and general means, derivants, narcotics, antirheumatics, &c., the redness, pain, and intolerance of light have continued, the calomel, in the hands of the author, has been attended, in a couple of days, or even after a single application, with an amelioration, or even a disappearance, of all these symptoms. Instead of the great intolerance of light, the eye becomes capable of bearing its stimulus; the burning hot tears become milder and less abundant; the sensation of pain in the eye gradually diminishes, and the patient is soon cured.

The same pleasing results follow the use of calomel in strumous ophthalmia. In the management of that most distressing symptom of this disease, intolerance of light, the author is not aware of one case in which the calomel was applied to the eye, where its application was not followed by success.

In other inflammations of the eyes, in which a general plan of treatment had failed, and in which the calomel was employed experimentally, to observe its action, two only are known to Dr. Fricke in which any injurious effects were produced.

The mode of employing the calomel is as follows:—Dip a miniature pencil into calomel washed in alcohol, and apply it immediately to the eyeball. This application should be repeated once or oftener in the twenty-four hours. The sensations produced in the eye are very different: in a healthy eye, in general, scarcely any sensation is produced, or at most a transient and slight warmth; in inflamed eyes, pain is produced, but it is generally inconsiderable, and only complained of in certain cases. The pain disappears wholly in from half an hour to two hours. One patient felt no pain, but said that the calomel produced a feeling of coldness. Others, to whom Dr. Fricke has communicated the results of his own experience with the use of calomel, have found them in their own practice confirmed.

Zeitschrift für die gesammte Medicin. No. 7. July, 1837.

Account of a new Cupping-glass. By M. LAFARGUE.

THE cupping-glass of M. Lafargue he recommends as obviating the objections which arise from the use of flame, and the expense which attends those to which an exhausting pump is attached: it consists of a glass funnel, and it is exhausted by suction. A small leathern valve at the extremity of the funnel prevents the return of air after it has been withdrawn by the mouth; and so great is the force which may be employed by this simple apparatus, that, “in having recourse to it,” says the author, “I produce constantly ecchymosis, and even an exudation of blood on that part of the skin to which I apply it.”

When M. Lafargue desires to obtain an energetic revulsion, he takes a disk of cotton or of old linen, holds it before the fire, and, when it has become very hot, he applies it to that part of the skin on which he wishes to operate, places his cupping-glass over, and exhausts it. At the end of two minutes he recomences the operation, which he repeats four or five times successively. It is after the application of cupping instruments that M. Lafargue thinks his instrument is of special advantage. He has no doubt that, by the use of his apparatus, as much blood may be obtained as by the use of leeches.

[This is the aboriginal instrument of the cow's horn improved by modern science. We doubt not but it is very efficacious.]

Gazette Médicale. No. 25. Juin, 1836.

Curious Case of a Silver Teaspoon swallowed during a Paroxysm of Madness.

By Dr. OTTO, of Copenhagen.

THE subject of this case endeavoured in various ways to put an end to his life. After attempting it by fourteen days' starvation, and finding that this did not

answer his purpose, he swallowed a silver spoon, thinking that this would ensure his destruction. This was on the 21st July, 1835. He was subject to such treatment, as would, as much as possible, prevent the occurrence of inflammation. A constant pain was complained of in the scrobiculus cordis, which pain had commenced after swallowing the spoon, and compelled the individual to lean forwards constantly. The pain continued without relief; palliatives only being given from the conviction that its cause was the presence of the spoon. All his insanity had ceased. In April, 1836, he remarked a small swelling in the præcordia, which broke, and discharged a clear fluid. July 23rd, he observed something dark coloured in this swelling, which was the handle of the spoon. The spoon was withdrawn; the pain ceased, and the ulcer from which the spoon escaped soon cicatrized. *Zeitschrift für die gesammte Medicin.* No. 7. July, 1837.

Ossification of the Vitreous Humour. By Professor ANT. GRILLO.

This case formed the subject of a paper read to the Acad. Med. Chir. of Naples, in March, 1837.

A young sailor, whose father was gouty, began to suffer from gout at the age of twenty-seven, attended with disease of the urinary organs, and the passage of much phosphate of lime in his water. His health was improved by proper diet and the use of diuretics; but, about two years afterwards, his sight grew weak, and, with slight ophthalmia, he suffered severe pain in the eyeballs and forehead; his urine being clear, and he being free from gout. An attempt was made to restore the gout to its former seat, but in vain, and he died of apoplexy.

The gouty parts were found enlarged, and loaded with phosphate of lime; as were also the kidneys, which were hypertrophied. The membranes of the brain were inflamed, and the eyeballs were smaller than usual, hard and swelled on each side, with a total loss of transparency. The sclerotic was hard, but could be separated from the parts beneath; but the iris had contracted adhesions to the cornea and to the lens, which was diminished in volume and opaque. The black matter of the choroid, which was confounded with the internal surface of the sclerotic, was alone distinguishable; all the internal part of the eye being occupied by a hard osseous body, in which were confused together the vitreous humour, the hyaloid, the retina, and the membrana Jacobiana.

This case, rare as it is, is not quite unparalleled; for, in August last, Dr. Casilli met with a similar morbid appearance in the eyes of a phthisical subject, and the specimen was laid before the Academy by Dr. Grillo, in confirmation of his own case.

Filiatre Sebazio. April, 1837.

Extraction of a foreign Body from the Female Bladder, by artificial Dilatation of the Urethra. By M. THOMAS.

A LADY, aged thirty-four years, was seized, without any apparent cause, with retention of urine. Her husband, thinking to relieve her, introduced an ivory ear-picker into the urethra, with immediate relief; but, the quantity of urine appearing exceedingly small, he reintroduced the same instrument more deeply. It, however, slipped from his fingers, and fell into the bladder. From that moment symptoms peculiar to stone in the bladder manifested themselves. M. T. was called in six hours after. With a sound he felt the foreign body, and thought to extract it by a small pair of polypus forceps; but failed. The patient suffered much, and passed some blood. An operation was proposed, and objected to. A sponge tent was then introduced into the urethra, and confined by ligatures. In the course of two hours it was swollen considerably by the absorption of the urine; this was then removed, and replaced by a larger. Two hours after, the index finger was passed into the bladder, and the foreign body was felt, placed transversely to its neck: the position being rectified, it was immediately expelled. The urine flowed involuntarily for six hours, but from that time it was under the influence of the will, and the patient recovered.

La Lancette Française. March, 1837.

MIDWIFERY.

On Extra-uterine Pregnancy. By J. E. DEZEIMERIS, M.D.

RESPECTING the seat of this abnormal conception, ten varieties are enumerated:—1st. Ovarian pregnancy. 2d. Sub-peritoneo-pelvic. 3d. Tubo-ovarian. 4th. Tubar. 5th. Tubo-abdominal. 6th. Tubo-utero-Interstitial. 7th. Utero-Interstitial. 8th. Utero-tubar. 9th. Utero-tubo-abdominal. 10th. Abdominal; primitive, secondary. M. Dezeimeris raises, *in limine*, the question of the possibility or impossibility of ovarian pregnancy. MM. Prevost and Dumas have proved the important office of the spermatic animalculæ in the act of fecundation, but not that their actual introduction into the ovule is necessary to vivification; nor yet that these animalculæ, imprisoned within the cavity of the womb, wait there for the descent of an ovule. On the contrary, tubal pregnancies are incontestible and uncontested; and, as it is not to be supposed that the fœtus developed in the tube had been fecundated in the womb, and had thence reascended to its narrow abode, so must its vivification in the fallopian tube be admitted by all. This first step being gained, the succeeding will be made with comparative facility. Even those who deny the possibility of impregnation taking place while the ovule is yet enveloped by the investment of the ovaries, must yet admit some vivifying influence that induces the ovule to burst its boundary walls and descend through the tube. Of the second variety are cited two cases where a fœtus was found between the layers of the broad ligaments, one of them dissected by Professor Lobstein. Among all the cases upon record of utero-Interstitial pregnancy, the most valuable for the authenticity of its details is that communicated by Dance to M. Breschet. It is evident in this case that the pregnancy was really and purely interstitial, that is, without the participation of the fallopian tube in the wall of the cyst containing the fœtus; the tube, in traversing the uterus, was connected with the wall of the cyst, and presented in one point an opening, which was indeed taken for a rupture made in detaching the placenta, but whose existence more probably preceded the passage of the fœtus into the substance of the uterus. This circumstance of the case conducts us by insensible gradations from the tubo-utero-Interstitial pregnancy to the simple interstitial, whose locality is more distant from the tube, and very much diminishes the mystery with which some have invested this state of parts.

Primitive abdominal pregnancy is interesting only as shewing upon what parts the fœtus is ingrafted, and how it supplies itself with the necessary quantity of blood. Secondary abdominal pregnancy involves the discussion, whether, as M. Guillemot states, some cases of normal gestation may not thus terminate, in consequence of rupture of the walls of the uterus: in either case, if the immediate danger be surmounted, the mother may sustain her unborn offspring for an unlimited period in this new condition.

Pathological Anatomy of the Extra-uterine Fœtation. In these cases the embryo generally retains its proper membranes, viz. the chorion and amnios, and also the placenta, if it has survived the first days of its existence: the placenta is larger than natural, thin, furnished with very small vessels; circumstances induced by the difficulty of obtaining an adequate supply of blood from the neighbouring organs. In the primitive abdominal pregnancy, there is rarely any enveloping cyst that can be considered analogous to the caducus; owing, no doubt, to the trifling inflammation produced in the first instance by the presence, in the cavity of the peritoneum, of so small a body. In the secondary form this cyst is always found, being indeed as necessary a consequence in this case as its absence was to be anticipated in the last: for, whether the fœtus be disengaged into the abdominal cavity, by rupture of its envelope, as a tubal, ovarian, or other gestation, or by rupture of the uterine walls, the presence of so large a foreign body could not but excite inflammation, the glueing together of the neighbouring organs, and thus at last a perfect cyst. As to the fœtus itself, 1, a remarkable development of the osseous system has been observed in some instances, as well as the presence of several teeth; 2, a putrid state of the fœtus, the bones of which made their exit from the body by different

routes; 3, a dessicated or mummified condition; and, 4, its transformation into a chalky mass, into amazone, or into bone. Examples of monsters in these situations are rare.

Of the Mother. The normal change in the size and vascularity of the uterus, its gradual diminution and return to the condition of vacuity, as well as the formation of the membrana decidua, are attested by cases. The secretion of milk and the menstruation obey the usual laws of natural gestation: most uneasiness to the mother is occasioned by tubal and utero-interstitial foetation: this condition offers no material obstacle to natural gestation and delivery. At the expiration of the ordinary period, childbed pains supervene, and last some days, and are often renewed at pretty regular intervals as long as the pregnancy continues.

Journal des Connaissances Med. Chir. Jann. 1837.

Unconscious Delivery. By M. LEONHARD.

THE following case is related chiefly as of importance in a medico-legal point of view, in its bearings on the following questions: 1. Whether a female may entirely mistake the nearness of delivery? 2. Whether she places herself unintentionally in a place unfitted for delivery? 3. Whether, whilst the pregnant woman is fully possessed of consciousness, a delivery, of which she is unconscious, may take place?

Mrs. K—, aged thirty-seven, the mother of six living children, and having thrice miscarried, became pregnant for the tenth time, and calculated the term of her pregnancy during the first fortnight of May. Early in March she was exposed to the contagion of small-pox, and acquired the disease in a very violent form. A laxative clyster had been ordered on account of constipation of three days' continuance; but it was not administered, as, during the afternoon, the patient complained of a disposition to evacuate the bowels. She was raised upon the night-stool, and became suddenly and at once freed from this constipation. She remained, however, for a quarter of an hour in the same situation, because she continued to feel some desire to evacuate feces, of which but a small quantity was passed. As she began to feel very faint, she was returning to bed, when, greatly to her astonishment, she found herself connected by a band with the night-stool, which she could not separate. This was discovered by an attendant to be connected with a child, which began to cry on being withdrawn from the bloody water. The female, who had given birth previously to six living children, could scarcely trust her eyes. *She had been entirely unconscious of what had preceded delivery, and had throughout perceived no indications of it.* The child was separated from the navel string, and the woman returned to bed. She died suddenly in about half an hour afterwards. The uterus had contracted; the secundines were loose in the vagina; that portion of umbilical cord which remained attached to it about an ell long. The child was a female, and appeared fully formed. From these observations, made in the case of a sensible and honest woman, and in the presence of several witnesses, the questions given in the first part of this paper may be answered thus: that a pregnant female, even though she has frequently borne children previously, may entirely mistake the approach of delivery,—may be unintentionally surprised in a situation and in circumstances the most inappropriate for it,—and may, even though quite conscious, be unconscious of the birth of her offspring. The importance of this case in some questions respecting illegitimate pregnancy, is evident to all.

[We cannot believe in the full consciousness of this patient during the passage of the foetus: the case is, however, both curious and important.]

Medicinische Zeitung. No. 24. 1837.

Speculum Uteri made of Glass.

DR. HACKER, of Leipzig, uses a glass speculum in examining the vagina and womb. It consists of a simple oval cylinder, the glass of which is about two lines

in thickness, its length being from four to six inches. The diameter of the end which is introduced into the vagina is half or three-quarters of an inch by a quarter or half an inch. This extremity is furnished with a blunt, rounded, and tumid border, whilst the cylinder at the other end is funnel shaped. Its form corresponds essentially with that of other specula, except that its circumference is less. The advantages of these specula of glass are their cheapness, and the facility with which they can be cleansed. When, too, they are introduced into the vagina, its mucous membrane, immediately before the tumid extremity of the speculum, is pressed into the cavity of the speculum, where it can well be examined. If the material be transparent glass, by means of a bright artificial light, the parts of the vagina which are covered by the speculum may be examined. Specula of dark glass are better fitted for examining the mouth of the womb.

Jahrbücher der in-und-ausländischen Medicin. Heft 3. 1837.

Instances of Menstruation occurring in Old Age.

1. MADAME K. had ceased to menstruate in her forty-second year, and, with the exception of some attacks of gout, had continued well to her eightieth year. At this time (1832), attended with pains like colic, the menses returned, continuing, as formerly, for about four days, and regularly recurring until August, 1835. From that time they disappeared. The individual died in the beginning of 1836. According to her own statement, the secretion which escaped was, in every respect, of the same sensible characters as that of her earlier life.

Med. Zeitung. No. 48. 1836.

2. A female, who has never had children, menstruated regularly until her fiftieth year: from this period, onwards, there appeared every three months, at the time of the previous menstruation, an excretion of from ten to fifteen drops of blood. The woman is now seventy-four years of age, is very healthy, and continues to menstruate in the same manner.

Jahrbücher der in-und-ausländischen Medicin. Heft 1. 1837.

Rupture of the Cæcum during Labour. By Dr. STAMPF, of Stargard.

THE wife of an officer, at the commencement of her first pregnancy, was much distressed with vomiting, but it went off at the end of a week, and she suffered no remarkable inconvenience during the remainder of the gestation, excepting in the eighth month, when venesection was necessary to relieve a determination of blood to the head. The labour went through its stages in the usual way, and the child was born; but both the mother and husband of the patient declared they noticed a peculiar noise in the abdomen at the time of the last labour-pain, which was a very strong one. The first twenty-four hours after the birth passed away without any particular occurrence beyond sharp after-pains and a slight uneasiness in the groin. From this period, fever, with a hard, full pulse, frequent nausea and vomiting, considerable thirst, severe pains in the abdomen upon the slightest touch, unconquerable costiveness, and, in short, all the symptoms of inflammation, made their appearance, more referrible to the intestinal canal than the uterus and peritoneum. Frequent bleedings, &c. &c. were resorted to in vain; the patient died seventy-two hours after delivery.

Upon dissection, it was ascertained that the cæcum was ruptured in a diagonal direction, to the extent of nearly two inches, and that half a wash-hand basin of fæces had escaped from the opening into the abdominal cavity. The parts in the vicinity of the rupture were of normal character and free from inflammation, excepting on some parts of the intestine and peritoneum in the right inguinal region; but nowhere was there any evidence of mortification. The cavity of the abdomen and the intestines were much distended with flatus.

Berliner Medicinische Zeitung. 20 April, 1836.

Vagitus Uterinus before the Rupture of the Membranes.

By Dr. C. F. DRESSER.

As Dr. D. was sitting one evening with his wife, aged thirty-one, who was pregnant for the third time, he heard two evident cries from the child in the mother's womb. At the same moment his wife looked at him alarmed, placed her hand upon her right side, and declared that the cry, which she also had heard, had proceeded from that spot. The doctor immediately applied his hand to the part, and felt the child move. In the moment of the cry the child had moved violently, and had caused a perceptible pressure on the right side of the scrobiculus cordis. The mother had been healthy throughout the whole pregnancy; there were no signs of delivery, which first commenced forty-eight hours afterwards, and ended by the birth of a fine girl. This case Dr. D. relates as a new instance of the possibility of the crying of a child in the womb before the evacuation of the waters.

Jahrbücher der in-und-ausländischen Medicin. Heft 1. 1837.*On Injections into the Vessels of the Umbilical Cord, in Cases of retained Placenta.* By Dr. DE BERGHES.

[This paper may be regarded as a sequel to one on the same subject in a former Number of this Journal, Vol. III., p. 244, to our remarks on which we beg to refer.]

The author has had several opportunities of witnessing the advantageous effects of injections of cold water into the veins of the navel-string, where the placenta is slow in being delivered. He relates four cases which fell under his own immediate notice. In all, the females had passed their fortieth year; they were strong, although lean; and in all of them the mammæ were but little developed. The first case is that of a woman who had borne eight children, and where, three hours after delivery, the placenta was still retained. The after-pains were few and feeble, the abdomen tumid, the discharge of blood trifling, and the os uteri sufficiently patent. By cutting off a small portion of the navel-string, it was easy to find an umbilical vessel, and to introduce into it the point of a common clyster syringe, with which a pint of cold water was injected. A sensation of chilliness and an increase of the after-pains were produced. A ligature was then applied to the navel-string, and the hand introduced into the womb, where the placenta was found everywhere adherent. The volume of the placenta, in spite of the injected water, was not large; and the fact of its being injected, and with cold water, very much facilitated its delivery.—The second case was somewhat similar, except that the placenta was but partially adherent.

In the third case, where the injection was made about two hours after the birth of the child, and in the fourth half an hour after the child was born, the placenta followed about eight minutes after the injections had been made, and without any assistance. In all the females the subsequent process was undisturbed, and the author is not aware of any injurious effects resulting from the treatment.

Wochenschrift für die gesammte Heilkunde. No. 86. 1837.

HYGIÈNE.

Diseases of Cutlers. By A. CHEVALLIER.

As M. Chevallier continues to publish monographs on the diseases of artisans, we shall persevere in giving a condensed notice of them. The present article, although it necessarily contains less important matter than the previous ones, on the diseases of printers and lead manufacturers, yet is compiled with similar pains-taking and industry. The grinders of cutlery are exposed to accidents from the breaking of their grinding stones, which may be owing, 1, to the centrifugal force; 2, to the dilatation of the wooden wedges by which the iron axle-tree is fixed into the stones; 3, to the mill-stone being badly mounted, so that it is liable to be dismounted and break. The workmen may also wound themselves with the sharp instruments they

are grinding, and when the mills are put in motion by mechanical means the apparatus may injure them. M. Chevallier proves the truth of these statements by cases from various treatises on medical police. The information which he has himself collected was furnished by M. G. Chevallier, a large manufacturer, and by his own observations in a journey to Nogent, (Haute-Marne,) in the neighbourhood of which place 3,300 men are employed in the manufactory of cutlery. In this district ten severe accidents have occurred from the breaking of millstones; the injuries were chiefly confined to the head and face, wounding the cheeks, jaws, and lips, and breaking the teeth. At the Royal Manufactory of Arms at Klingenthal, where sabres are sharpened and cuirasses polished, seven or eight large mills have broken in five years and two workmen been injured. The wounds which the men sometimes receive when sharpening instruments are owing either to awkwardness, and this is very rare, or to grease on the stone causing the instrument to slip rapidly over the mill-stone, and thus to deceive the workman. Two accidents took place during twenty years at Nogent, from the workmen's clothes becoming entangled in the machinery. The precautions which are taken to avoid accidents are the following. The mill-stones are carefully examined before they are purchased, and if there are cracks they are rejected. Some workmen try the stones, by turning them very rapidly before they use them; at the manufactory of arms at Klingenthal this precaution is always taken, the stones being revolved as rapidly as possible for three or four hours; during the last five years four or five mill-stones broke during this trial. In some of the larger workshops the grinding stones are guarded by a semicircular hoop of iron fixed by its two extremities to the stone trough, and an iron bar fixed at one end to the centre of this hoop, and at the other to the end of the trough. Should the stone break, this apparatus is calculated to prevent the fragments from striking the head and face of the workmen, which parts are most frequently injured in these accidents. Instead also of fixing the iron axle-tree into the stone by means of wooden wedges, they are fastened by a nut and screw guarded by leather soaked in oil.

The workmen are subject to varicose veins, and ulcers of the legs; the mill when used dry is dangerous to the grinders, and badly ventilated and imperfectly lighted workshops warmed with coal are injurious to the health. The sight of some is weakened by not using lamps throwing the light from above, and shades, so that they are obliged to give up their trade at an age when other workmen who have taken precautions can still follow it. Apprentices when placed too young at the vice ("étai") often become deformed. The workmen in the neighbourhood of Nogent (amounting to 3,300) are more sober than they were thirty or forty years since, and it does not appear that their trade shortens their lives. They often die from imprudence, and from carelessness regarding the preservation of their health, but they appear to live to an old age, for those who will not take precautions with their eyesight, are sometimes obliged to *relinquish* their trade (which is a profitable one,) at fifty-five or sixty years of age.

Advice to Master Cutlers. In order to preserve the health of their men they should, 1, have their mills mounted and guarded so as to prevent accidents; 2, prevent grease from soiling the mills; 3, surround those parts of the machinery which might injure the men, with gratings of wood, or iron, or wire; 4, guard against great rapidity of motion in the mills; 5, examine the mill-stones previously to mounting them; 6, prevent the apprentices from placing themselves in such positions as produce deformity; 7, ventilate the shops, and place them in dry situations.

Advice to the Workmen. 1. To use sky-light lamps, shades, and even preservative glasses in time to prevent weakness of sight. 2. To wear laced stockings if they have varices. 3. To live soberly. 4. To guard against placing themselves in bad positions. 5. To avoid exposing themselves to cold when hot, and to wear wooden shoes when they work in low and moist places. 6. To use moderate exercise on their days of rest, instead of frequenting beer-shops (cabarets). 7. To economize, and place the savings in saving-banks. 8. To establish benefit clubs as a provision for illness and old age. It is much to be desired that boys should

not be apprenticed at too early an age before they are sufficiently strong to bear the fatigue, otherwise it enervates them, and makes them prematurely old.

Annales d'Hygiène Publique et de Médecine Légale. Avril, 1836.

MEDICAL STATISTICS.

On the Prevalence and Mortality of Cholera in Venice, during the Years 1835, 1836.

[THIS paper is drawn up from authentic documents, and the object of it is to prove, by statistical returns, that the cholera is not a formidable disease to all, but only to certain classes, whom he urges to apply for relief at the hospitals, instead of trusting to treatment at home. We give a summary of the results.]

TABLE I.—*Summary of 286 Cases of Cholera, occurring in a Population of 9554 Persons in three adjoining Parishes, from Oct. 20, 1835, till July, 1836.*

Cases	Males	Females	At home.		In hospital.		Class.					Class not given
			Died	Cured	Died	Cured	i.	ii.	iii.	iv.	v.	
286	150	136	122	58	62	43	60	55	25	55	14	90.

The number of patients, compared to the population, is 1 in 33; exactly three per cent.

Of the three parishes, one contained none but persons of the very poorest class; of another about half, and of the third above half, possessed the means of support.

The proportion of cholera patients in the poorest to those in the wealthiest parishes in Venice, is 100 to 15.

The cures in the hospitals were to those at home as 70.5 per cent. to 47 per cent., or as 1.5 to 1.

The relative proportion of deaths in the two sexes, (males 98, females 86,) is very near the proportion of the cases: males 150, females 136.

The patients are nearly all comprised in the five classes of the table; the well-educated and respectable (*regolati*) being too few to constitute an exception.

The occupations of the patients appear to have had considerable influence in the production of the disease, particularly among the washerwomen, who, from the facts collected by the author, became affected in many instances from washing the clothes of the sick. Of washerwomen, there were 31; servants, 21; labourers, 34; masons, 13; cooks, 14; gondoliers, 8: almost all of them of the lowest order.

The persons included in the five classes of the table are the following:—
1. Drunkards with wine or spirits, persons irregular in habits and diet. 2. Careless at the first appearance of disease, or who used improper remedies. (Two-fifths of the patients belonged to these two classes.) 3. Using bad food. 4. Affected with chronic complaints. 5. Bilious persons, the fearful, the weaker aged, the exposed to extremes of temperature.

It is a most remarkable circumstance that, of 2500 Jews inhabiting Venice, not one was attacked. The author attributes this immunity to their strict observance of their religious rites and ordinances, to their careful attention to their diet, and to their habit of retiring within their houses in the evening. [The Polish Jews suffered far more from cholera than the Christians in 1831, so that whole villages were almost depopulated; and this was attributed to their excessively dirty habits.]

TABLE II.—*Numerical Account of Cholera Patients treated in the Hospitals, from October 9th, 1835, when the Disease broke out, till December 20th, when the Hospitals were closed.*

Men.	Cases	Cured	Died	Per cent. of deaths	Women	Cases	Cured	Died	per Cent.	Total.			
										Cases	Cured	Died	perCent.
71	40	31	43 ²⁵ ₁₀₀	110	39	71	64 ⁵⁴ ₁₀₀	181	79	102	56 ³⁵ ₁₀₀		

[If we take Venice as the average for Italy, the mortality by cholera in that country is a little higher than in Scotland, and lower than in France; the average deaths, from the official returns, being in England one-third, in Scotland one-half, in France two-thirds, of persons attacked.]

TABLE III.—*Summary of Cholera Patients treated in the Hospitals, from March 31st, to June 30th, 1836.*

Admitted.		Cured.		Dead.		Under treatment.		Mortality per Ct.
Males	Females	M.	F.	M.	F.	M.	F.	
444	355	170	153	256	189	18	13	55.69.
Total, 799		323		445		31		

Appendice al Bullettino delle Scienze Mediche di Bologna. Fasc. 2. 1836.

Statistics of Cholera in Naples.

THE cholera in Naples, as elsewhere, exhibited three well-marked stages, in its commencement, increase, and decline. The first period lasted from the 2d till the 23d of October, when the weather was very tempestuous; and the number of cases and deaths increased rapidly for a month, when, about November 24th, the disease began to decline, and continued to decrease to the end of December, at which time it had almost entirely ceased.

	Cases.	Deaths.
First period, (commencement,) from Oct. 2d to 23d	210	126
Second period, (increase,) from Oct. 24th to Nov. 23d	6837	3620
Third period, (decline,) from Nov. 24th to Dec. 31st	2184	1314
Total	9231	5060

The military are not included in this account: their numbers were, cases 359, deaths 135, which make the whole number of cases 9590, and deaths 5195; the cures were 4090, the remaining 305 being still under care.

Quarters of the City.	Population	Cases.		Deaths.		Locality.	Inhabitants.
		Total	Per Ct.	Total	Per Ct.		
Porto	36715	2410	6.57	1418	3.36	Damp, unhealthy.	Very poor.
Pendino	31086	1179	3.79	551	1.77	Ditto ditto	Ditto.
Mercato	51551	1821	3.53	977	1.89	Ditto ditto	Ditto.
Chiara	27606	641	2.32	421	1.50	Rather better.	Better.
S. Carlo all' Arena	21209	492	2.32	248	1.17	Same as Stella.	{ Contains the poor-house
Vicaria	39653	755	1.90	386	0.97	{ As Chiaza.	
S. Ferdinando	31204	575	1.84	261	0.83	{ Higher and	{ Better off.
S. Guisepe	19608	323	1.65	212	1.08	{ healthier.	
S. Lorenzo	11191	168	1.50	105	0.94	{ On high ground, at a distance from the sea.	Wealthy.
Montecalorio	31154	409	1.31	215	0.69		
Stella	23278	196	0.84	127	0.54		
Avvocata	33028	262	0.79	145	0.43		
Citta Intera	357283	9231	2.58	5066	1.41		

The great majority of the persons attacked were the poor, the over-worked, the dirty, the depraved, the intemperate, and those who were exposed to the changes of weather. There were very few persons of property who suffered, except those who lived in unhealthy situations, or whose habits were such as above.

Many pregnant women took the disease, which was generally fatal to them.

Returns from the Hospitals and Provincial Districts.

HOSPITALS.	Cases.	Deaths.	Cured.	Under Care.
Consolazione	819	515	276	27
Sa. Maria de Loreto	902	487	415	
Total.....	1721	1002	691	
DISTRICTS.				
Castellamare	1360	619	741	
Cavoria	116	61	55	
Pozzuoli	104	44	60	
Total.....	1580	724	856	

During January, 1837, 119 cases occurred, of whom 96 died; and 16 among the soldiers, of whom two died; which, added to the previous totals of 9590 cases and 5195 deaths, make a grand total of 9725 cases and 5293 deaths.

Fil. Sebez. January, 1837.

Sydney Dispensary, New South Wales.

[THE following short document possesses some interest in itself, as indicating the amount and relative prevalence of diseases in a climate of which we as yet possess scarcely any medico-statistical details: we confess, however, that it is more interesting to us as a testimonial of the progress of our science—ever the handmaid of benevolence,—in the new nations of the earth.]

“The Committee of the Sydney Dispensary have published their Report for 1836, and we are pleased to see in it an approximation to the usages of such Institutions in Britain, by which alphabetical lists of the diseases treated during the year are published for general information. This is a new feature in Colonial Reports of this kind, and will prove interesting to science and the colonists. Inflammatory complaints, it will be perceived, are more frequent than those arising from other causes. As a document of value, we present to our readers the following list of diseases without abridgment:—

Alphabetical list of the diseases treated at the Sydney Dispensary, from 1st May, 1836, to 30th April, 1837:—Abortion 1, Abscess 4, Amaurosis 1, Amenorrhœa 2, Aneurism 1, Aphthæ 1, Apoplexy 1, Asthma 4, Biliary derangement 7, Bubo 2, Burns and Scalds 12, Cancer of the Lip 1, ditto of the Mamma 2, Carbuncle 1, Cholera 1, Chorea 1, Colic 3, ditto Painters 2, Constipation 7, Consumption 9, Contusions 33, Cramp 1, Croup 2, Cutaneous diseases 14, Delirium Tremens 1, Dentition 4, Diarrhœa 20, Disease of the Heart 3, ditto of the Hip Joint 2, ditto of the Knee Joint 1, ditto of the Spine 1, Dislocations 3, Dropsy of the Belly 5, ditto of the Brain 1, ditto general 10, ditto of the Knee Joint 1, Dysentery 22, Dyspepsia 5, Erysipelas 9, Fever, continued 6, ditto, intermittent 1, Fistula in Perineo 2, Fracture of the Arm 3, ditto of the Clavicle 1, ditto of the Leg 3, ditto of the Ribs 1, Gangrene 1, Gastrodynia 4, Gonorrhœa 12, Gout 1, Headach 6, Hemoptysis 1, Hemorrhoids 6, Hernia 6; Ileus 2, Imperforate Nostril 1, Inflammation, Bowels 5, ditto Bronchia 19, ditto Ear 1, ditto Eyes 38, ditto Liver 7, ditto Lungs 3, ditto Pericardium 3, ditto Periosteum 1, ditto Peritoneum 2, ditto Pleura 6, ditto Testes 6, ditto Throat 4, Influenza 27, Insanity 1, Jaundice 2, Leucorrhœa 4, Marasmus 5, Menorrhagia 4, Mercurial Disease 5, Necrosis 2, Palsy 9, Paraphymosis 1, Prolapsus Uteri 2, Rheumatism, Acute 7, ditto Chronic 32, Scurvy 2, Scrofula 7, Sprain 1, Strangury 7, Stricture 1, Syphilis 34, Toothach 1, Tumours 1, Ulcers 22, Varicose Veins 3, Vertigo 3, Worms 12, Wounds 12. Total number of cases, 560—of these nearly one hundred were attended at their own houses.

The Sydney Herald. May 29, 1837.

Contributions to Statistics of Diseases. By Dr. CASTER.

THE following table is not only interesting, as showing the proportions in which various diseases and malformations occur in France, but as affording an additional proof that those phenomena of nature which appear the most accidental,—e. g. the existence of hernia, &c.—are dependent on immutable laws, the existence of which the science of physico-medical statistics has yet to prove and to establish. The following table shows the number of young men who, in France, in the years 1831, 32, 33, were returned, under the recruiting system, and the diseases or defects on account of which they were returned.

	1831.	1832.	1833.
Deficiency of fingers . . .	752	647	743
Deficiency of teeth . . .	1,304	1,243	1,392
Deficiency of other limbs . . .	1,605	1,530	1,580
Deafness and dumbness . . .	830	736	725
Swellings of the glands of the neck, .	1,125	1,231	1,298
Lameness . . .	949	912	1,049
Other "deformities" . . .	8,000	7,630	8,394
Diseased bones . . .	782	617	667
Myopia . . .	948	891	920
Diseases of the eye . . .	1,726	1,714	1,839
Itch . . .	11	10	10
Scald-head . . .	749	800	794
Tetters . . .	57	19	29
Other skin diseases . . .	937	983	895
Scrofula . . .	1,730	1,539	1,272
Diseases of the chest . . .	561	423	859
Hernia . . .	4,044	3,579	4,222
Epilepsy . . .	463	367	342
Other diseases, not yet mentioned, .	9,168	9,058	10,286
Weakness . . .	11,783	9,979	11,259
Too small stature . . .	15,935	14,962	15,078

The regularity of the occurrence of diseases is here very remarkable. The only irregularity deserving notice is under the class "Tetters;" and the arrangement of diseases under this head would, of course, depend much upon the different opinions of the medical men. Any one accustomed to inquiries of this kind will readily grant that the slight disparities observable in the above list would appear much less, if the table included a period of from twenty to thirty years, instead of but three years.

Wochenschrift für die gesammte Heilkunde. No. 51. 1836.

VETERINARY MEDICINE.

Observations on Rabies in Animals. By Dr. WAGNER, Medico-Forensic Censor of the Schieben District.

[THE following observations are interesting and valuable, on account of their authenticity. We particularly recommend them to the notice of gentlemen practising in the country, who may have opportunities of witnessing similar cases. We are firmly convinced that medicine is destined to derive no slight advantages from observations made on the diseases of animals; and, on this account, as well as others, we are gratified in observing the gradual, but still too slow, improvement in the pathological knowledge of our veterinary surgeons.]

1. *In the Dog.* A dread of water is not a symptom invariably attending rabies. I have met with two instances of men being bitten by dogs, and dying from rabies, when the dogs, nevertheless, ate and drank shortly after having made

the attack. One of these was seen to swim through the river Elster on the ensuing day. Another, in the same state, displayed great activity in the water for several hours, in search of wild fowl, until, on a sudden, it ceased to recognize its master, appeared to have lost the senses of sight and hearing, rushed out of the water, and scampered away, snapping on the road at every object within its reach.

Though rabid dogs are frequently in the habit of gnawing at wood, straw, or hides, snapping round about in the air, &c., such signs are equally fallacious with the foregoing one, healthy dogs being sometimes observed to do the same.

An appearance of dejection and shyness, a hanging of the head and tail, running at the eyes, or the fierce flashing eye without this, foaming at the mouth, refusal of food and drink, are all suspicious symptoms, and demand caution; but, as they are attendant upon other canine diseases likewise, they are not conclusive. In fact, experience has furnished me with no evidence *decisive* of the existence of rabies, except that of a dog running away and recklessly attacking both men and animals, especially those of its own species. No other dog, even the most vicious, when accidentally separated from its master, will attack any save animals of its own kind, unless in self-defence. In a rabid state, some dogs will even leap over high fences, in order to reach dogs or cats which they discover to be on the other side; a proof that scent, sight, and hearing are still unimpaired. Others will sneak along a wall, or run forward in a straight line, attempting to bite whatever they thus meet with, but never diverging from their headlong course, unless to pursue other dogs. I have observed some rabid dogs which never moved from one spot, merely gnawing at everything within reach, as they lay, snapping at vacancy, and ultimately refusing food and drink, or, if they attempted to partake of either, appearing incapable of deglutition. In these cases, palsy of the loins and hind extremities appears to have taken place at the outset, and to have deprived the animals of the power of locomotion. Drooping of the tail and head, and foaming at the mouth, are, of all signs, the least to be relied on: they belong only to the last stage of the disorder, when the animal has almost ceased to live. On the other hand, it is a well-known fact that some dogs, in a state of health, constantly foam at the mouth, and that all look dejected and hang their tails when they have wandered astray. The same observation applies to the absence of barking, or to the hoarse bark: many dogs bark but rarely, whilst with others the bark is perpetually hoarse. On the contrary, many rabid dogs continue to growl or raise a howling bark, (so that a single short bark is succeeded by a shrill lengthened howl,) similar to that made by healthy dogs at certain sights or sounds, from hunger, thirst, &c. Even in the appearance of the eye in rabies, the unprejudiced observer will not discover any essential change.

The moment a dog evinces any traces of illness, it is no longer to be trusted; and it would be well to lock it up, or fasten it to a stout chain. But, when it begins to gnaw wood, to show a dull eye, to snap at animals with which it had become familiarized, to bark hoarsely; when it attempts to run away or to break its chain, eats and drinks with a snapping gesture, at intervals appears lively, and then again sneaks sulkily to its kennel; when it disregards its masters' call, and, contrary to its former habits, growls and snarls at well-known persons, the animal ought to be despatched; for there can no longer remain a doubt of its being rabid. If a dog runs away, and returns home on the second or third day, with any unhealthy symptoms, its rabidness is equally certain, more particularly if it then ceases to know its master.

The rabid dog is often very sudden in its motions, darting at its victim with the quickness of lightning. In the first stages it bites slyly, and rather pinches than wounds; but, at a later period, the bite is so terrible, that force is sometimes necessary to disengage the animal. I witnessed two cases in which the dogs could not be made to relax their hold until their spine had been divided by a blow with a hatchet.

2. *In Oxen.* In horned cattle, amongst which I have met with the most nume-

rous instances of rabies, this disorder evidently occurs under two distinct forms. In solitary cases, where we are not previously aware of the animal having been bitten, we should scarcely be led to a suspicion of rabies. The commencement and progress are as follows:—With a healthy exterior and clear eye, the animal loses its appetite, eats and drinks by fits only, without however refusing choice morsels that may be offered to it; at times it appears as if suddenly stupified, recedes from the manger, and forgets to chew, then again chews on, listens with liveliness to every noise that is made, seems to notice every object, continues to obey its keeper, and sometimes remains capable of working: borborygmi are, however, already to be heard, and there is sometimes observable a slight straining or disposition to tenesmus. In the cow, the secretion of milk decreases, but by no means ceases at the very commencement. In the open air, the animal ceases to graze, appears lost, strays from the rest of the herd; although when, by itself, it generally allows itself to be led quietly back to the stable.

After the lapse of from one to three days, all the symptoms increase; neither hunger nor thirst is experienced, although no dread of water is apparent, and the animal still ruminates at intervals. The eyes have commonly a healthy look, but flash at times, without seeming inflamed. The creature lows but seldom; but, when it does, several times consecutively, and either with a hoarse tone, or, on the contrary, in so clear and powerful a key, that I have heard it distinctly at a distance of more than a mile. The mouth and tongue have still nothing abnormal about them. The borborygmi increase. At the same time the animals are observed to lick some particular part of their bodies, especially one of the feet, (probably the spot where they were bitten,) until that part becomes excoriated and bleeds. Palsy of the loins now ensues; the animal remains lying, and, if forced to rise, which it does with difficulty, totters on its hind legs; an increased straining is remarked at the anus, and this is sometimes followed by hard but ultimately by thin evacuations; it moves its head alternately from side to side, and appears anxious to lick at cloth or fur, &c., which it by degrees gets between its teeth, and tears. To this the suffering animal appears not to be urged by any vicious propensity. At this period, in the cow, the secretion of milk is at an end.

The above symptoms continue on the increase until the sixth or seventh day. At length the hind extremities become so paralysed as to render standing on the feet impossible; the constant straining at the rectum still forces out faeces, and the animal refuses all, or almost all, further sustenance; even bread that is forced with the hand into the creature's throat is not swallowed. A dread of water is never observed, even though the latter be refused. Naturally thin and rough-haired cattle become still thinner, and their hair rougher; but such as were originally in good condition and smooth haired show little alteration in either respect. Some time between the sixth and the ninth day, the animals sink on one side, (mostly, I have observed, on the left,) with the head commonly stretched backwards; the eye still remaining bright, lively, and uninfamed. From henceforth the trunk continues motionless, whilst the legs undergo a constant but languid movement to and fro, until the animal has ceased to exist. In the form above described, the animals may be approached with little or no danger. Not so in the second form; of which, however, scarcely one case is observed in six.

This form commences like the other. In the stable, the animals recede still more from the manger, and will even break the rope with which they are fastened, if it is not very strong. Their bellowing, without being frequent, is prolonged, its tone clear, thrilling, and unmodulated. They scratch the ground with their fore hoofs, often with such violence that the dung is projected to the roof of the stable; whilst with their hind legs they kick violently at any one who approaches. At this stage the paroxysms are periodical, and little is to be observed during the intervals, except a reluctance to feed or drink. About the fourth day, if they are naturally strong, they will snap every kind of fastening in twain during the paroxysm, and then attack and gore all who approach them. They rage about in the stable, gnawing the cribs and other objects to pieces, until palsy of the joints supervenes, when they sink down, and ultimately fall on one side. In this

position they still manage, spasmodically as it were, to shove themselves about the stable by means of their hind legs. Last of all, they lie for hours on the side, as if dead, when the bright shining eye, and occasionally a convulsive movement, alone indicate a remnant of life. On the seventh, eighth, or at most the ninth, day, they die.

On dissection, nothing abnormal is to be discovered beyond a gall-bladder filled to excess with dirty, yellowish-green coloured bile in a state of complete fermentation. From the great distention of the bladder, the fermentatory process is easily distinguished from without.

3. *In Horses.* With horses the symptoms of rabies are as follows: in the team they display unwonted activity and emulation. Here, however, as well as in the stable, they assail their fellows with bites and kicks. They generally refuse both provender and water, nod or jerk the head frequently, (as horses are in the habit of doing, when they are in spirits), and have a fiery eye. On the second, or at most on the third day, these symptoms increase to such a degree that no creature can approach them without being bit and kicked in the most frightful manner, and to offer them either food or water appears only to augment their rage. Yet moments of quietness intervene, provided the stable be kept dark and nothing around them stir. On the other hand, such is their fury when disturbed, that on a plank being taken out of the stable door in order to destroy them with safety, they will rush to the spot, and thrusting their head through the aperture, make attempts to bite in every direction. How long such a state continues, until death occurs spontaneously, I am not able to determine, as in the cases which came under my observation, the natural termination was not waited for.

4. *In Pigs.* During my long continuance in office, I met with but a solitary instance of rabies in the pig. This animal, a fattening boar, had been bitten, four days previously to my seeing it, by a mad dog, but it already raged with such violence, that I could only observe it through the crevices of the sty wherein it was inclosed. It gnashed its grinders, jumped up at the sides of the sty, at one instant, and threatened to burst through them, at the next, by the violence of its efforts. I directed it to be killed, and, together with the cleansings from the sty, to be buried at a great depth in the ground, which I was afterwards assured had been done. I subsequently discovered, however, that a butcher who had slain the animal with a hatchet, instead of allowing the carcass to be buried, had cut it up and exposed it for sale, and that, from its fatness, it had been readily disposed of. Notwithstanding the infamy of this act, aware that the possible evil consequences could no longer be averted, I too much dreaded the effects of the imagination in those who had partaken of the meat, to hazard a legal investigation on the subject. The disgraceful occurrence therefore slept in silence, but from that day to this—and it happened twenty-four years ago—I have not heard of any mischief arising from it.

5. *In Sheep.* In like manner, I have met with but one instance of rabies in sheep; in this one, however, five sheep, which were bit by a mad dog, all became affected at one and the same period, after the lapse of a few weeks. The symptoms, first noticed, were that the animals left off grazing and dispersed the flock by indiscriminate attempts to butt and to mount upon their fellows. They were all killed save one, which, being confined in a stall, kept quietly staring at the wall with a dejected look, as long as every thing around it was noiseless; but on hearing the slightest sound, the animal turned to the direction from whence it proceeded, and jumped up at the sides of the stall. Food and water were left untouched. Here my observations were cut short by the proprietor declining to await the natural termination. In a large flock, single cases of disease are usually overlooked until the symptoms become very conspicuous, and it appears more than probable that such was the fact in the present instance. The above details are therefore only to be received as a rough sketch.

The peasants would frequently hold fast the tongue of the rabid cattle with one hand, whilst they thrust the other deep into the creature's throat, in the endeavour to force nourishment into its stomach. That the naked hands and arms were on

some of these occasions *not* entirely free from injury, I can scarcely doubt; and yet I never witnessed any evil effects to arise from the practice, although these people neglected to wash their hands subsequently. However, I knew a farmer to die of rabies, merely from having, with hands apparently uninjured, washed out the wound inflicted on a pig, by the bite of a rabid dog. On one occasion a slaughterer who had killed a rabid ox, in spite of my admonition, repeatedly thrust his naked arm into the animal's intestines without experiencing any evil therefrom, and cowherds, soi-disants veterinary surgeons, and the like, often commit similar acts with equal impunity.

I have frequently known the milk of rabid animals to be taken without detriment, and in two instances the flesh of rabid oxen which was clandestinely eaten, proved no less innocuous. It is nevertheless a fact that, at a period when many cattle perished of rabies, the instances of canine madness become unusually numerous; and it may be supposed that, however deeply the carcasses may be buried, dogs will still here and there succeed in disinterring them. Further experience is therefore requisite to decide on the effects of the saliva, blood, excrements, &c. in all domestic animals, with the exception of the dog and the cat. In conclusion, I have witnessed many instances where the bite of decidedly rabid animals has produced little or no effect on the human subject, although the remedies employed were merely such as were suggested by superstition. From this, I am led to infer, that with mankind a predisposition to hydrophobia very rarely exists.

Hecker's Annalen. 1st Band, Heft 4. 1836.

ANIMAL CHEMISTRY.

Microscopical Observations relative to Stains upon Linen. By M. RATTIER.

THE most simple method for this mode of investigation consists in separating, if possible, the part on which the experiment is to be made, placing it in a vessel of moderate depth (such as a watch-glass,) and pouring upon it some distilled water to the depth of one or two lines. As soon as the stain is well soaked, its surface is to be slightly scratched with a spatula; in some cases, where the matter is in very great quantity, it is sufficient to shake the liquid for a moment. If we then submit the solution thus obtained to the microscope, we shall find, if the stain is from blood, globules of blood, some of which are deformed, in proportion to the antiquity of the stain and the rubbing it has sustained; but, in the majority of cases, they will be always found presenting such characters that, if they are once seen, cannot be mistaken. In the solution of mucus, of pus, and of semen, especially of pus, the globules are seen with the greatest facility; and, in the solution of semen, in the midst of the remains of spermatc animalcules, we shall always find some entire.

Journal de Chimie Médicale. March, 1837.

Observations on Blue Urine. By M. DRANTZ.

OF all the changes of which the urine is susceptible, none are more remarkable than the various colours it assumes in different diseases, and of the nature of which we know so little. M. Braconnot, in analyzing some blue urine, obtained, by simple filtration, a matter of the same colour, to which he has given the name of cyanourine. MM. Julia de Fontenelle, Mojon, and Cantu have announced that they have ascertained that this colour was due to hydroferrocyanate of iron (Prussian blue). M. Drantz took the opportunity of examining a small quantity of blue urine passed by a young man afflicted with influenza. It was of a deep blue colour, deposited, upon standing, a blue matter, which had all the properties of hydroferrocyanate of iron. Deprived of the colouring matter by means of filtration, it gave, upon analyzation, a large quantity of albumen, gelatin, salts and some traces of urea. The patient was not making use of any ferruginous preparation at the time.

Journal de Chimie Médicale. June, 1837.

PART FOURTH.

Selections from the British Journals.

(FOR THE QUARTER ENDING NOVEMBER 30, 1837.)

PHYSIOLOGY.

On the Acoustic Principles of the Stethoscope. By CHARLES COWAN, M.D.

CONSIDERABLE difference of opinion has always prevailed respecting the cause and mode of transmission of the pectoral sounds in auscultation, and, consequently, respecting the principles on which the stethoscope should be constructed. By common consent, however, the perforated wooden cylinder of Laennec has been received as the essential type of all, however varying in external form; and it has been generally considered that the bore, open at both ends, was very important in transmitting the respiratory sounds by means of the vibrations of the air contained in it. In the present paper, however, Dr. Cowan expresses doubts both of the principle and of the alleged fact. We have only room to notice here some of the results of numerous experiments performed by him, and the conclusion to which he has arrived respecting the principle on which the stethoscope acts, and its best form. We invoke the aid of Dr. Charles Williams, to extricate us from the difficulties into which Dr. Cowan has thrown us.

“1st. If one or both ends of the stethoscope are accurately closed with cork, the respiratory murmur is only *very slightly* weakened, and the instrument would still suffice for every practical purpose.

2d. If the interval between the corks be filled with water, the sounds appeared rather louder and sharper than when the instrument was unclosed.

3d. If the cone be filled with soft cotton, and the ear end plugged with the same material, the difference in the respiratory murmur was barely appreciable.

4th. The tube was successively obstructed with dough, wax, paper, at one or both extremities; the only appreciable result was a certain diminution of the sounds, but never to such an extent as to make them not easily heard, or to neutralize the indications of the instrument.

6th. When the cavity was obstructed by a silk handkerchief, it produced very little, if any, difference in the sounds: filling the tube with water considerably diminished its conducting power: leaving the extremity of the tube next to the chest open seemed rather to diminish than increase the sounds.

In all cases, accurate contact of the conducting medium with the chest and ear was indispensable. The vocal vibrations were evidently louder and more defined through the stethoscope closed at both ends and filled with water, than when open.”

“From all these facts and considerations, we feel justified in concluding that the stethoscope acts almost entirely as a solid conductor, under all circumstances, in reference to the sounds within the chest; and that the column of air which it encloses exerts little, if any, influence upon its efficiency as a conducting medium;—that the superiority of a perforated cylinder over a close one simply depends upon the former being more susceptible, by its form and lightness, of transmitting vibrations too feeble to affect a more solid instrument; but that the benefit obtained has been greatly overrated, and the explanation hitherto given erroneous.

We are inclined to believe, that the best form for a stethoscope would be a simple cylinder of light wood, about an inch and a half in diameter, and a tenth of an inch in thickness, carefully closed at both ends by a layer of the same wood,

and of the same thickness. The exact length is of secondary importance. This would combine lightness and uniformity of material, a vibrating surface in contact with similar media on both sides, and great simplicity of structure."

Med. Gazette. October 28, 1837.

On the Reaction of the Saliva upon Red and Blue Litmus and Turmeric Paper.

By T. LAYCOCK, Esq., House-Surgeon of the York Hospital.

THIS is an interesting addition to the physiological subject to which it refers, and is precisely such a communication as the house-surgeons of hospitals are so well qualified to give, but which, we are sorry to say, they so rarely do give. Our readers may remember that, some time since, M. Donné made experiments on this subject, which were afterwards repeated and extended by Dr. Robert Thomson (see *British and Foreign Review*, Vol. III. p. 541.) The former gentleman stated that he never found the saliva acid when the digestion was good, and stated that acidity of the saliva is a diagnostic symptom of gastritis. Dr. Thomson found the acid reaction in all cases of inflammation of the mucous and serous membranes. With the view of testing these opinions, Mr. Laycock instituted numerous experiments, and in the present paper gives, in tables, the results of no less than 567 observations.

Mr. L.'s observations were made indiscriminately upon forty-eight in-patients of this hospital: of this number there were not three cases alike: it comprised diseased joints in every stage, ulcers, accidents, and the various other diseases usually found in a general hospital. Thirty-six, or three-fourths of the whole number, asserted that they had a good appetite and digestion. This number included the most varied cases: ulcers on the legs, with and without impaired health; diseased hip-joint with sinuses, emaciation and diarrhœa; senile gangrene; schirrous uterus, &c. Eight had appetite and digestion more or less impaired; of these, two were under the influence of mercury: four had loss of appetite, or frequent vomiting. The diet of these individuals was various: a few were on fever and full diet, but the majority on house diet, consisting of beef or mutton, and baked flour or rice puddings, on alternate days, for dinner; and tea, milk, or broth, for breakfast and supper.

The following are the principal deductions supplied by Mr. L.'s tables: they are at variance with those of M. Donné.

1. The saliva may be acid without any apparent disease of the stomach, and when the individual is in good health.

2. It is alkaline during different degrees of gastric derangement, as indicated by the tongue.

3. It may be alkaline, acid, and neutral, when the gastric phenomena are the same; and, consequently, acidity of the saliva is not a diagnostic mark of gastritic derangement.

4. In general it is alkaline in the morning, and acid in the evening.

Med. Gazette. October 7, 1837.

Observations on the Pulses of Infants. By J. GORHAM, Esq., M.R.C.S.L.

SOME short time since, Dr. William Guy read a very valuable paper "on the Effect of Posture on the Pulse," before the members of the Physical Society of Guy's Hospital, of which a very brief notice appeared in the *Med. Gazette* of October 28th. In this paper, which contained the results of numerous original experiments made by himself, Dr. Guy showed that the increased frequency of pulse in different positions of the body was in the ratio of the muscular action required in each case, and depended on this. We are sorry that the non-publication of Dr. Guy's paper prevents us from giving even an outline of its contents; but it has already been productive of much good, by giving occasion to the present very valuable communication by Mr. Gorham. It consists of a series of tables of the pulses of infants at different ages, and contains the results of 150 different observations, carefully made (the author informs us,) under varied circumstances and conditions of the infantile state. We can only find room for the principal conclu-

sions deduced by Mr. Gorham from the different tables. These, however, exhibit very distinctly the general results, and show the important nature of this communication.

I. From Birth to the age of Twenty-four Hours. (16 Observations.)

1. The mean number of the pulse of the new-born infant is 123 and a fraction.
2. The maximum number in the above experiments was 160, and the minimum 100.
3. The mean number of three, who were stated in the table as being in the horizontal posture (for I believe all were horizontal, although I cannot aver this for a fact), was 124.
4. The average number of the pulse of three male infants, taken from the above experiments, was 120.
5. The pulse of one twin female was 116.

My first conclusion is derived from facts, without any allusion to the posture, the state of motion or rest, or muscular exertion. Neither do I know of any author who has noticed these particulars as regards the infantile state, and as connected with the pulse. My third conclusion is derived from three experiments: a scanty number, truly; yet more, indeed, than has been made by any one else, according to my reading.

II. From one Day to one Week Old. (42 Observations.)

1. The mean pulse of the infant between the age of one day and one week is 128.
2. The maximum in the above experiments is 160, the minimum 96.
3. The pulse of one female in the horizontal posture (although most of them were probably in the horizontal posture, yet it is not stated in the table, and must not, therefore, be given as a fact) was 120.
4. The mean number taken from eleven females is 131.71.
5. The mean number taken from nineteen males is 122. From four and five it results that the pulse of females is much more frequent than that of males.
6. The average of three experiments on children asleep, and in the horizontal posture, is 108.
7. The pulse is quicker during the first week than during the first day.
8. The pulse is slower during the first week than it is found to be afterwards.

III. From one Week to one Month Old. (31 Observations.)

1. The mean number of the pulse in the infant, from one week to one month old, is 135.45.
 2. The maximum in the above table is 176, and the minimum 104.
 3. The mean, from experiments on eight in the horizontal position, is 125: the mean of two of them which were asleep being 122, and of two which were twins, and not asleep, 108.
 4. The mean, from experiments on six females, is 141.67.
 5. The mean, from experiments on five females, is 130. From five and six it results that the pulse is quicker in females than in males.
- From the second and third tables, which together include experiments on 73 infants, it results—that the pulse in the infant from one day to one month old, is 131.

IV. From One to Five Months old. (15 Observations.)

1. The mean pulse of the infant from one to five months old is 148.85.
2. The maximum number in the above experiments is 176, and the minimum 104.
3. The mean of five, which were in the horizontal posture, is 144; and of these, four were asleep, the mean number of the pulse being 140.
4. The mean of five, in the semi-recumbent position, is 151.20; and of these, three were sucking.
5. The pulse is much more frequent after the first month than before it; and this frequency increases up to a certain period.

V. *From five Months to two Years old.* (19 Observations.)

1. The mean pulse of the child, from the fifth month to the second year, is 130.
2. The maximum from these experiments is 158, and the minimum 100.
3. The mean of five experiments, in which the children were in the horizontal position and fast asleep, is 126.40.
4. The mean of five experiments, in which the children were sitting, is 142.
5. The mean of three experiments, in which the children were standing, is 128.33.
6. The pulse at this age, which embraces the teething period, is quicker than at birth.

VI. *From two to four Years old.* (14 Observations.)

1. The mean pulse in the child from two to four years old, is 112.56.
2. The maximum number in the experiments is 124, and the minimum 92.
3. The mean of four, who were in the horizontal position and asleep, is 102.
4. The mean of four who were standing, is 118.
5. The number of one who was sitting, is 120.
6. The pulse at this age, which has passed the teething period, is much slower; and from this time it gradually diminishes in frequency to the end of life.

VII. *From four to ten Years old.* (13 Observations.)

1. The mean pulse in the child between the fourth and tenth year, is 107.63.
2. The maximum in the above table is 133, and the minimum 88.
3. The mean of five experiments on those in the horizontal position, is 104.
4. The mean of six experiments on those who were standing, is 110.17.
5. The mean of four experiments on those who were sitting, is 107.50.
6. The pulse is slow in the horizontal posture. It acquires frequency if this be changed to a sitting, and becomes still more frequent in the erect position.

Mr. Gorham very candidly admits "that, although all the observations that have been hitherto made on the pulse of the infants are not without their value, yet that they are rendered imperfect and susceptible of improvement, by taking into consideration the circumstances of posture, and laws emanating therefrom, mentioned in the extremely valuable paper of Dr. Guy."

Owing to the inability of very young infants to preserve themselves in the sitting or erect position by means of the action of their own muscles, Mr. Gorham thinks that the acceleration of pulse witnessed on placing infants in the erect position must be attributed to some other cause than that adduced by Dr. Guy, viz. muscular contraction. We do not, however, coincide in this opinion; because infants, although incapable of *supporting themselves* by muscular contraction, may still employ this, and no doubt do so, to a greater extent than when laid in the horizontal position.

Med. Gazette. November 25, 1837.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Report of St. John's Fever and Lock Hospitals, Limerick.

By W. J. GEARY, M.D.

In our last Number, (p. 536) we gave a brief notice of this Report, which is now concluded. It is, as we stated, a very valuable document, and is highly creditable to the good sense and zeal of the author. The following extracts are both interesting and important.

I. *Illustrations of the Causes of Fever.*

1. *Suddenness of Contagion.* (Dr. Geary's own case.) "After walking a good deal during the day, the weather being unusually warm, I visited a child, residing in a miserable, ill-ventilated cabin, who was represented to have been ill for several days before. The room in which she lay was very confined, and, having no window, I had her brought to the door of the outer apartment, when she was found to

be attacked with maculated typhus. Immediately after examining the case, I was seized with nausea, vertigo, and a general chill. On returning home, I was unable to take my dinner, and, on returning to my room from the table, I stated the circumstances, and my belief that I had caught fever. The disease set in that night, and so urgently did the acute symptoms supervene, that the head was leeches and blistered the next morning. The attack lasted for twenty-one days; and, among other distressing symptoms, singultus continued for seventy-two hours."

2. *Fever from Putrid Effluvia.* "We have seen a very decisive instance of this kind, arising from the effluvia of a foul and ill-conditioned wound of the head, succeeding an extensive fracture of the right parietal bone, with laceration of the dura mater. It occurred in 1819, to my father, while attending an inquest on a young man, who died from the injury. Having sat in the draft, he perceived the 'scent from the wound' intolerably offensive, and felt that he had then inhaled the poisonous dose. In the course of the day he became more sensible of his situation; on the next evening he was obliged to yield, and thenceforward continued in severe and dangerous fever for many days."

3. *Fever from Camphor.* "Miss —, a young and healthy lady, about twenty years of age, walking by a chemist's shop, was suddenly overpowered by the odour of camphor as she passed the door. She turned to her sister, and exclaimed, 'Oh, did you smell the camphor?—I am sure to have a fever now!' Her predictions were too truly verified; she was attacked with the disease that night, and had a serious illness of several days' duration. This case is the more remarkable as one of the lady's sisters had fever some years before, which was attributable to the 'smell of camphor!'"

II. Influence of Age.

"Table shewing the Number admitted at stated Ages of five Years; their Relation and average Mortality per Cent., from 6th January, 1836, to 5th January, 1837.

Ages,	5	10	15	20	25	30	35	40	45	50	55	60	65	70	100	Tot.
Admitted,	81	489	762	701	362	304	100	203	70	82	23	36	2	10	2	3227
Died,	2	13	18	37	22	27	12	45	13	22	5	12	1	5	1	235
Average Mortality per cent.	2 $\frac{1}{4}$	2 $\frac{1}{2}$	2 $\frac{1}{4}$	5 $\frac{1}{4}$	6	8 $\frac{3}{4}$	12	22 $\frac{1}{4}$	18 $\frac{1}{2}$	27	21 $\frac{1}{2}$	33 $\frac{1}{4}$	50	50	50	7 $\frac{1}{4}$

"By comparing the number of admissions and of deaths for successive terms of twenty years, it will be found that the susceptibility to fever diminishes, while the proportionate fatality augments, with the increase of age:—viz. for the first term, in 2033 admissions, the deaths were 3 $\frac{1}{2}$ per cent., while, for two succeeding periods of equal duration, the mortality reached 11 and 25 per cent. respectively; the admissions being 969 and 211. Of the entire treated for the year, full two-thirds were under twenty years of age; and only one-third of the mortality occurred within that term. These deductions shew satisfactorily the influence which age has both on the extent and fatality of fever cases."

III. Influence of Sex.

"The influence of sex appears in favour of the males, one-third less having been attacked than of the females. The greater delicacy of the latter, as well as the disproportionate amount of labour they undergo, slenderly clothed, and barefooted, seem to be the principal cause of their greater susceptibility to fever. As regards the mortality, there does not appear to be any remarkable difference, the proportion being 1 in 13 $\frac{1}{4}$ of females, and 1 in 14 of males; though that for six years, from 1814 to 1820, gave the proportion of females as 22 to 15; a relative average which commonly exists."

IV. *Influence of the Period of commencing Treatment on the Mortality.*

"Among the assignable causes of mortality, too much importance cannot be attached to the period of disease at which the sick are brought to hospital. It seems a well-established point, that the chances of recovery are in the exact ratio of the period of illness at which the sick come under treatment. . . . The beneficial result of early treatment is clearly represented in the following table.

Day of illness when admitted,	2	3	4	5	6	7	8	9	10	11	12	13	14	Over	Total.
No. of Cases	84	319	484	538	472	289	479	170	131	28	41	16	67	88	3206
Deaths,	3	10	22	28	33	30	36	15	21	2	5	1	5	24	235
Mortality per cent.	3 $\frac{1}{4}$	3 $\frac{1}{8}$	4 $\frac{1}{2}$	5 $\frac{1}{8}$	7	10 $\frac{1}{4}$	7 $\frac{1}{2}$	9	16	7	12 $\frac{1}{2}$	6 $\frac{1}{4}$	7 $\frac{1}{2}$	27 $\frac{1}{4}$	7 $\frac{1}{4}$

Dublin Journal. September, 1837.

On the Inadequacy of the Valves of the Aorta. By WM. HENDERSON, M.D.

In this paper the author reviews the state of our knowledge respecting the diagnosis of this affection, as successively improved by the observations of Drs. Corrigan, Hope, Guyot, Charcelay, M'Adam, and Watson. In addition to the signs already announced, Dr. Henderson gives a new one from his own experience; viz. "a greatly increased interval between the systole of the heart and the pulse of the remote arteries, such as the radial." . . . "The increased interval," says Dr. H. "was so great that the radial pulse exactly alternated with the systole of the ventricles, or occurred in the middle of the interval between the successive pulses of the heart. I have examined many persons affected with disease of the heart in different forms, and found this peculiarity of the pulse in none, except those in whom there were signs of defective action of the aortic valves."

In the following extract, Dr. H. "sums up what appears to be warranted regarding the more prominent signs" of this affection.

"1st. Visible pulsation, or remarkable leaping of the arteries of the head and upper extremities, is a very common attendant on the disease; yet not worthy of being considered a pathognomic sign, because it is not always present in a degree of peculiar significance, may result merely from a greatly enlarged left ventricle, (to which, indeed, M. Guyot attributes it in patency of the valves,) and may be caused by continued nervous excitement, (Corrigan;) nor is its true cause revealed by,

"2dly. Bruit de soufflet and freuissement, accompanying the diastole of the arteries, as maintained by Dr. Corrigan; for the former of these may result from mere irritability, (Hope) and the latter is often absent altogether in cases of patency. These phenomena may also result from disproportion between an enlarged ventricle and the orifice of the aorta, and from vegetations on the valves.

"3dly. A bellows-murmur, in place of the usual second sound, most audible along the track of the aorta and its primary branches, is an indication of patency of the aortic valves, except in those rare cases of aneurism in the course of the sub-sternal aorta which have the power of originating two murmurs. The musical or cooing note, in place of the second sound, has hitherto been found to originate only in cases of patency of these valves. The morbid murmur during the diastole of the heart, whether a bellows-sound or a sonorous note, may, contrary to the opinion of Dr. Hope, be very loud in cases of incompetency of the valves. In some cases, however, there are no such sounds.

"4thly. The occurrence of a preternatural interval between the period of the heart's contraction and of the remote arterial pulse, promises to prove a valuable

sign of inadequacy of the aortic valves. Should it prove, on further enquiry, to be constant, it will remove the difficulties in the differential diagnosis of patency.

"5thly. The usual full pulse and tortuous state of the arteries are natural consequences of the great volume of the left ventricle, which, since the time of Haller, has been known to result from insufficiency of the semilunar valves."

Edinburgh Journal. Oct. 1837.

Effects of deficient Ossification of the Cranium. By JOHN GRANTHAM, Esq., Surgeon, Crayford.

OUR readers will remember the important observations first made by the late Sir Gilbert Blane, of the necessity of proper support from the cranium to enable the brain to execute its functions; and the practice founded thereon of bandaging the head in certain cases of hydrocephalus. In the present communication, Mr. Grantham adduces several cases to shew that epilepsy, cerebral congestion, hydrocephalus, &c., are often the consequences of imperfect ossification of the skull in infants, and are relieved by bandaging the head. The titles of the first three cases will shew the nature of the author's views and practice:

1. Epileptic fits—open sutures—roller to the head—recovery. 2. Epileptic fits—temporary improvement from the application of roller—death. 3. Restlessness and general indisposition—bandage to the head—recovery.

Med. Gazette. Sept. 1, 1837.

On Aortitis, as one of the Causes of Angina Pectoris; with Observations on its Nature and Treatment. By D. J. CORRIGAN, M.D., Physician to the Jervis-street Hospital, Dublin.

THIS is a valuable contribution to the pathology of the diseases of the heart and great vessels, and throws some valuable light on that complex and occasionally obscure affection, angina pectoris. The object of the paper is, "1st, to shew that *inflammation of the lining membrane of the mouth of the aorta* is capable of producing the group of symptoms to which we give the name of angina pectoris, and is therefore entitled to a place in the list of the causes of that formidable affection; 2dly, to trace the pathology and treatment of aortitis." With these views, he relates three series of cases, first, "cases in which the patient died, while the disease was acute or recent; then those cases of longer duration, which exhibit the alterations of structure produced by the disease, when uncontrolled in its progress; and, lastly, those cases in which, from analogy with the symptoms of the former cases, there was every reason to suppose the existence of the disease, and in which treatment founded on the supposition was attended with success."

The first class contains two dissections, from Portal and Bouillaud, of patients dying suddenly from attacks of suffocative dyspnoea and palpitation, in whose bodies dissection shewed evident signs of recent inflammation at the origin of the aorta; and two cases observed by the author. The following is one of them:

Glynn, æt. 44, "had been complaining, for three months previously, of debility and cough, accompanied with attacks of dyspnoea, attacking him when walking or working, and obliging him to stop frequently to sit down. He also suffered acutely from sensations as if of tearing asunder in his chest. After exercise, he suffered from palpitation.

"Physical examination of the chest gave extensive dulness over the præcordial region, with tumultuous and indistinct action of the heart, and puerile respiration in the lungs, but no other sign of disease. On the 24th, five days after admission, head symptoms set in,—viz. dry retching, followed by delirium and stupor; and he died on the 26th.

"*P. M.* There was some fluid in the pericardium; the whole heart was enlarged, the left ventricle and auricle particularly so. The lining membrane of the aorta, just above the valves, was of a vivid red colour, and was protruded considerably beyond the natural level by an effusion of red and (apparently) organized lymph, which lay behind it, effused between it and the fibrous coat of the vessel

This vividly red and swollen portion of the vessel contrasted strongly with the pale and polished surface of the artery a little further on."

There is only one case of the second class, which exhibited during life the symptoms of disease of the heart and angina pectoris. "On dissection, the semi-lunar valves of the aorta were found perforated and cartilaginous, and the lining membrane of the aorta, from the mouth of the vessel to its arch, contained underneath it innumerable atheromatous depositions."

The third class contains two cases, supposed to be examples of inflammation of the aorta, and cured by treatment administered with this view. The following is one of them:

"In February, 1835, Mr. D. passed through a very severe attack of acute rheumatism, and in the course of it was frequently seized by what were supposed to be fits of spasmodic dyspnœa. I saw him in his convalescence. He was suffering little from articular pains, but there was very strong action of the heart, with indistinct bruit de soufflet; and exercise brought on severe palpitations. I warned him of his danger, but in vain. He was so impressed with the conviction of his heart symptoms being nervous, that no persuasion could induce him to submit to treatment conducted on any other supposition. In eighteen months afterwards he came to town to consult me: his lips were livid and his feet œdematous, and he was suffering from severe and frequent paroxysms of dyspnœa. He dreaded to lie down, and the fits of palpitation and dyspnœa were brought on by any exercise, but more particularly by attempting to walk up an ascent. The abdominal organs were sound. The chest sounded well on percussion; and the respiration was in every part natural, or somewhat puerile. The pulsation of the heart was felt over a large space with strong impulse, and there was very slight bruit de soufflet in the præcordial region; pulse was about eighty-five, and small. These were the symptoms on the 4th of August, 1836. Leeches were applied over the region of the heart, and ten grains of hyd. c. magnesia given three times a day; while abstinence from wine and from stimulant antispasmodics was strictly enjoined. A seton was inserted before the left mamma.

"In four days the mouth was made sore, and the change in his state was as gratifying as it was rapid. The breathing became easy, the fits of dyspnœa ceased, and he slept without disturbance and without dread in the recumbent posture.

"On the 22d, I again made his mouth sore. He then returned home, and has never had any return of his former distressing symptoms. While these sheets are going through the press, (October, 1837,) I have again had an opportunity of seeing him. He is in perfectly good health, after having undergone the exciting and arduous exertions attendant on taking an active part in two of the most violently contested elections in the kingdom."

The other case was treated in a similar manner.

The following conclusions are deduced by Dr. Corrigan from the review of the premises:

"1st. That, in some cases of what are called angina pectoris, the paroxysms of dyspnœa, anxiety, mental distress, &c., constituting a fit of angina pectoris, and often supposed to be merely nervous, are really the symptoms of aortitis, or inflammation of the mouth of the aorta.

"2d. That the treatment, in such cases, is the adoption of local bleeding, counter-irritation, and the exhibition of mercury, which experience has taught us are the means best calculated to prevent the effusion or cause the absorption of lymph.

"3d. The pathology of the disease, which these cases have enabled us to trace, encourages us also to put into requisition our treatment, and to persevere in it after even a considerable lapse of time has passed by, in instances too where, without this knowledge, we should have looked upon the case in despair, from the belief that irremediable organic disease had been established."

These observations are highly important in a practical point of view, and deserve attention. We believe that many cases of affections of the heart and large vessels are lost, from the false conviction of their hopelessness entertained by practitioners. On the whole, we think Dr. Corrigan has made out his case; although the co-

existence of disease of the heart, along with that of the aorta, in some of his cases, renders the conclusion, that the aortitis was the cause of the angina, not quite logical.

Dublin Journal. Nov. 1837.

On the Remedial Powers of the Aconitum Napellus in Headach.

By W. C. RADLEY, Esq., Surgeon, Newton Abbot.

MR. RADLEY was induced to try this remedy, many years since, at the recommendation of the late Dr. Turton, the botanist; and found it most efficacious. The cases in which the aconite was successful (for it was not always so,) were those of idiopathic nervous headach, not dependent on other causes; and it acted better on the delicate and nervous than on the sanguine and robust. Six cases are related, in four of which it produced almost an immediate cure, and in one constant relief in the attacks. "The form which I have used," says Mr. R. "is the simple extract made from the inspissated juice expressed from the bruised leaves of fresh-gathered plants, in the latter end of May, just before the time of flowering, poured into shallow vessels of earthenware, and allowed to evaporate slowly in the shade in warm weather. Its consistence is tough, colour nearly black; it keeps well, and a few grains, the last of about a pound made nineteen years ago, are as good and as efficacious now as they then were."

This extract was administered in the form of pill, in doses varying from one grain to two and a half grains; the smaller dose being preferred. The pill was repeated according to its effects: sometimes a single dose seems to have effected a cure.

Lancet. Sept. 23, 1837.

On the Use of Tartar Emetic and Opium in Spasmodic Affections.

By R. G. ACKERLEY, Esq., Surgeon, Liverpool.

THE author deprecates, and with much propriety, the habitual employment by practitioners, of that class of remedies usually called antispasmodics, but which are in fact powerful stimulants, in cases of spasm. Mr. A. thinks that we should rather look to remedies "possessing power of relieving the excited state of the brain and nervous system, and thus allaying the tonic muscular contractions which characterize this class of diseases." He fulfils his indication as follows:

"The remedy which I propose, (and it is one which I have used with the greatest success for the last three years,) is the tartar emetic, in combination with opium in some of its forms. The sedative effects of the opium are powerfully increased by uniting with it the antimonial salt, while its narcotic properties are diminished. The manner in which I prescribe it is the following:—Three or four grains of tartar emetic, with two drachms of laudanum, or one of Battley's sedative liquor of opium, and two ounces of water. A tea-spoonful of this mixture to be given every fifteen minutes, until relief is obtained, and afterwards every hour, until all symptoms of the affection have disappeared. Its first effect is generally to produce nausea, or even vomiting. The latter I encourage, where, as is frequently the case, I have every reason to suspect that the spasm proceeds from improper or undigested food in the stomach, acting as a source of irritation. After this, the medicine is generally retained; *tolerance*, as Rasori describes it, being established, and the spasms speedily subside. It may be given in spasm of the stomach, diaphragm, spasmodic asthma, and during the paroxysm of hysteria, with the most beneficial results. I have administered it in such cases in the advanced stage of pregnancy; nor do I consider an irritable state of the stomach, with vomiting, any objection to the use of it, the sickness generally subsiding after the second or third dose. Should the symptoms be very urgent, if the case will in other respects allow of it, I do not hesitate to bleed, as a most powerful auxiliary; but phlebotomy may in general be dispensed with. Enemata are also at times beneficial, particularly where the attack is accompanied by constipation, though the tartar emetic sometimes produces evacuations from the bowels, and renders their administration unnecessary."

Med. Gazette. October 7, 1837.

Observations on the Nature of Neuralgia, and on the Principles according to which the Treatment ought to be conducted. By JONATHAN OSBORNE, M.D., Dublin.

THIS paper contains some interesting facts and remarks, but we doubt if the author's views are so original as he supposes. He says,

"After a due consideration of the recorded facts, and a comparison of them with the cases under my own observation, I have for some time past arrived at the conclusion, that the nature of neuralgia is altogether different from any of the opinions respecting it now mentioned; and that it is nothing else than pain arising from paralysis of the nerve sufficient to alter its mode of sensation, but not so complete as to obliterate it."

In proof of the accuracy of this opinion, he adduces many facts and arguments, the principal of which we will give in an abridged form:

- "1. Pressure on a sound nerve produces a kind of neuralgia.
2. The affections usually accompanying neuralgia are of a paralytic character.
3. The *adjuvantia* and *lædientia* prove the disease not to be one of excitement. Sedatives rarely give relief, while tonics and stimulants often do.
4. Inflammation of a nerve or its sheath is marked by different symptoms from those of neuralgia.
5. The recorded cases of neuralgia from supposed irritation of a foreign body originated from pressure on the nerve, producing paralysis.
6. In neuralgia from indigestion, &c., the accompanying symptoms point to paralysis, rather than any other state.
7. The pain accompanying various organic diseases are really neuralgic, and arise from pressure produced by the organic disease. The intercostal and other pains commonly attributed to spinal irritation are rather to be viewed as the consequence of vascular congestion, pressing on the nerves at their origin, and thus causing imperfect paralysis."

"11th. What appears to me, however, the strongest and most decisive argument that neuralgia is a torpid state of a nerve, is derived from the effect of acupuncture, which cannot be conceived to act in any way than as an irritant."

In illustration of the truth of his theory, and of the efficacy of acupuncture, he gives the following interesting case.

"Mr. M., aged thirty-six, during the last two years the left side, but during four years previously, the right side, was the chief seat of pain. His sleep was only obtained during short intermissions of pain, which, although never entirely absent when awake, was yet variable, and at times increased to the utmost degree of violence. The mouth was slightly drawn upwards to the left side; the motion of the opposite side impeded. The tongue slightly protruded towards the left; the right eye directed inwards from the axis of vision. Within about six months from the commencement of his illness, he gradually lost the sight of the right eye, and about the same time the hearing of the right ear. His sight had been recovered, but in the right ear hearing remained defective, with the sensation of a buzzing noise in it." . . . "I commenced the local treatment by rubbing over the affected part the extract of belladonna, which was kept constantly applied. Having on former occasions seen much relief from this in slight cases of facial neuralgia, it was desirable to see what it could effect in this case of six years' standing. He experienced no appreciable action beyond the prickling sensation it causes on the skin, and the dilatation of the pupil. Acupuncture was then practised behind the ear, and about the zygomatic arch. It was followed by a remission of pain greater than he had hitherto ever experienced. Next day the pain behind the ear again began to be felt: the needles were applied there in greater number. On the following day the pains were reported as fugacious, and rather resembling an apprehension of its recurrence than pain actually present. On several following days he continued in this state, but his nights continued sleepless, although free from pain. Considering that his want of sleep was now kept up principally by the habit of lying awake, I gave him acetate of morphine, in doses of one-sixth of a grain, in solution, every

two hours, commencing in the evening. This proving inadequate, he got twenty drops of the black drop. Sleep was at once restored. His general health, which had suffered much, began rapidly to amend. The pulse, previously 116 and hard, fell to 84, and became soft. About four nights afterwards the pain recommenced behind the ear, and further back than when acupuncture had been applied. Needles were applied to that part, with the effect of completely dislodging the pain."

The following is Dr. O.'s account of his mode of using acupuncture.

"Acupuncture being, according to my opinion, indicated exclusively in neuralgia, and being in it a most powerful remedy, which rouses the nerve from incomplete to complete sensation; its efficacy is greatest when the nerves themselves are transfixed in the greatest number. In tic douloureux of the face, as the nerves of that part are spread over a greater extent of surface than in the fleshy parts of the lower extremities, which are the seat of sciatica, it is evident that the chance of nerves being transfixed is much less; and hence, I think, acupuncture will in these cases require to be more frequently repeated. The needles which I use in facial neuralgia are less than one-half inch in length, and are generally the pointed ends of well-tempered needles broken off. They must also be inserted in greater number."

Dublin Journal. November, 1837.

On artificial Respiration in the Asphyxia of Still-born Children, and during Convulsive Fits. By LAWSON CAPE, M.D.

THE practice here recommended has been long in use in the first class of cases. Mr. Terry, of Northampton, seems to have the credit of having first enforced the great importance of long-continued perseverance in it, even in apparently hopeless cases.

"In one instance related by him, resuscitation took place after two hours and a half; and in the other, after one hour and three-quarters: in the latter instance the child being completely restored. I have met with several instances where I have been equally successful, when the action of the heart was not in the least perceptible, and where, for a very considerable time, all appearance of vitality was absent, and yet, after artificial respiration had been kept up for a time in which perseverance seemed hopeless, the action of the heart began to be distinguished, the livid aspect gave place to the colours of health, and the lungs at length were called into action."

Dr. Cape strongly recommends the same means in the asphyxia frequently met with in the convulsive fits of children, and gives an interesting case illustrative of its effects. The infant was five days old, and had a succession of violent convulsions for thirteen or fourteen hours, on the average of a fit each hour.

"The pressure produced upon the brain was such that respiration was entirely suspended during the greater part of the fits, and even the action of the heart could not be felt for more than ten minutes in the third and twelfth fits, and the child lay to all appearance dead. It was at such times that I proceeded to restore the suspended functions by artificially inflating the lungs, in the manner described by Mr. Terry: namely, by breathing into the mouth of the infant from my own, closing the nostrils, and compressing the thorax after each inflation; observing the natural periods of frequency as much as possible. I am quite convinced the child would have been lost, had it not been for the artificial aid thus afforded to nature in the severe struggle, till the offending matter was expelled. By means of the artificial respiration, the colour (especially of the face and lips) turned from purple to red, but still there was no breathing, till a convulsive gasp announced the termination of the fit."

In this case the child recovered perfectly.

Med. Gazette. October 7, 1837.

Disease of the Spino-occipital Articulation, with the appearances on Dissection.
By T. R. BLACKLEY, M.R.C.S. &c.

THIS is a very valuable case in relation to headach and diseases of the spine, because it affords an instance of what may be termed physical pathology; a rare event in regard to these affections. It was not necessary to prove the intermitting character of *symptoms* depending on a fixed organic cause; but it is a good example of this common fact.

"Mary King, aged eight years, was admitted March 17th, 1837, into the Hospital for Children, Portobello, for violent headaches, to which she had been very subject for the last two months; these pains commenced suddenly on her return in the evening from the funeral of her sister, to whom she was much attached.

"Her countenance was peculiarly expressive of caution, and was florid, and full, if not bloated; the chin was advanced preternaturally beyond the chest, the mouth slightly opened, and she kept the arms parted from the sides as if to poise herself. On looking laterally, she strained her eyes in the direction of the object, and failing in this, turned her entire body for the purpose. The effect produced when she attempted to observe anything placed near her feet was yet more remarkable; for this purpose she generally put her hand to her forehead, as if fearful of undue weight in the head, and bent her body, thus avoiding the least motion between the first and second vertebræ. In getting up from bed also, or in lying down, she invariably supported the head with her hand.

"The headach was not permanent, sometimes she was free from pain for a couple of days, especially while using the warm bath, and on those occasions exhibited a most cheerful disposition; as the disease advanced, however, the intervals of rest were fewer and of shorter duration; the use of calomel seemed to afford considerable ease, until it slightly affected the mouth on the fifth day, from which time her sufferings appeared rather to be aggravated; leeching and blistering, which had been tried at the commencement of my treatment, to the back of her neck, were perfectly useless, nor was the insertion of a seton in the same situation attended by any happier result. The sufferings of this poor little creature were really most painful to witness, and at times amounted to agony; on these occasions she always wished to have a bandage bound tightly round her forehead, or requested the nurse to squeeze her head between her hands, but the most effectual remedy was the tincture of opium in small quantities, repeated when necessary. Of late she referred the headach more to the left than the right side; the pain on several occasions left the head, and traversed the spine to the sacrum, from whence it returned to its original seat; she also complained occasionally of pain in her stomach, but this was always relieved by the application of a jar of hot water to the part; the bowels were regular throughout.

"She expired without a struggle on the 29th June. Owing to the objections entertained by her friends to have the body examined, I was obliged to content myself with the removal of the diseased portions, namely, the cervical portion of the spine, and the portion of occipital bone in connexion with it; and with my friend Dr. Houston made a minute examination, the result of which was as follows:

"The articulating surfaces between the first and second vertebræ, and between the condyles of the atlas and occipital bone, were diseased, the cartilages eroded, the denuded bones gritty, and the capsules thickened and coated with lymph. The capsules of the loose articulations between the oblique processes of the atlas and dentata were particularly enlarged, and protruded forwards under the deep muscles of the spine, forming three distinct abscesses pressing against the pharynx; that on the right side had made its way downwards for more than an inch between the bones and muscles. The odontoid process was completely detached on all sides, and stripped of cartilage and synovial membrane, it was immersed in pus. The perpendicular, oblique, and transverse ligaments had altogether disappeared, and the odontoid process was only separated from the spinal marrow by the thickened sheath of that organ. All the joints enumerated, including those of the odontoid process, appeared to be engaged in one suppurating surface. The fibrous sheath of the medulla oblongata, and commencement of the spinal marrow, were much

thickened in texture, and the arachnoid and pia mater to the same extent were red and swollen; but the roots of the nerves were all perfect, and the medullary texture did not exhibit any organic lesion. The latter was perhaps a little softened, but had not undergone any alteration of colour; I say a *little* softened, because, as Dr. Houston remarked, in the healthy state this part of the spinal marrow feels more resisting than any other, and in this instance it yielded most easily to pressure with the finger." *Dublin Journal.* Sept. 1837.

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On Acupuncture in Hydrocele and Ascites. By D. LEWIS, Esq., and
T. KING, Esq.

ON a former occasion (*Brit. and For. Med. Rev.* Vol. III. p. 559,) we noticed Mr. Lewis's novel mode of treating hydrocele by acupuncture, and referred to its employment in other cases of circumscribed dropsy. In the present short paper, he records the result of his further experience of the practice, and adds an interesting case of its application to ascites by Mr. King. The following extracts specify both methods:

1. "Although I have seen radical cures take place from the simple operation of a single puncture, yet, as acupuncture is not a radical cure in all cases, I beg to submit to my professional brethren the result of my own experience. My method is to make a single puncture twice a week, for the first few weeks, so as to keep the tunica vaginalis in a collapsed state, and to bring on a healthy instead of a morbid secretion: by so doing, I find that the disposition to secrete water diminishes; and, therefore, if, on examination, I find only a drachm of fluid, I puncture. By pursuing this method, and employing at the same time the following ointment,—R. Hydriod. Pot. 3ss.; Ung. Cetacei, ʒjss. M. ft. unguent.—twice a day, a radical cure is effected very speedily in recent hydrocele; but, when the hydrocele has been of ancient date, the tunica vaginalis is so thickened that, although the fluid is absorbed, the parts do not appear to diminish, the thickening of the part preventing any evident contraction."

2. The disease shewed itself in July. "Upon deliberation, we were of opinion that some benefit might accrue to him from puncturing the abdomen with a long needle, (of the size used for darning,) so as to give the effused serum an opportunity of oozing from the peritoneal sac into the subcutaneous cellular tissue. The first puncture was made September 2d, on the right side, midway between the umbilicus and spine of the ilium, and was followed, as I withdrew the needle, by the issuing of three or four drops, one by one, of yellow serum. September 3d, another was made, anteriorly to the first. September 4th, there was a marked diminution in the tension. The abdomen was quite altered to the touch; the cellular tissue, which in this region is two inches thick, presenting an œdematous state, in lieu of the tense unyielding condition it was in prior to the operations. A third puncture was made between the other two. September 7th, I made two more punctures. 9th. Amelioration still marked, and the patient's general health is improving. As every operation was followed by some amendment, I made three punctures on the 11th, three on the 16th, three on the 18th, five on the 20th, six on the 21st, six on the 22d, and seven this day. The result may be stated to be a diminution of four inches in the circumference of the abdomen, and an amendment of the patient's general health, such that he is able to walk out."

Med. Gazette. Oct. 7, 1837.

In a subsequent communication, dated November 13th, Mr. King informs us that, since the former report, he has continued the same treatment, having made thirty-three additional punctures; and that the patient is, at the time of his writing, almost restored to health, the abdomen being reduced from four feet four inches to three feet four inches in circumference. The following account of the apparent mode of action of this treatment is curious:

"The fluid, with the exception of a small quantity which escapes through the opening in the skin, oozes for three or four hours from the cavity of the peritoneum into the subcutaneous cellular tissue, which it loads and œdematizes. From hence

it is gradually absorbed, and chiefly during the next five or six hours; after which the patient voids a good deal of urine, and often perspires pretty freely. This process appears to be continued or repeated more or less completely until the peritoneal orifices of the punctures close. Thus has the patient been relieved of the greater part of the fluid which the peritoneum contained." *Ib. Nov. 25, 1837.*

On the Treatment of Spasmodic Cholera by Sugar of Lead and Opium.

By R. J. GRAVES, M.D., of Dublin.

SCEPTICS as we are of an efficacious remedy being likely to be soon found out for the true and terrible Asiatic cholera, and utterly distrusting the power of *all* heretofore proposed,—most of them with such blind and unphilosophical confidence,—we cannot fail being struck by that here so strongly recommended by a physician of such talent and experience as Dr. Graves. During the prevalence of cholera in Dublin, in 1834, Dr. G., having had occasion to treat some cases of diarrhœa in fever with large doses of acetate of lead, according to Dr. Bardsley's plan, and being struck with its efficacy in such cases, was induced to give it first in dysentery, and then in cholera. The following extracts exhibit Dr. Graves's mode of treatment, and its very striking results:

"I saw a case of cholera, still in the stage of premonitory diarrhœa, or rather just passing from the bowel complaint into the fully formed disease. I tried the acetate of lead boldly, and with the happiest success. Thus encouraged, I applied this new method of treatment in every case to which I was called, and I was employed both night and day in visiting cholera patients, and every hour gave me additional proofs of the efficacy of the remedy. My formula was as follows:

R. Acetatis Plumbi, ℥j.; Opii, gr. j. M. fiat secundum artem massa, in pilul. xij. dividenda.*

"The premonitory diarrhœa has almost invariably stopped by taking one of these pills, at first every hour, and, as the stools became less frequent, every third or sixth hour, according to circumstances. When the vomiting, spasms, and the state of collapse had begun, it was necessary to give a pill every quarter of an hour: after a couple of hours the effect of the pills became perceptible, in a diminution of the serous evacuations upwards and downwards; then the pills were given only every hour; and as the symptoms yielded they were given less and less frequently, and could in general be laid aside altogether before twenty-four hours. In some it was found necessary to give the acetate of lead in solution, combined with a little vinegar and minute doses of acetate of morphia. Minute doses of opium were useful; anything of large doses hurtful. . . . Many took more than forty grains of the acetate of lead in twenty-four hours: it usually darkened, or even blackened, the alvine discharges, before they ceased altogether. Were I to enumerate all the cases of violent cholera that yielded to this treatment, I would be led into a tedious but not an uninteresting detail.

"After I found out the benefits resulting from the employment of acetate of lead, I no longer desponded when called to cases of cholera, knowing that in the great majority of instances the disease would yield. Of course, there are cases of cholera which admit of no treatment, and which an experienced eye will at once recognize as fatal: they occur generally among the aged or the very young, and are fatal in the course of a few hours, often without any premonitory diarrhœa. But this constitutes no valid objection to the practice; for in what disease do not cases occur which baffle all our efforts?

"I cannot conclude," says Dr. Graves, "without imploring the profession, in every part of the world where cholera prevails, to give my plan of treatment a fair trial; for I feel confident of its efficacy."

Med. Gazette. October 14, 1837.

* The most convenient way of making these pills is to add five or six grains of powdered liquorice to the scruple of the acetate of lead, forming a mass by mucilage.

On the Use of Colchicum in Scarlatina. By WM. TAIT, Esq., Surgeon.

IN our last Volume (pp. 249, 565,) we gave an account of the employment of this remedy by Dr. Lewin in fever. Mr. Tait here informs us, that he had employed it in scarlatina, without any knowledge of Dr. Lewin's practice.

"I administered it only to thirty-five patients, being little more than one-fourth of those for which I prescribed; but these, of course, were of the worst description, being all of the pure inflammatory type. In the most of these, bloodletting, both general and local, was had recourse to; in others, local bleeding only; and I may here remark, that the effects of the colchicum were always most apparent after detraction of blood; but in all the following changes were more or less manifested in a short time before its administration. The pulse was diminished in frequency and force; the palpitation of the heart, which in young subjects was often perceptible to the eye, subsided; the inflammation and pain of the throat were alleviated; and the patient often expressed himself 'much better.' Vomiting was excited in a few cases; but, as this seemed always to be followed by an improvement in a state of the tonsils, and generally abated after the rejection of a quantity of bile, it was never found necessary to interrupt the use of the medicine. The bowels were generally more or less purged, and the improvement was always so sudden and marked, after a free discharge of dark bilious stools, that I always considered my patient out of danger the moment they appeared. In some cases, where bloodletting was not premised, these effects were not so easily produced; two days having elapsed in one case before any change was observable.

"When called to a case of inflammatory scarlatina, it was invariably my practice, after administering a purgative, and bleeding from the arm, or locally by leeches, (as circumstances might require,) to begin with the Vinum Colchici, and continue it till all the inflammatory symptoms were subdued; a blister round the throat being all that was necessary to complete the cure. Almost the only gargle used was a little warm water, and the occasional inhalation of vapour; and with this, and the treatment above detailed, I have the utmost satisfaction in saying that I never saw a tonsil or any part of the mouth ulcerated. The dose of the Vinum Colchici was considerably below that administered by Dr. Lewins, and differed according to the age and strength of the patient. In no case did it exceed twelve or fifteen drops every three or four hours, in a little water, sweetened with syrup, and this only in strong, robust farm-servants: for children of four or six years, I began with three or four drops, and decreased the dose, watching its effects, and stopped whenever these were manifested."

Mr. Tait describes his success as singularly great, both actually and comparatively with that of his brother practitioners in the same epidemic. He only lost one patient out of 126, he says; while others lost one in five or six. We cannot believe that so great a difference as this could result from any difference of treatment.

Lancet. Nov. 4, 1837.

SURGERY.

Case of partial Paralysis of the Face from Injury of the Head, with Observations. By ALEXANDER SHAW, Esq., Assistant Surgeon to the Middlesex Hospital.

THIS is a very interesting and valuable case, and the remarks appended to it constitute an important addition to the pathology of partial paralysis of the face, as well as to the physiology of the parts concerned. A man, aged twenty, was admitted into the Middlesex Hospital on the 3d September, having fallen from a railroad-carriage in a state of intoxication.

"When brought to the hospital he lay comatose, breathing heavily, occasionally vomiting, and when roused, he struggled and became noisy. There was an open contused wound of the scalp, near the tuberosity of the occipital bone, a little to its left. Blood flowed freely from the right ear." . . . "6th. The muscles of

the right side of the face (except those that move the jaw) are found to be paralyzed. On a close examination, these are the only parts similarly affected. The sensibility of the side of the face is not impaired." . . . "7th. The symptoms are nearly the same. Although the distortion of the features is more remarkable than it was yesterday, the right eye is closed as in natural sleep; and even after the eyelid is raised, it descends again, so as to cover the eye." . . . "13th. The features are now so near being equally balanced, that the distortion of the face can only be perceived with distinctness when he is roused, and uses the right side of his face.

"16th. All his symptoms are improved, but the amendment in his face has not continued." . . . "On inspecting the throat, the uvula was found to be turned obliquely to one side, so that its point was directed towards the right amygdala; yet, when he inspired, the velum was elevated on both its sides. The eyelids were observed to be closed, as before, although their margins did not quite meet; and after elevating the upper eyelid, it descended again when he was asked to wink. But it was noticed that the eyelid did not descend with any degree of force; on the contrary, it presented a looseness and flaccidity which proved that the orbicularis was passive. On narrowly watching this motion, it was seen to depend, in a principal manner, upon the revolving of the globe of the eye, which takes place when we wink: for it was only after the cornea had been elevated, so as to be hid behind the raised eyelid, that the eyelid dropped over the white part of the eye." . . . "27th. Hitherto, owing to the general stupor with which he has been affected, it has been difficult to ascertain to what degree his hearing was injured on the right side. At one time it was our impression that the sense was entirely lost; but he now hears apparently quite well on this side, and only complains of a crackling noise in the ear.

"October 6th. When asleep, his eyelids were noticed to be perfectly closed; and he afterwards mentioned that it was without his consciousness that they were shut. A marked difference, however, is observed in their position when he is awake. A short time ago, when he lay comatose, they were always shut; and even after he recovered, the eyelids closed when he made the attempt to wink; but now they remain wide apart, and the upper one no longer descends, even if he winks with all his force. Indeed, it appears raised to a higher level than before."

Mr. Shaw enters into a somewhat minute consideration of the rationale of all the symptoms in this case, of the state of the eyelids and eye, the uvula, &c. We must confine our extracts to that portion of the paper which relates to the main affection of the nerve, and the ingenious explanation of the cause of the accident advanced by Mr. Shaw.

"The circumstance that gives the principal interest to this case, is the occurrence of partial paralysis of the face, in connexion with concussion of the brain. Previous to our improved knowledge of the functions of the nerves, such a combination could not have been explained in any satisfactory manner. No reason could have been assigned for certain muscles of the face alone being paralyzed, while others immediately adjoining those that were affected retained their power; nor could it have been understood how any of the muscles of the face should have become paralyzed without the integuments being deprived of their sensibility. The only way of accounting for this kind of paralysis would have been by supposing that the injury to the brain, occasioned by the fall, had given rise to it. Yet such an explanation would only have increased the obscurity that already characterizes the symptoms resulting from blows on the head; for no lesion of the brain hitherto described has ever been found to produce a loss of power so limited in its nature as that presented in the foregoing case. When it is known, however, that each nerve of the brain is endowed with its appropriate function, it becomes an easy task to distinguish between the effects of a local injury to one of them, in its course from the brain, and the symptoms denoting a general disturbance of this important organ itself. Here, for example, no doubt can be entertained, that independently of the violence to the brain giving rise to the usual symptoms of concussion, the portio dura of the seventh pair has received an injury in some part of its course;

and that this has occasioned the partial paralysis of the face. The portio dura, it has been ascertained, alone controls the particular muscles that have been paralyzed; it is known, also, that in its passage through the bones of the skull, it pursues a different route from the rest of the nerves; it can easily be understood, therefore, that it alone may have been injured, while the others escaped. Such an explanation will account for the sensibility of the skin, and also for the power over the muscles of the jaws remaining unimpaired, since neither of these properties depends upon the portio dura, but they are both conferred by the fifth pair, which takes a distinct course through the bones from that nerve.

“This case is a correct parallel to others that I have witnessed. Indeed, it may be stated that partial paralysis of the face, of the same kind as was exhibited in this patient, is not an uncommon effect of injuries of the head. It is, at all events, remarkable that from such accidents the portio dura is more liable to be injured than any of the other cerebral nerves. There is evidence of this in the recorded cases of affections of the different nerves of the head. If we refer to the papers on Partial Paralysis,* by Mr. John Shaw (containing the first illustrations submitted to the profession, of the practical benefits to be derived from the discoveries in the nervous system,) or to the collection of cases in Sir Charles Bell’s work on the nerves, where a most extensive series of local nervous affections from different causes is given, it will be found that a considerable number of these resulted from blows received on the head. Ten such cases, at least, may be counted. Now in all of these the portio dura was injured; and in one alone, characterized by Mr. Shaw as uncommon in its occurrence, the fifth pair was partially involved in the injury, along with this nerve. Hence it is interesting to enquire, what is remarkable in the anatomy of the portio dura that should render it so peculiarly subject to have its function destroyed in these cases of blows upon the head. The explanation, as it appears to me, is to be sought for by attending to the course which this nerve takes through the bones of the cranium, and by bearing in mind, at the same time, the principle of *contre-coup*.

“It is by a circuitous route, through a canal of some length and complexity, that the portio dura pierces the temporal bone. Now, it is at this very part of the skull that we learn, both by experience and by studying the forms of the bones on the principle referred to, that the vibrations caused by a severe blow on the head are most powerfully felt: it is here that fissures produced by injuries received at distant parts of the skull are most frequently found. We cannot be surprised, therefore, when the portio dura (which, notwithstanding its name, is extremely delicate in its texture) has to pass through the temporal bone, enclosed in a narrow canal, with boundaries of such great density, that it should be peculiarly liable to have its function destroyed by the vibrations of the skull. Blows may be inflicted on the head, of every different degree of violence short of producing actual fracture of the bone, which will, at the same time, cause a considerable spurring out of the bone at the temple, and consequently a shock extending through the canal that contains the portio dura; and no one can venture to say what slight degree of concussion communicated to the nerve thus enveloped may not produce the immediate loss of its power; or by occasioning the effusion of blood, or of serum, or of lymph, around it, give rise to the subsequent destruction of its function. Just as hæmorrhage from the ear, which was one of the symptoms in this case, may either indicate a trifling laceration in the tympanum, or a fracture extending through the base; so may paralysis of the muscles of the features indicate either a slight or a formidable injury to the temporal bone. But let this opinion be correct or not, I conceive that I am borne out in saying, that the frequency with which paralysis of the face, hæmorrhage from the ear, and, I may add, effusion of blood at the temple from rupture of the meningeal artery, attend violent blows on the head, is to be accounted for by the tendency which the vibrations have, on the principle of *counter-fissure*, to be concentrated towards the temples.

“Without hazarding an opinion as to the exact nature of the injury received by the nerve, in this particular instance, I may be allowed to make a few observations on the subject. When we consider the kind of accident that befell the

* Quarterly Journal of Science, 1821: Medico-Chirurgical Transactions, 1822.

patient, the state of insensibility in which he lay for more than a week, and that the hæmorrhage from the ear lasted even for three days, we cannot help concluding that there must have been a fracture extending across the temporal bone, lacerating the portio dura in its canal. A fissure through part of this bone might have existed, even although the symptoms were only those of concussion. Nor will the recovery of the patient be offered as an argument against this opinion. I have, in the museum of the school, two interesting specimens of skulls, exhibiting fissures (one complicated with fracture) extending into the temporal bones in both instances; the fissures approach closely to the canal for containing the portio dura; yet it is obvious, from the signs of reparation, that the individuals from whom they were taken survived the accidents. But if there had been a fracture, in Kearsley, we should have expected to find, along with the hæmorrhage from the ear, some puffiness or discoloration of the integument at the temple. It may also be argued, that had the injury to the bone been of such a severe nature, the paralysis would have immediately succeeded the accident, instead of appearing, for the first time, on the fourth day. Besides, the manifest improvement in the condition of his face, shortly after the first accession of the paralysis, is inconsistent with the idea of the nerve having been torn through. It is to be presumed, likewise, that if the bone had been fractured to any extent, the portio mollis could scarcely have escaped; yet when the patient left the hospital, his hearing was scarcely, if at all, impaired.

"It will, perhaps, therefore, be more consistent with the facts observed in the case of Kearsley, if we consider that instead of there being a fissure extending completely through the bone, there was only, in consequence of the shock communicated by the fall, a partial breaking up of the thin partition in the interior of the temporal bone that divides the portio dura from the cavity of the tympanum. Such a fracture would be attended with laceration of the lining membrane of the tympanum, and might have given rise to the hæmorrhage from the ear, without being sufficient of itself to disturb the functions of the portio dura, so long, especially, as the blood continued to escape from the ear. When, however, the blood ceased to flow, it is probable that it accumulated in the tympanum, coagulated and pressed upon the nerve, thereby producing the paralysis. This corresponds with the manner in which the paralysis of the muscles took place; for it did not commence till the hæmorrhage from the ear ceased; and it corresponds also with the partial recovery of the patient, for it is not unlikely that the effused blood would be gradually absorbed in part."

Med. Gazette. 1837.

On an Undescribed Displacement of the Bones of the Fore-arm in Children.

By JOHN GARDNER, Esq., Surgeon.

THIS is a case which most surgeons of experience must have noticed. We call the attention of our younger readers to it; it is well described by Mr. Gardner.

"There is an accident of very frequent occurrence happening to children, from the time when they are just beginning to walk to the age of from three to four years. A parent or servant is leading a child, or it is supporting itself by its hand—a sudden slip occurs—a slight crack is heard—the child screams—and upon examination is found unable to use its hand; the arm hangs powerless by its side, or is supported by the other hand, and every attempt to move it is attended with considerable pain. A surgeon is summoned, and on the first aspect supposes that either the clavicle is fractured or the shoulder-joint dislocated. But when, on a careful examination, this is found not to be the case, and the non-existence of either dislocation or fracture is satisfactorily ascertained, he believes it to be a mere bruise, places the arm in a sling, and keeps it bathed with cold lotions. After some time, whilst dressing or undressing the child, or on some sudden movement, another fall, or pull upon the arm, a slight crack is again heard, and to the great surprise of the parent, the hand is forthwith used, and is found to be quite well.

"The displacement consists in the tubercle of the radius, to which the tendon of the biceps flexor cubiti is attached, slipping over the edge of the ulna, and being retained there. I have never seen this displacement in adults; probably the laxity of the ligaments permits it only in children, and most frequently in very young

children. When a child is presented to me under these circumstances, after carefully ascertaining that there is no fracture either of the clavicle or bones of the arm, and no other dislocation, and the existence of this displacement being evident, I grasp the upper arm firmly in one hand, and with the other bring the fore-arm tightly supine, and suddenly bending the fore-arm upon the upper, the bones slip into their proper places: a slight crack is heard, and the child is well, and can at once use its hand.

Med. Gazette. September 9, 1837.

On the Objections made to the Performance of Extraction in Cataract.

By P. G. KENNEDY, M.D.

THIS communication is devoted to the consideration of the validity of Dr. Robertson's objections to contraction, of which we gave an account in our last volume, p. 257. It is a well written and well-reasoned paper, and it is due to Dr. Kennedy that we should notice it as well as Dr. Robertson's. The following extract from the conclusion of Dr. K.'s paper, gives a summary view of his opinions, and shews wherein he differs from Dr. Robertson.

"1st. Abstractedly considered, no particular operation for cataract is superior to another, because the forms which the disease assumes are so essentially different, and its complications with other ophthalmic and constitutional affections so numerous, that it is impossible to treat successfully all cases by any one particular plan.

"2d. The surgeon must therefore be able to apply each operation to those cases to which it may be suited; and the circumstance of extraction requiring the greatest degree of skill and dexterity can form neither a valid objection to its performance, nor an excuse for its non-performance, where the case may demand it.

"3d. When the case is one which leaves a choice to the surgeon, whether he shall perform reclinacion or extraction, the latter is to be preferred, because no objection to its performance is valid; and it is admitted by those to whom I am opposed, that, but for their supposed objections, it would be preferable.

"4th. It is absolutely necessary for the success of the operation in question, that the patient's constitution be sound, his general health good, and the eye, with the exception of the simple structural and functional alterations attendant on cataract, perfectly healthy."

Edinburgh Journal. October, 1837.

On the division of the Tendo-Achillis in cases of Club-Foot.

By JOHN WHIPPLE, Esq., Surgeon, Plymouth.

THIS is a very valuable practical paper, containing the details of no fewer than nine cases, all operated on by Mr. Whipple, and all with perfect success; if we except one in which the cure has been for the present defeated, owing to the occurrence of sloughing on the foot from improper management by the attendants after the operation. The patients were aged 9, 8, 28, 8, $2\frac{1}{2}$, $7\frac{1}{2}$, 14, 7, $1\frac{1}{2}$ years, respectively; and the comparison of the details justifies the deduction drawn by Mr. W. that infancy is the time most favorable for the operation. This surgeon's experience has so convinced him of the curability of this deformity by the operation when properly conducted, that he asserts "that every case of talipes verus, if not arising from cerebral or spinal irritation, can be cured by steady attention and perseverance on the part of the surgeon; and that to him alone, and not to the operation itself, is all blame due if he fails of success."

The following extract contains Mr. Whipple's account of his manner of operating, and his reasons for the particular plan adopted by him.

"The foot being extended as much as possible, the integument posterior to the tendon is pinched up about two inches above the os calcis, in order to separate it from the latter, when a narrow-bladed knife, with a rounded cutting extremity, is passed from within obliquely downwards and outwards; between the integument and tendon; and as soon as the point of the knife is felt under the integument, and on the outer side of it, considerable flexion of the foot is made by an assistant, the point of the knife being at the same time depressed, so as to bring it in contact with

the tense tendon, when, by firmly depressing and withdrawing the instrument, the object is instantly effected. This is made evident by the sudden jerk with which the heel is brought down, in some instances two or three inches, as in cases of talipes equinus. The knife should be passed from the inside outwards, for this reason: should you depress the point more than is necessary to divide the tendon, there would be no risk of wounding the posterior tibial artery, which would be the case were you to introduce your knife from without inwards; and it is essential to depress with some force, or you leave undivided some fibres of the tendon most remote from your puncture, and have to introduce your knife again (not a little embarrassed at your own bungling) for the purpose of dividing them. However, although the point of your knife be dipped some distance anterior to the edge of the tendon externally, in order to secure its division, this will not be necessary internally, as, the moment you feel your object effected, you discontinue the pressure on the knife, and withdraw it carefully, so as not to enlarge the integumental opening.

"This, I think, is by far the best mode of operating, as by this means you pass your knife across a relaxed tendon, which, when rendered tense, is brought up to meet the edge of the instrument, and therefore more readily divided than when you pass your knife between it and the deeply-seated muscles. Another objection to the latter plan with me is, that the tendon is in such close contact with the integument, that you run a great risk of dividing, or partially dividing, the latter, which, from the years of contraction to which it has been subjected, is rendered exceedingly tense when the foot is flexed. In upwards of thirty cases which I have examined, I have found no exception to this. Again, where the toes are the points of support, the tendon will be found nearly embraced by the integument, as in the corresponding tendon in the horse, though certainly not to such an extent. I must not leave this part of the subject without a remark relative to the division of other tendons apparently implicated, without the division of which it might be imagined that little would be gained; and, indeed, such was my own impression after the operation in the second case I have recorded. I had promised that one tendon only should be divided; but I confess that I left my patient with regret at having so given my word, and determined to gain the consent of the parents to the division of the others, if the muscles did not elongate by steady and constant extension, as I at first conceived they would, looking upon them as secondarily affected, their contractility being favoured by the rolling inwards of the foot. A few days, however, served to remove all doubt from my mind, as they were evidently relaxing. I abandoned then the idea of their division being necessary, and as yet I have had no occasion to regret it. I am free, however, to acknowledge, that it might be the means of a more speedy alteration of the shape of the foot; yet the chances of inflammation, together with the weakness which a want of union would necessarily induce, are sufficient reasons for its division not being attempted. No doubt can exist of its impropriety in cases of talipes verus, as will be illustrated hereafter.

"My reasons for dividing the tendon obliquely are as follows: First, by so doing you have a larger surface for nature to carry on her operations; secondly, you have the obliquely divided tendon in nearer approximation, and thereby secure a firmer ligamentous band than in the transverse division; and thirdly, the application of the instrument does not separate the lips of the wound—a desirable point, as the sooner it heals, so as to prevent the escape of lymph, the better. The puncture is dressed with adhesive plaster, and the instruments applied at once, as, where this has been deferred, the act of stretching the inflamed part has caused considerably more pain than the operation and early application combined. Much care and attention are required for the first three weeks or month, in order to keep the heel well down. Every thing depends on the heel and instep straps, and neither the fears and doubts of the surgeon, nor the ill-timed meddling of the parents, must interfere with the application of these straps; for, however aggravated the case may be, the removal of the deformity by proper treatment is certain. I know of no instance where patience is more necessary to the surgeon than in treating these cases; every thing is to be gained by it; for, by strapping too tightly and screwing too firmly, vesications are produced, which compel you to remove every thing for

their cure, and you lose more in twenty-four hours than you have gained in a week. Therefore, all you can do is to secure the heel firmly to the iron-sole, and to screw the plate so that it may merely rest on the cuboid and tarsal bones; then, from day to day, to draw in the strap a little tighter, so as to bring the end of the splint to the knee: a little pain and inconvenience are of course attendant upon this proceeding, but provided it does not produce vesication, this cannot be of any consideration when put in competition with the importance of the result."

Med. Gazette. September 2, 1837.

On the Use of Nitrate of Silver in some Cutaneous Diseases.

By HENRY T. CHAPMAN, Esq. Surgeon.

OUR readers are well aware of the use made of this remedy in erysipelas, &c., first recommended by Mr. Higginbottom. The present valuable paper contains practical remarks on its use in such cases and in others not contemplated by the original proposer. The following extracts contain the more important results obtained by Mr. Chapman.

"Of erythema nosodum two cases were treated: in one of them the redness and induration disappeared entirely after a single application of the caustic; in the other it was necessary to repeat it once more. My next experiment was made on squamous affections of the skin; lepra, namely, and several varieties of psoriasis. I possess the details of more than a dozen cases successfully treated by it, and have since employed it with equal success in a great number of instances, of which I have neglected to take notes."

"Besides the squamous disorders above mentioned, I have made trial of the nitrate of silver upon other cutaneous diseases. In porrigo, a strong solution of it is recommended by some practitioners almost as a specific. It was, I believe, a favorite remedy with Mr. Wilkinson, who acquired some celebrity for his treatment of this troublesome malady. I have tried it repeatedly both in solution and in the solid state, but in neither form was any permanent advantage gained by it. In sycosis menti and eczema, in both of which disorders, as well as in herpes zoster, its efficacy has been rated highly, I have been equally disappointed. In short, as far as my experience of its beneficial effects in diseases of the skin extend, it exerts a decided influence over those of a squamous character alone."

The following is the mode of applying the caustic as originally proposed by Mr. Higginbottom:

"After cleansing the skin with soap and water, and drying it, the surface to be submitted to its operation is again moistened, and a solid stick of the lunar caustic is rubbed lightly over it once or twice, according to the delicacy of the skin, and allowed to dry. No dressing is required, and the part must be kept cool. The slight vesication produced after the first twenty-four hours soon subsides, and about the fifth day the black pellicle loosens and peels off."

Our younger readers must not adopt this practice empirically or regard it as specific. Mr. Chapman is far from doing so, as the following sensible remarks show:

"In squamous disorders of the skin, besides the separation of the loaded cuticle, the nitrate appears to correct the diseased action of the capillaries, on which the accumulation depends, and restore a healthy tone to the vessels. This result, however, can never be expected, while the original source of the disease still exists in the system; and before the local remedy can produce any permanent benefit, constitutional measures must be steadily enforced. Some may exclaim that the cure, in that case, is unjustly ascribed to the nitrate; but is it not highly probable that the diseased product still continues to be formed habitually, long after the state of health giving rise to such an excretion has been completely rectified? In these affections, therefore, the nitrate of silver is only to be looked upon as an auxiliary to other remedial means, which must always occupy the foremost place."

Med. Gazette. October 14, 1837.

Cure of congenital Club-Foot by division of the Tendo-Achillis.

By THOMAS INGLIS, M.D., Glasgow.

THE interesting case here related is that of the narrator, and the operation was performed by Dr. Little, on the 11th of August last. At the date of the present communication (3d October, 1837,) Dr. Inglis says, "I have been walking about for the last four weeks. I may now say that I am not only free from deformity, but enjoy an ease, freedom, and power in locomotion, such as it was never my lot to enjoy at any previous period of my existence."

Lancet. October 21, 1837.

MIDWIFERY.

Cases of Inversion of the Uterus; with Remarks. By THOMAS RADFORD, Surgeon to the Manchester Lying-in Hospital, &c.

THIS is a valuable practical paper. The author takes a comprehensive view of the whole subject,—examines candidly the various opinions of preceding writers as to the causes, nature, and treatment of the affection,—and renders good reasons for his own. We can only find room for a few extracts relating to this latter part of the enquiry.

Causes of Inversion. "This accident has been attributed to causes purely mechanical, the uterus being unresisting, and passively obedient to their influence. The practice of pulling too early and violently at the funis, after the expulsion of the child, before the uterus has contracted, so as to detach and expel the placenta, has been generally considered as the cause of inversion; but we know that the accident happens before any force has been applied to the funis. . . . It has occurred when the patient had been delivered of a dead child, the funis so putrid as to break with a very slight effort. It has been found before the cord was separated, and the child given to the nurse. In the practice of Ruysch, this circumstance took place after he had extracted a dead child, &c. These circumstances shew that there is a power inherent in the uterus to become inverted. The pulling of the funis is so common a practice amongst our midwives, and done without the least consideration of the condition of the uterus, that, if it was so frequent a cause as is usually stated, inversion, instead of being one of the most rare, would be the most common accident in midwifery. Some writers have thought that a short funis is a frequent cause of inversion; whilst others think, in order to act, it must be inserted in the centre of the placenta, and that this mass must be attached to the fundus uteri. Now, it is evident, if brevity of the cord is capable of producing so serious an accident, these peculiarities will greatly add to its influence. But, amongst the published cases of inversion, there is, so far as the writer knows, but one where this shortness existed. It often occurs without diminished length in the cord; whilst, on the contrary, children are frequently born where it is very short, and yet no such event happens. The funis has been ruptured, and the placenta disrupted, and yet the uterus was not inverted.

"In order that the causes which have been now alluded to could operate effectually to produce inversion, there must be such condition of the uterus present that it becomes tacitly obedient to their influence. Most systematic writers, as also others, have supposed such to be the case. They have said that the uterus, previous to inversion, is in a state of extreme relaxation, exhaustion, or collapse, and that it offers no resistance to any force applied by the funis. These opinions are at variance with that of the writer.

"It appears to the writer that the uterine pain, diminution of bulk, firm resisting feel, sudden formation, and rapid protrusion, warrant him in the deduction that the *fundus* and *body* of the uterus, so far from being in a state of *collapse* or *relaxation*, are really in a state of *unnatural excitement and action*. But this is not the case with the *os uteri*: on the contrary, it is soft and yielding, as we find that it offers no resistance to the coming down of the tumour, whose protrusion is

forcible and rapid. If these statements be true, it is evident that the fundus and os uteri are in directly opposite conditions: the former is in a state of violent contraction, the latter in a state of relaxation; and that this relative difference in these two parts of the organ is indispensably necessary to exist where inversion occurs.

"From what has been stated, it may be concluded that quick labour, whether natural or artificial,—a disturbance of this process in any of its stages,—or any of those circumstances which produce irregular contraction of the uterus, are, singly or combined, the causes of inversion.

Treatment. "When the uterus is inverted only in a slight degree, the reduction may be accomplished with great ease, and the attempt should be made as soon as it is discovered. As the fundus uteri has not, or only slightly, passed through the os, the placenta cannot wholly protrude through this orifice; and, consequently, the fundus should be returned before the placenta is separated. For, if an attempt were made to detach the placenta, the operation must be slow, uncertain, and incomplete, and the danger of hemorrhage incurred, or a greater degree of inversion produced. When the hand is introduced through the os uteri, the fingers should be slightly bent, so as to form a kind of crutch, to carry up the fundus, which sometimes rapidly springs up. The placenta is now to be separated, and the hand retained until the uterus contracts.

"In the treatment of this accident, [great inversion,] the great object to be constantly kept in view is to attempt the reinversion as soon as possible after the occurrence. But in general the placenta adheres to the inverted organ, and the question is whether it should be separated or not before or after the reduction. It is an important point to settle, especially as there is such a difference of opinion upon the subject. . . . The dread of hemorrhage is the reason assigned why the placenta should not be first detached, but the writer trusts that the cases he has adduced, and the references he has made, are sufficient evidence to the contrary. In no case has this dreaded effect been induced, or even aggravated, by a complete separation of the placenta. The uterine vessels are as effectually constricted under this accident as when the organ is in its natural situation, if the placenta be entirely detached; and flooding is produced here, in the same manner as in ordinary cases, by a partial separation or disruption. As the greatest disadvantage arises from failing in our first attempt, it is the more necessary that every impediment should be removed, so that we can proceed with the greatest chance of success. By delay, the organ becomes less fit to bear the operation, not only from the increased size of the fundus and the contraction of the os, but also from the increased sensibility and irritability which it has acquired, even previously to its becoming actually inflamed. The attached placenta must increase the obstacle, because the fundus cannot be so freely and sufficiently compressed. The result of free manipulation would lead to partial detachment and disruption, and consequently to flooding. By detaching the placenta, great advantages are gained; the bulk of the part is diminished, the operator is enabled further to reduce the size of the fundus itself by compression; and he has more freedom to judge of the changes he has effected.

"When the placenta is detached, our next object should be to attempt the reduction of the general bulk of the tumour, by compressing it. We are indebted to Mr. C. White for this method. The plan recommended by some writers, to push the fundus directly upwards, should not be adopted.* There are strong reasons to think that the fundus is, after the os uteri, the most irritable part of this organ. When the accident has existed a short time, pressure upon this portion induces pain, bearing down, and hemorrhage; but the body may be taken hold of, and compressed. If we could press the fundus upwards, and thereby dimple it within itself, we should find ourselves opposed by a double inflexion; for the body would be grasped by the os uteri, and the fundus would be within the body. It is obvious that our force should be directed so as to act upon the angle of inflexion, or where it turns into itself.

"It will be found that the tumour will freely pass through the os externum, and, as only one hand can be admitted into the vagina, the chief compression should be

effected whilst it lies externally. And, as the upper part of the vagina descends along with the uterus, no real effect can be produced until it is made tense by carrying this organ upwards. When it arrives at this point, resistance is met with; but, by keeping a steady pressure upwards, the inflected portion of the cervix then yields, and it gradually recedes, followed by the hand of the operator, until the reduction is completed."

Dublin Journal. Sept. and Nov. 1837.

Case of Hidrosis, or Hidrotic Fevers, with Remarks. By J. C. W. LEVER, Surgeon.

THIS is a minutely described and very interesting case of one of the many varieties of affections which are usually confounded under the general name of Puerperal Fever. It has received the above name from Dr. Blundell, owing, we presume, to the continued and profuse perspirations which constitute a marked feature in this variety. We cannot agree with either Dr. Blundell or Mr. Lever in regarding this disease as distinct from puerperal fever, until we find pathologists more agreed as to what puerperal fever really is. At any rate, the affection here described by Mr. Lever is well known to all experienced practitioners as one of the most untractable forms of the puerperal malady. It has been well and very particularly described by Dr. Marshall Hall, many years since. In this case the patient, as frequently occurs, was in a state of disorder and general irritation long before her confinement, insomuch that the fatal result might have been prognosticated. "During the last three months of gestation, she suffered from sickness, from a constant dull pain in her left side, in front, near the linea semilunaris, midway between the crest of the ilium and the extremities of the floating ribs,—from relaxation of the bowels, frequently having as many as four or five motions in the course of twenty-four hours,—and from almost perpetual watchfulness, so that night after night was passed without one wink of sleep." The pulse was also at this time extremely variable and quick, ranging from 100 to 150: and it retained this character, in an increased degree, throughout the course of the disease. The other most marked symptoms were, the continual profuse colliquative sweats; diminution, but not entire suppression, of the secretion of milk; copious secretion of urine; extreme irritability of the nervous system, with great mental excitement or forced calmness; abdominal tenderness; tympanitis and diarrhœa; tongue clean. The patient died on the eighteenth day after delivery. The only morbid appearances of importance were those in the uterus. "On the left side of the uterus, where the placenta was adherent, and where the pain and soreness were located, only coagula were found occupying the uterine sinuses and veins, such as are commonly met with when the uterus is examined a few days after delivery, while on the right side, where there was no pain nor even uneasiness, there were spots of ecchymosis, ulceration, and softening; the uterine sinuses and veins were even found filled with a fluid more or less purulent, corresponding in its colour and nature with the discharge which was received on the napkins during life." The treatment employed in this case was various, (perhaps too varied,) and consisted of aperients, opiates, mercury, quinine, astringents, &c. Cases of this kind often resist all treatment, even the most rational.

Med. Gazette. Sept. 9, 1837.

MEDICAL STATISTICS.

Statistics of the Sickness and Mortality which occurred among the Troops employed on the Expedition to the Scheldt, in the year 1809. By HENRY MARSHALL, Deputy Inspector-General of Army Hospitals.

THIS is a most valuable contribution to medical statistics, and proves how admirably fitted its author is for the task in which we are happy to learn from this paper that he is engaged, along with Lieut. Tulloch, viz. "the compilation of the statis-

tics of the British Army, for the purpose of ascertaining the extent of the sickness and mortality of the troops, together with the prevailing diseases among the men in each military command." We are gratified to learn from the same authority, that some of these reports are already finished, and are forthwith to be published.

The present paper gives a full report of the medical tragedy of the Walcheren expedition, and places in the most striking light, at once the immense value of a knowledge of medical topography and medical statistics, and of the utter ignorance of these by the projectors and directors of this ill-fated armament. We can only find room for a few extracts and remarks; but we recommend the paper earnestly to the attention of our readers.

Medical Topography of Walcheren. "The surface of the islands of Zeeland is flat and low, being in some places below the level of the sea at high-water. As the water percolates through the banks, and accumulates by rains, much care and labour is required to remove it, which is commonly effected by means of sluices and mills. The water is collected in ditches and canals, and conveyed to the points where it can be most conveniently discharged over the banks. Inundations also occasionally occur, and as the water stagnates, the grounds are frequently left covered with slime and mud. The soil is excessively fertile, and produces corn, abundance of fruit, vegetables, and madder."

Its Influence on Strangers. "Dr. Wind, who translated into Dutch Dr. Lind's Essay on preserving the Health of Seamen, and who practised medicine in Walcheren for many years, informs us that the Scotch regiment in the Dutch service has been known to bury their whole numbers at Sluys, in Dutch Flanders, in three years. We learn from the report of Dr. Borland and his coadjutors, that, upon an examination of the sick returns of the French army for a period of seven years, it was found that at least one-third or 33 per cent. of its force was annually cut off by endemic disease. I may add, that when the English landed in Walcheren, there were only eighty-five men alive in a Dutch regiment, which at its arrival there, three years before, was eight hundred strong. The annual ratio of mortality in this corps must have been about 31 per cent., or rather more than double the mean ratio of mortality which occurs among troops in Jamaica."

Into this hot-bed of malaria were upwards of 60,000 men sent in the beginning of August; and here they remained until they were forced to reembark, a broken and disgraced host, vanquished by the elements, not the enemy. In the month immediately succeeding that of the debarkation, we find the proportion of the sick to the well exactly *one-half!* and the mean ratio of mortality, from the commencement of the expedition in July to the end of January following, (about six weeks after the reembarkation of the troops,) was among the officers 3.7, and among the men 10.3 per cent.! Six weeks after Walcheren was evacuated,—that is, after the return of the troops to England,—one-third of the troops of the line were on the sick-list, and of these a large proportion died. Nor did the evil terminate here. "Long after this date, (says Mr. Marshall,) many of the men who had apparently escaped the obnoxious influence of the climate of Walcheren were attacked, and suffered severely from the specific endemic disease, so that a very extended investigation would be required before a near approximation could be made of the consequences of the expedition to the Scheldt, in destroying the efficiency, and promoting the mortality of the British army. It is well known that among the regiments which had been employed in Walcheren, and which served afterwards in the Peninsula, many of the men were, upon the first exposure to cold and fatigue, rendered unfit for duty, so as frequently not to leave one-third of the strength fit for service."

After reading these statements every one must cordially assent to Mr. Marshall's proposition, that "the topography and statistics of the different colonies and dependencies to which troops are sent should form part of the education and study of every military man." Most certainly they ought to be studied by every medical man who enters into the public service; and we would here suggest to the present enlightened directors of our naval and military medical establishments the expediency of enabling every medical officer, before entering on active duty, to profit by the attendance on a course of lectures specially dedicated to this and other subjects of naval and military Hygiène. If, for this purpose, a special professorship

were necessary,—and here both branches of the public service might be combined,—we are sure the public funds could not be more beneficially appropriated than in its establishment: and we doubt if the proposed office could be better filled than by the enlightened author of the paper now before us.

Edinburgh Journal. October, 1837.

Of the Probability of Death and Recovery in Asiatic Cholera.

By WM. FARR, Esq.

[THE following extract is from a very valuable paper on *Prognosis*, which, like all Mr. Farr's productions, exhibits a profound acquaintance with the more recondite and philosophical parts of Medical Statistics. Our limits prevent us from laying more of this article before our readers; but we regret this the less, as we presume the small work in which it appears will be seen by most of them. We can conscientiously recommend the British Medical Almanack to their notice, as containing an infinity of matter of the greatest interest and importance; and we consider the profession under great obligations to Mr. Farr for the manner in which he continues to conduct it.]

Table of 4907 fatal Cases of Cholera, shewing the Number remaining alive at each of 16 Periods; and the Number dying in the Period following.

Hour.	To die.	Dying.*	Day.	To die.	Dying.	Day.	To die.	Dying.
0	4907	204	1	2523	823	7	372	171
6	4703	615	2	1700	502	8	201	35
12	4088	392	3	1198	382	9	166	36
18	3696	1173	4	816	240	10	130	111
			5	576	125	15	19	19
			6	451	79	20	0	

The tendency to speak in weeks, and well-known periods, produced the irregularity at the 7th day. Of the 171 thrown on that day, some died a day or two before, some a day or two afterwards. For the same reason it may be safely admitted that the deaths increased regularly on the first day. The daily rate of mortality in the first 12 hours was 16 per cent.; in the next 12 hours (12—24) 37 per cent.; in the 2d day, 11 per cent.; in the 3d day, 8 per cent., if the mortality of cases of cholera in Paris was 49 per cent.—it could not have been higher,—and none of the severe cases were cured in the first 3 days. The force of mortality attained its maximum in cholera by the 21st hour (18—24 hours); the maximum intensity in small-pox is attained in days 10—15; in phthisis, in 6—9 months. Taking a year as the unity of time, the relative maximum force of mortality—the deaths out of 100 constantly living,—in the height of these three diseases is, Cholera, 13614; Small-pox, 1150; Phthisis, 148. The danger of cholera decreases as the time advances; the longer a cholera patient lives, the more likely he is to live. The way in which the prognosis becomes favorable is shewn in the following table:

Table of the Probability of Recovery from the severer Attacks of Cholera at the end of 12 hours, and 1, 2, and 3 days.

Cases.	To Recover.	To Die.	Probability of Recovery.
0 hours 10000	5093	4907	.509 nearly 1 to 1
12 9181	5093	4088	.555 1.3 ... 1
1 day 7616	5093	2523	.669 2 ... 1
2 6793	5093	1700	.750 3 ... 1
3 6291	5093	1198	.809 4 ... 1

These facts prove that, in cholera, the probability is generally not in favour of death; they also establish the importance of early treatment, for half the deaths happen in the 24 hours. What the practitioner does, he should do quickly.

British Medical Almanack for 1838.

* Rapport sur la Marche et les Effets du Cholera Morbus dans Paris et le departement de la Seine.

PART FIFTH.

Medical Intelligence.PROGRESS OF THE ANATOMY AND PHYSIOLOGY OF THE
NERVOUS SYSTEM DURING THE YEAR 1836.

BY PROFESSOR MÜLLER, OF BERLIN.

ENQUIRIES into the minute structure of the nerves form an unusually large proportion of the scientific communications of the past year. Besides Ehrenberg's* excellent work, we have a treatise by Valentin† full of interesting observations on the ganglia and on the terminations of the nerves. Emmert‡ has traced the distribution of the extremities of the nerves in the muscles. Gottsches§ and Treviranus¶ have examined the structure of the retina, and Treviranus that of many other parts of the nervous system. Volkmann|| and Langenbeck** have directed their attention to these and similar objects, and Kronenberg†† has investigated the structure of the plexuses. These labours, as well as those of Remak,‡‡ have for their object the rich and still unexhausted topic of the minute anatomy of the nervous fibre.

The distinction of the nerves according to their fibres becomes more and more difficult. Treviranus found the fibres, in the recent subject, in the brain as well as in the nerves, for the most part straight and uniform; Volkmann, that the varicose fibres in the nerves of sense (the optic nerve) are not constant; whilst Remak's observations prove that it is impossible to classify the nerves according to their varicose or cylindrical form, as single varicose fibres were seen more or less frequently in the most different nerves. And to denominate those fibres organic, which are to be found distributed alike in the nerves of sense and motion, is forbidden by their great changeableness and the impossibility which exists to draw exact limits between the cylindrical and varicose fibres: for one and the same fibre is often alternately cylindrical and varicose, and the nervous fibres in young animals are generally more inclined to this latter form. But even the existence of varices in nervous fibres which are not exposed to any disturbing cause is become altogether a matter of doubt. Treviranus was the first who expressed this doubt. The articulated form of the fibres of the brain is, no doubt, an observed fact; but this form is produced, according to Treviranus, by the influence of air and water; according to Valentin, by tension; and, according to E. H. Weber, by water and pressure combined. Weber, therefore, examined the nervous fibres uninfluenced by these disturbing causes, by merely moistening them with albumen. He then

* Ehrenberg in *Abhandlungen der K. Akademie der Wissenschaften zu Berlin* aus d. J. 1834.—Berlin, 1836, p. 605.

† Valentin über den Verlauf und die letzten enden der Nerven. *Nov. act. nat.* Vol. xviii. p. 1. p. 51.

‡ Emmert über die Endigungsweise der Nerven in den Muskeln.—Bern, 1836. 4.

§ Pfaff's, *Mittheilungen aus dem Gebiet der Medicin, Chirurgie, und Pharmacie.* Neue Folge.—Altona. Heft 3. 4. Heft 5. 6. (See *Brit. and For. Med. Rev.*, Vol. iv. 499.)

¶ Treviranus, *Beiträge zur Aufklärung des organischen Lebens.*—Bremen II.

|| Volkmann, *Beiträge zur Physiologie des Gesichtssinns.*—Leipzig 1836. 8.

** Langenbeck de Retina *Observationes Anatomico-pathologicae.*—Götting., 1836. 4.

†† *Plexuum Nervorum Structura et Virtutes.*—Berolini, 1836. 8.

‡‡ Müll. arch. p. 145.

found, in common with Valentin, that, when he was able to isolate thin lamellæ of the brain, as, for instance, the delicate layers of the *valvula cerebri*, without pressure or stretching, the fibres had the appearance of nearly uniform tubes; and both observers agree in ascribing this structure to the fibres of the brain. There seems, indeed, to be no longer any doubt that the fibres of the brain and of the nerves, examined in a recent state, and independent of all disturbing causes, approach mostly to the cylindrical form, though they are not everywhere perfectly uniform, or free from slight irregularities. It is always, however, a characteristic of the fibres of the brain and of the nerves of sense, that they very readily assume this form, which is the case with no other tissue: this character, therefore, cannot be omitted from a definition of these nerves, and may be usefully applied in the discrimination of doubtful cases. It is not exactly made out on what this property discovered by Ehrenberg depends; the cause is probably to be found in the unequal cohesion of the tubes or of their contents. According to E. H. Weber's observations, unevenness and bulging out of the sheath which surrounds the soft contents, (a protrusion which occurs more on one side than on the other,) arise under the eye of the observer during protracted examination. As far as the contents of these tubes are concerned, they are, according to Valentin, perfectly clear, transparent, oily, and without any trace of globules, and the fibres, with the exception of a different degree of softness in the walls in the central parts, are throughout identical in the centre and in the periphery of the nervous system. This point, namely the proportion which the sides of the tubes bear to their contents, with the discrimination of which the modern minute anatomy of the nervous fibre began, is still one of the most difficult subjects of investigation. Valentin does not express himself everywhere with equal confidence on this question. According to him, (p. 165,) the substance of the primitive fibres is everywhere a half-fluid, slightly adhesive and transparent oily substance, which, in consequence of its refractive power, shows, when isolated, a finer internal line parallel to its superficies, without being, for this reason, divisible into a tube and its contents; when isolated it assumes either the globular form of all fluids or at least that of a flattened sphere. The contents, however, (p. 92,) may readily be pressed out of the varicose fibres, so that the mere sheath remains. At p. 70, he describes the expressed contents of the tubes of the nervous fibres as a grumous mass, consisting partly of divided and curved threads, and partly of isolated irregular globules, between which again are other globules isolated, oily, perfectly transparent, regularly or irregularly swollen, and exceeding for the most part in thickness the thread-like tissue of the brain and spinal marrow.

Whether or not the minute elementary structure of the nerves is known, must still remain a matter of doubt. These large cylinders, which are called primitive fibres, greatly exceed in strength the elements of other tissues. Schwann saw, in fibres of the thickness of the primitive fibres of the mesentery of a frog, still finer fibres running out of them. Treviranus discovered stripes running in the length of the cylindrical tubes, he even saw distinctly smaller elementary cylinders in the primitive fibres, the first having a diameter of 0,0013 millimètres, the latter of 0,0053 in a spinal nerve of the Crucian (*Cyprinus Carassius*, Linn.) In rabbits the elementary cylinders were 0,0016 in diameter, and the stronger cylinders enclosing these, and called the primitive fibres, 0,0099. In his latest observations Remak observed the contents of the nervous tubes to be a somewhat smaller, flat, and quite solid thread, or a flat pale fillet which could be isolated from the easily wrinkled tube in large pieces by pressure. He did not succeed in detecting a more minute fibrous structure in this fillet, although, in some cases, it could be split into two or more fibres.

Much also still remains to be observed in the minute anatomy of the primitive fibres of the central parts, before general physiological conclusions can be formed. In some animals there are striking exceptions, as, for instance, in the spinal marrow of the *Cyclostomata*. The spinal marrow of the *Petromyzon* is generally known to be extensible; it can easily be torn into threads, which may be regularly pulled off in the direction of its length. In the examination of these parts I find that, in the spinal marrow of the *Petromyzon marinus*, there are different kinds of fibres,

consisting of flat, thin, band-like threads, as broad as the primitive fibres of the nerves of the ox. I have never seen such bands in the spinal marrow of any other animal. These flat bands exist in all parts of the spinal marrow, their edges are parallel and without swellings of any kind, and they are pale, transparent, not divided into a tube and contents, nor can any minute structure be discovered in them: they do not join together, nor do they give off branches. Besides these, there are somewhat finer threads, of which it is difficult to say whether they also are flat; and there is moreover a peculiarly fine fibrous structure, consisting of other fibres which are greatly more minute even than those already mentioned.

Concerning the distribution of the nervous fibres in apparent anastomoses and plexuses, Kronenberg's communications are very complete. His researches give additional confirmation to the opinion that the primitive fibres of the cerebral nerves continue separate up to their ultimate distribution, that they merely change from one bundle to another, a change which takes place not only in the plexus but in every part of the nervous trunks and branches. No one has hitherto studied with so much perseverance as Kronenberg the comportment of the fibres in the several plexuses: the researches which this author has made into the minute structure of the brachial plexus of the mammalia and of the lumbar plexus of the frog form a very good foundation for physiological experiments. We shall again refer to the physiological part of these researches.

The ultimate distribution of the nerves has been illustrated by the labours of Treviranus, Gottsche, Valentin, and Emmert. Although Schwann's investigations were less favorable to a loop-like termination of the primitive fibres in corresponding ones distributed to the muscles, and Treviranus stated that he had observed a sudden termination of the primitive fibres in the muscles without minuter subdivisions, the observations of Valentin and Emmert tally with the opinions of Prevost and Dumas, who had not themselves examined the primitive fibres. The same distribution has been observed by Valentin in the iris and ciliary ligament, and even in parts exclusively sensible, as in the interior of the cochlea of birds, in the ampullæ, in the sacs of the teeth, and in the skin of the frog. Breschet saw this distribution in the cochlea and ampullæ, and in his earlier examinations of the papillæ of the skin. Burdach, junior, also hit upon the same loop-like union of two fibres, as stated in a written communication to me on the examination of the nerves in the skin of a frog, and he saw these loops formed by the fibres of different branches of the nerves. It would be very difficult at present to form any conclusion from these observations respecting further propositions in the physiology of the nerves, as every such conclusion must rest upon the hypothesis that the so-called primitive fibres are really the minutest parts of the nerves. Even if this were the case, we should still hesitate to consider the loop-like union of two primitive fibres with each other as a general fact, as regards the nerves of sensation; for this does not occur in the retina of vertebrated animals and of insects, in both of which the nervous fibres continue separate to their termination. As to the organ of hearing, I have lately convinced myself that it is just as little the case in the lamina spiralis, the most important part of the cochlea of birds. The largest portion of the nerve of the cochlea lies upon the edge of the cartilage of the cochlea and distributes its branches very regularly to its substance. A number of minute fibrillæ arise from the cartilage and pass transversely close together and in parallel lines through the greater part of the breadth of the lamina spiralis. These transparent fibrillæ are much more minute than the primitive fibres of the nerves, and end indistinctly without coalescing. In all probability they are the continuations of the primitive fibres of the nerve of the cochlea which pass through the cartilage and are here divided to the utmost degree of minuteness. The difference between these fibrillæ and the darker nervous fibres before they enter upon the cartilage may perhaps depend upon the absence of their sheaths and tubes. The lamina spiralis of the cochlea of birds, first discovered by Windischmann, is undoubtedly the most important part of the cochlea; and I cannot therefore agree with Huschke in considering the arched and wrinkled vascular membrane, falsely called the membrane of hearing (*Gehörblätter*) as such. Breschet also agrees with what has been stated on this subject. The nerves of the cochlea are not distributed to the

lining membrane (Gewolbten Haut); the sac (Flasche), on the other hand, receives numerous nerves, but I have never succeeded in tracing the loops of the primitive fibres, and observed merely plexuses consisting of bundles of minute fibrillæ.

In reference to the present state of the physiology of the nerves, the question whether or not the fibres of the nerves of sensation terminate by loops is of no moment. The conclusions to which we have arrived depend, in no degree, upon this alternative; and, as it is certain that the truncated ends of a divided nerve are as capable of sensation as the extremities of the nerves themselves, it is also clear *à priori* that if the loop-like connexions of the extremities of the fibres of the nerves of sensation do exist, it can make no difference, as, after the removal of these loops by dividing the nerves, the result remains the same. Treviranus found a papillary termination of the nervous fibres not merely in the retina, but also a similar distribution in the auditory and olfactory nerves. In these latter nerves the papillæ are more thread-like. He examined the auditory nerve in the lamina spiralis of the cochlea of young mice, and found the bony part entirely covered by thread-like papillæ, lying close the one on the other. The cylindrical fibres of the nerve pass more disunited underneath the lining membrane of the cochlea to the membranous edge of the lamina spiralis, and after making numerous gyrations in the canals which contain them, pass through small openings and make their appearance externally as minute round globules, having a diameter of from 0,0016-0,0033 millimètres. The cylindrical fibres of the auditory nerve itself had the same diameter. Treviranus found that, in the fox, the nerves of the semicircular canals, at their entrance into the ampullæ, expand on each side into a sheet in which the cylindrical fibres subdivide into still more minute cylinders, and again reunite to form still larger cylindrical fibres. The line of this reunion is marked by a dark ring, which consists of very minute cylinders. In a turkey-cock the extremities of the nerves had the appearance of bundles of extremely minute cylinders, and seemed to spread themselves over the internal surface of the ampullæ. In a bream, the nerves, which, before their entrance into the ampullæ, had a diameter of 0,0066 millimètres, terminated after their entrance in cylinders of 0,0010-0,0015 millimètres. Gottsche found the extremities of the nerves of the cochlea in hares and rabbits, and of the acoustic nerve in fish (*Platessa borealis*) club-like. In many fish, for instance in the sturgeon, the auditory nerve divides into threads which appear truncated; but in the plaice it forms, according to Gottsche, swellings which are twice as thick as the fibres themselves: these swellings contain in their interior a cavity, and the nervous fibres themselves exhibit a complete canal. In hares the nervous cylinders terminate upon the cochlea by egg-shaped knobs, which are distinctly raised above the internal membrane of the cochlea, like flower-buds. Treviranus found the papillæ of the olfactory nerve of mice thread-like and elongated; in the mouse they lie close to each other, but in the hedgehog they are more isolated. Their diameter is from 0,003 to 0,005 millimètres. Upon the lining membrane of the nose of the turkey-cock, turtle, and bream, he saw merely the blunt extremity of the minute cylinder, without any projection.

Enquiries into the minute anatomy of the retina have yielded a great deal of information. B. C. R. Langenbeck has been able to distinguish in the retina, 1, an external granular coat, (the cortical layer;) 2, Ehrenberg's filamentous nervous coat; and 3, a vascular coat, consisting of blood-vessels, which are united by much cellular substance into a delicate membrane. He found the fibres in the nervous coat, as in the optic nerve, varicose. Volkmann also made the same observation, but states that the varicose appearance is not constant, the swellings being at times entirely absent. These discrepancies no doubt partly depend upon the time and manner of observing. In recent eyes of mammalia and fish, I saw the fibres in the optic nerve as well as in the nervous membrane without swelling of any kind, and much more minute than they appeared when examined later. In the brain of different animals also I saw the fibres, when not submitted to pressure, very frequently at least, with almost straight outlines, and when pressed upon swollen and irregular. In the situation of the *macula flava* of the retina, Langenbeck observed that the granular layer terminated only by a sharp edge; where the retina borders upon the ciliary ligament, the granular layer of the retina also terminated, and the

fibrous coat becoming much thinner formed what our author calls the ciliary part of the retina; this part lies upon the ciliary ligament, overlaps the posterior edges of the ciliary processes, is continued to their anterior edges, and terminates at the junction of the corpus ciliare with the uvea. This prolongation is said to contain small articulated tubes. Langenbeck saw the vessels of the retina in connexion with those of the ciliary ligament in the fœtus of the pig. Treviranus does not recognize any *ciliary part* of the retina. At some little distance from the rounded edge of the retina he could distinguish neither the vascular membrane nor Jacob's membrane. Near this edge, however, both its surfaces were covered with delicate membranes, of which the external was homogeneous and the internal highly vascular. Both of these membranes were prolonged over the edge of the nervous coat, they then lay close together, were formed into long folds, and extended to the ciliary ligament. The most careful examination detected no nervous matter between these membranes.*

The exact relation which the coats of the retina bore to one another had hitherto remained unknown; we therefore stated some years since that its exact structure seemed problematical. Treviranus has made a great step towards the elucidation of this subject; and his discovery, published a little before his death, was the last benefit he conferred on science. The chief part of this discovery is shortly expressed in the author's own words. The optic nerve divides into nervous cylinders, which radiate upon the external surface of the retina. Each separate cylinder, or each bundle of cylinders, deviates at a certain point from the horizontal direction, and turns towards the opposite internal surface of the retina. Soon after it has changed its direction it passes through openings in a vascular network which springs from the central vein of the optic nerve. Before it reaches the internal surface of the retina it penetrates a second vascular network derived from the last branches of the central artery. After it has passed through this last it is covered by a sheath-like continuation of the vascular membrane of the retina, and ends behind the vitreous humour in the form of a papilla. The transverse diameter of the cylinder was, in the hedge-hog 0,001 millimètres; in the rabbit 0,0033, and in birds 0,002-0,004; in the frog the cylinders had a diameter of 0,0044 millimètres, the papillæ of 0,0066, and the latter in the *Crucian* of 0,0039-0,0040 millimètres. Ehrenberg recognized also in the retina of frogs and fishes staff-shaped bodies, whose connexion with the nerves seemed indistinct; he saw also club-like bodies in the Schneiderian membrane of the nose. Gottsche's observations contain many interesting details in illustration of the discovery of Treviranus. The so-called granular membrane of former writers, concerning which so many different opinions have been broached, is not found in recent eyes. If we observe a perfectly recent retina, we see, according to Gottsche, an appearance similar to a thatched roof, and small distinct nervous cylinders project out of it; if we let this remain upon the glass and moisten it from time to time, we see distinct granules; the eye of a fish which has been dead from three to four hours has the same appearance. We agree perfectly in this remark. Upon the internal surface of the retina of recently killed mammalia, frogs, or fishes, we distinctly perceive the staff-like cylinders projecting from the surface, presenting an appearance like a thatched roof. Sometimes we see these cylinders externally, as Gottsche also states. Michaelis thinks that they are merely seen through the membrane, an opinion with which I fully agree. The little cylinders are easily broken off, and swim about in a detached form; when separated they are much longer than broad: in frogs I have often seen them of unequal lengths, and some of these are slightly bent.

Volkman and E. H. Weber have confirmed the statements of Treviranus in respect to mammalia: they saw the fibres always tortuous, but without papillæ, which indeed are not present in mammalia, but merely the staff-like bodies. The fibres of the retina were obviously free from swelling, but they saw some varicose fibres (in rabbits) which were not produced by water, for they employed the aqueous humour and avoided using pressure in their observations. The swollen fibres

* Here follows an abridgment of Gottsche's dissection of the retina given in our last number.

were most visible in the brush-like expansion of the optic nerve, and were situated by the side of and between the uniform fibres. In frogs both these observers saw all the fibres at first uniform and free from enlargements.

I make the following extract from a treatise on the structure of the retina which Dr. Michaelis has communicated to me in manuscript, and which is about to appear in the *Nov. Act. Net. Cur.*

According to Michaelis the retina extends only to the ciliary ligament, for so far only does the granular membrane and the expansion of the optic nerve proceed. He found the structure most distinct in a young owl. In this case the thick, defined, projecting border of the ciliary ligament overlaps the edge of the retina, whilst half the thickness of the retina lies above, and the other half below this border. On the other hand, in the pig, the retina, prominent and abruptly cut off, lies over the ciliary ligament which firmly attaches itself by a defined margin to the inner surface of the retina. In the calf, again, the retina becomes thin at its edge and attaches itself to the ciliary ligament by a sharp border. In man the ciliary ligament projects over the retina externally by a zigzag edge, and under this covering the retina first becomes thin.

Michaelis describes four layers in the retina: an external serous layer, a granular layer, a nervous and vascular layer, and lastly an internal serous layer. The external layer is the *membrana Jacobi*: in mammalia this membrane shows under the microscope only slight dimples; in the heron they appear in regular and clear divisions sprinkled with small blood-red globules, having a diameter of $\frac{1}{300}$ of a line, and between these there are still other globules more minute and of a citron-yellow colour: these globules adhere only to the external surface. In owlets they are of a bright yellow colour. The second or granular coat is the thickest membrane of the retina, and exhibits both on its external and internal surface a globular formation; when fresh it is as clear as water, but after death and under the influence of chemical agents it becomes opaque, whilst the nervous membrane remains transparent both in spirit and water. When divided, the cut surface seems to consist of cylinders standing upright, closely packed, and bearing each upon the extremity which is in contact with the nervous coat a small globule. It appears to be the substratum of the nervous threads which run upon it; in the situation of the so-called *foramen centrale* it becomes thin and has the appearance of a mere granular coat. The fasciculi of the nervous coat are best shewn by means of spirit of creosote. Our author states the thickness of the fibres at $\frac{1}{2500}$ of a line. These fibres have very slight and infrequent swellings; in fish they are quite uniform. The trunks of the vessels lie internal to the nervous coat. The serous or innermost coat is seen by maceration in dilute sulphuric acid. On the surface which is in contact with the retina hang a number of minute globules having a diameter of $\frac{1}{1500}$ lines, and separated from each other by tolerably regular intervals of $\frac{1}{100}$ to $\frac{1}{50}$ lines. Most of these little globules are furnished with fine threads of different lengths which resemble the primitive fibres of the nerves. Michaelis considers these threads as the terminations of the nerves. The serous membrane always remains attached to the retina after the separation of the vitreous humour. Our author has not been able to ascertain the exact connexion of the *tunica Jacobi* and of the internal serous membrane of the retina with the ciliary ligament. The thickness of the retina gradually decreases towards its anterior edge, where the primitive fibres are separate and do not touch each other.

Michaelis has made many observations on the *macula lutea*; at this spot the granular membrane is very thin, but begins to increase in thickness at $\frac{1}{10}$ line from its centre, and obtains its greatest thickness at a distance of $\frac{1}{2}$ line. This strong ring or ridge around the *foramen centrale*, which foramen is formed by a single layer of little globules, is the *macula lutea*. The fibres of the retina have a peculiar arrangement around this point; for whilst, in other situations, the nerves radiate in straight lines from the optic nerves, around the *macula lutea* they are arranged in the form of arches, of which one part meet in the so-called *foramen centrale*, the next in succession converge in a regular manner from each side towards a line which stretches from the *macula lutea*. Between this spot and the optic nerve the nervous fibres are less numerous, more or less straight, and perfectly

distinct the one from the other. The thin transparent spot, which has obtained the name of *foramen centrale*, is not quite round, but star-shaped; in young subjects it is elongated.

Although the modern investigations into the structure of the nerves are highly important, many things still remain to be determined. The termination of each separate fibre of the fibrous layer in a staff-like body seems still rather a postulate than an ascertained fact. For it is of the first importance to the physiology of vision to know what proportion the number and size of the primitive fibres of the optic nerve bear to the number and size of the terminations of the nerves in the nervous coat, or to the number and thickness of the staff-like bodies. If every nervous extremity corresponded to a fibre of the optic nerve, the thickness of the retina ought to progressively diminish from the point of entrance of the optic nerve to the border of the ciliary ligament, independently of the varying thickness of the other coats of the retina. The observations of Gottsche favour this supposition. It is not easy, however, to understand how so many fibres as are necessary to furnish the staff-like bodies can be compressed into the narrow compass of the optic nerve. (Compare J. Müller's *Physiology*, i. p. 688, and Volkmann, a. a. O. p. 104.) At all events, the fibres of the optic nerves and the nervous fibrillæ of the fibrous coat of the retina are much more minute than the strong cylinders of other nerves. As far as I have been able to compare the optic nerve of rabbits, and its minute fibrillæ distributed to the fibrous coat of the retina, with the cylindrical tubes of other nerves of the same animal, I should say that the union of many fibrillæ of the retina or of the optic nerve would be necessary to occupy as much space as the primitive cylinders of any other nerve; and the extraordinary minuteness of these fibrillæ clearly shows how small must be the staff-like bodies in the retina of the rabbit. Wagner also (*Burdach's Physiology*, v. p. 143,) found the nervous fibres of the retina of the ox very minute, $\frac{1}{800}$ - $\frac{1}{900}$ lines, whilst the cylinders of the ciliary nerve of the same animal ranged from $\frac{1}{150}$ - $\frac{1}{300}$ lines. The primitive fibres are most easily examined in the optic nerve of fish. I have never seen any fibres so minute as in the optic nerve and retina of rabbits, and in the optic nerve of the cyprinus, (probably there are varieties here.) It is difficult to understand how the fibres, pressed into the small compass of the optic nerve, should suffice for the formation of so large a surface as that of the retina, if this surface be formed, like mosaic work, by the extremities of the nerves. But if we assume that the staff-like bodies are situated in rows upon the sides of the primitive fibres, so that a great number of them are attached to one fibre, such a supposition is at variance with the theory of sensations; for the sensation of any one part does not appear to take place in the length of the fibres of other nerves, but depends upon the number of fibres, of which each has only one origin in the brain. If the sensation of different parts took place in aliquot parts of the length of the fibres, we must suppose the mind to be present in the entire body, and in every minute part of the whole length of a fibre: with such a supposition the sensations felt by those who have lost a limb by amputation are at variance. But if sensation be conveyed to the brain only from the extremities of the nerves, a fibre can refer every change which takes place in aliquot parts of its length to one spot only. For it would be absurd to suppose that the sensation of a part depends on the different distances which the nervous principle has to travel from the various points of a fibre to the brain. If, on the other hand, we consider the superior nerves of sensation as different from the other nerves, and as participating more closely in the operations of the mind, so that this is actively percipient in the extremities of the nerves of the retina, the difficulty of supposing the fibrillæ of the optic nerve sufficient to form the entire *mosaic* of the retina is at an end; but other difficulties suggest themselves, into which we cannot enquire further at present. This state of the question, which we have long considered, has determined us to mention merely those facts in the physiology of the nerves which are clearly established, conscious how many things yet remain unexplained and contradictory in the still unfinished theory of sensation.

The observations on the decussation of the optic nerves refer merely to the general course of the fibres, in respect to their crossing or not crossing. Whether

there were transverse unions or divisions could be ascertained only by minute dissection. In the decussation of the optic nerves Volkmann saw no divisions of the fibres. I also looked for such divisions in vain, even when the crossing of the crossing parts was very perceptible. Treviranus says that many of the internal fibres appear to come in an arched form from both sides, and to anastomose with each other; but that these connexions, when they take place, never extend to the primitive fibres. He also saw them cross one another without uniting.

The terminations of the fibres in the brain have been examined by Valentin. The primitive fibres of the nerves which join the spinal marrow do not terminate there, but pass on to the brain. The primitive fibres which unite with the extremity of the spinal marrow run forwards; those, on the other hand, which come literally from the higher nerves go first transversely inwards to or near the cineritious substance, and then pass on in a longitudinal direction to the brain. In the true medullary substance the fibres lie side by side, but where the medullary and cineritious substances touch each other they receive between them the globules of the cineritious substance, which we shall presently mention, and radiate through the cortical substance. Here they form loop-like terminations, as in the peripheral extremities of the nerves. These fibres are most distinctly seen at the union of the medullary and cineritious substance, or in the yellow substance at the periphery of the hemispheres of the cerebrum and cerebellum. In examining the latter, this appearance has presented itself here and there. The existence of different systems of fibres on the surface of the cerebrum, those namely of the crura cerebri and commissures, and especially of the corpus callosum, and the presence of three distinct sets of fibres in the cerebellum, renders it doubtful to which system the different loops of fibres belong. These loops do not appear to me to be exactly traceable to the continued nervous fibres; and we must give up the idea of a complete and continuous circle of the nervous fibres, to which even the observations of E. H. Weber concerning the origin of the nervus trochlearis in the central line of the valvula cerebri are not favorable.

The investigations also of the ganglia, and of the grey substance of the central parts of the nervous system, have been very fruitful in results. In the interior of the ganglia of the non-vertebrated animals (leech, common snail,) Ehrenberg observed club-like bodies. In the ganglia of the leech these clubs form eight bundles, of which each pair pass through long tubes into the four roots of the ganglion. These clubs contain in their swollen parts a nucleus, and in the leech, in addition to these, numerous small globules. Valentin has described similar bodies in the abdominal ganglia of the leech. He saw globules which, like those of the ganglia of the higher animals, contained a nucleus. On a spot near the surface of this nucleus is placed a larger and redder body, and sometimes many swollen ones.—*Jahresbericht über die Fortschritte der anatomisch-physiologischen Wissenschaften im Jahre 1836.*—*Archiv. für Anat. Jahrg. 1837.* Heft. iii.

REPORT ON THE RADICAL CURE OF HERNIA BY MEANS OF TRUSSES WITH SOLID BLOCKS, (PADS.) READ BEFORE THE PHILADELPHIA MEDICAL SOCIETY, AND ORDERED TO BE PRINTED, APRIL 29TH, 1837.

BY REYNELL COATES, M.D., AND ISAAC PARRISH, M.D.

[In a former Number of this Journal, (Vol. III. p. 234,) we gave some account of the new method of treating hernia by means of trusses with solid wooden pads, introduced some years since in America. The present Report is the production of two experienced and eminent surgeons, and is founded on the continued observations and enquiries conducted by them since their appointment as a committee of examination in December, 1834. The practical results announced in it are of the highest importance; and, as they are based on actual observation, and are supported by the detail of cases, no doubt can be entertained of their authenticity; so that we seem, at last, justified in asserting that a radical cure for chronic hernia has been discovered in the American truss. The following extracts contain the more important facts in the Report. Of the six instruments invented by Dr. Chase

we shall give figures and a detailed account of three only; but the principle and general plan of the whole will be sufficiently evident from our extracts.]

The trusses with solid blocks, now in use or recommended by inventors, may be divided into two classes. 1st. Those which are constructed for the express purpose of producing irritation, in order to effect a condensation of the skin, cellular tissue, and the fascia superficialis or the abdominal tendons about the hernial orifice, into one common mass by adhesion. 2d. Those which are designed to secure the constant, perfect, and safe retention of the bowel, without the attempt to create intentional irritation in the parts pressed by the instrument. The first class includes the truss of Stagner, and the various apparatus of Dr. Hood for the treatment of common inguinal, ventro-inguinal, femoral, and umbilical hernia.

The second class embraces all the instruments invented by Dr. Chase, which are five in number.

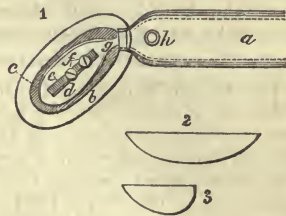
From the result of all the evidence presented to them, and their reasonings upon it, the committee are irresistibly drawn to the following conclusions. 1st. That the trusses of the first class do not secure the complete and permanent retention of the bowel with all the certainty which may be obtained by mechanical means. 2d. That although it is extremely probable that radical cures may be occasionally effected by the use of such instruments, it has not been proved that the success following their employment exceeds that which has been obtained by the better kinds of trusses previously in use. 3d. That the action of these instruments is often attended with serious and unnecessary inconvenience, uneasiness, and pain. Lastly, That their employment for too long a time, when the degree of pressure exerted by them is considerable, sometimes produces absorption of the tendons, dilatation of the hernial orifice, and an extension of the evils they are designed to remove; and that any attempt to obviate this danger, by lessening the pressure while the support of the instruments continues to be required, will diminish the security of the retention. For all which reasons the committee do not feel warranted in making a favorable report on the claims of this class of trusses upon the confidence of the society.

The object of the instruments of Dr. Chase is to secure the perfect and permanent retention of the viscera in hernia, in order to permit the powers of nature to effect a radical cure after the removal of the misplaced parts, which are supposed to offer the greatest obstacle to her success.

The inventions and improvements of Dr. Chase extend to all parts of the truss and its appendages, and his attention to minute but highly important details has been carried to an extent never equalled by any of his predecessors in this branch of surgery. The complete instruments employed by him are—1st. The Inguinal or Common Inguinal Truss. 2d. The Ventro-Inguinal Truss. 3d. The Femoral Truss. 4th. The Umbilical Truss. 5th. The Umbilical Belt. 6th. The Double Truss.

I. Of Inguinal or Common Inguinal Truss.

Fig. 1. *a.* The extremity of the main-spring of the truss. *b.* The block. *c.* The brass block-rider: the screws by which it is attached being covered by the block-slide. *d.* The block-slide. *e.* The window in the block-slide. *f.* The two broad-headed screws of the block-adjustment, securing the rider to the slide, and, when loosened, sliding freely in the window. *g.* The soft iron flexible neck, attaching the block-slide to the main-spring. *h.* The button for the pelvic strap, which is generally used for the perineal strap also.



The proper perineal strap button on the end of the block-slide is omitted in this and some succeeding figures, to prevent confusion.

Fig. 2. Longitudinal section of the block.

Fig. 3. Transverse section of the same.

Of the Block. The block of this truss was warmly approved in the Preliminary

Report, and it has amply maintained its character throughout the more recent investigations: it is so perfectly adapted to the form of the parts interested in common inguinal hernia that the committee are unable to perceive in what manner it could be improved; nor has it ever failed, under their observation, in retaining the bowel both permanently and completely during the time of its employment, after the first few days required for the accurate adjustment of the instrument. Nothing farther appears necessary to prove the decided superiority of this block over all others known to the profession, in the particular form of hernia for which it is designed.

Of the Block-attachment. Two very important improvements upon the old modes of attaching the pad to the spring of the truss are observable in the block-attachment of the inguinal truss. The block is surmounted by a thin oval plate of brass, termed by the inventor a *block-rider*; and this is adapted to the under surface of an iron plate of nearly similar form, called the block-slide, to which it is attached by means of two round-headed screws, playing freely, when loosened a little, in a longitudinal fenestrum in the block-slide, so as to admit of any required change of the position of the block in this direction, to the extent of about an inch in the trusses designed for adults. The block-slide is connected to the spring by means of a round neck of soft iron, about three-quarters of an inch in length, sufficiently stiff to resist any change of shape during the most active movements of the patient, and sufficiently pliable to act like a universal joint under the hands of the surgeon. The combined action of the slide and the neck enables us to adjust the block with the utmost precision to the edge of Poupart's ligament, the rout of the abdominal canal, and the internal ring, whatever may be the peculiar form of the abdomen of the patient, while the block remains invariably in the exact position chosen by the surgeon; advantages possessed by none of the trusses previously in use, so far as they are known to the committee. These improvements are, in themselves, sufficient to add very greatly to the value of the instrument.

Of the Spring and Strap attachment. The endless varieties of form which have been given to the springs of trusses, render it apparently impossible that any thing intrinsically novel, in this part of the hernial apparatus, should be presented to the public hereafter; but it is of the utmost importance that the profession should determine what class of springs are calculated to give the greatest degree of security and permanency to the action of trusses.

This subject has been amply discussed in the work of Dr. Chase, and the committee are prepared, after due reflection, to coincide in the opinion expressed by that gentleman, that the semi-circular steel springs of Salmon and Ody are objectionable, because they are brought into accurate relation with the body only at the spots corresponding with the spine and the hernial orifice; the whole arch of the spring resting loosely over the side of the pelvis without a fixed location, and remaining liable to continual change of place from the movements of the glutei muscles and the reaction of the dress of the patient. The changes just mentioned must inevitably lead to the danger of corresponding changes in the position of the pads or blocks, and consequent insecurity of retention. The motives for the invention of this class of springs were the three following, and they are obviously fallacious. 1st. It was supposed that the pressure of the spiral elastic springs, being exerted throughout their whole length, renders them liable to derangement by the motions of the parts on which they press: but, excepting on the front of the hypogastric region of the abdomen, those parts have so slight a degree of mobility—based as they are upon the solid structure of the pelvis, and almost uninfluenced by muscular contractions, that their alterations of figure are of no real importance. The changes in the figure of the hypogastric region are fully compensated by the elasticity of the spiral springs, and those of the parts over the ring of the ilium are successfully counteracted by perineal straps, so that the accuracy and permanence of retention are not contravened when spiral springs are employed. 2d. It was supposed that the changes of shape in the hypogastric region required some mode of adjustment more complete than that effected by the elasticity of the main spring, to enable the pad or block to accommodate itself at all times to the form of the parts; and hence the ball-and-socket pad attachment, to which the semi-circular spring was deemed peculiarly adapted. But, if desirable, this mode of attachment

may be as readily employed in connexion with the spiral spring. Your committee do not deem it desirable; because the ball-and-socket attachment renders secure but one point on the back of the pad or block, while the circumference may be tilted in any direction by the pressure of an intestine from within, almost as readily as by the movements of the abdomen, to which the pad is designed to yield; for the soft and compressible surface of the hypogastric region cannot securely prevent this tilting when the adjustment of the pad is not remarkably accurate, or when the propulsive force of the intestine in hernia is considerable. A third argument urged in favour of the introduction of semi-circular springs was drawn from the tendency of the strap attached to the spiral spring trusses to draw upwards, and thus displace the pad; but this difficulty is completely removable by giving to the spiral spring and the accessory parts of the truss a proper form and disposition, as will be explained hereafter.

Your committee are therefore of opinion that Dr. Chase has done wisely in adopting the spiral spring, and retaining the strap so as to encircle the whole pelvis by the truss, in preference to the semicircular spring and universal joint of Salmon and Ody's instrument, and the modifications of the same by the late Dr. Hull, of New York, the Rev. Mr. Reid, of Georgia, &c.

Although there is nothing positively novel in this part of the inguinal truss of Chase, the inventor has established definite rules for the degree of temper and the extent of the various curvatures of the spring, and also for the position of the strap-button, which render it easy to adjust the instrument more securely and permanently in all cases than can be done when these points are left to the discretion of instrument makers. Experience has decided that there is an advantage in giving an elastic temper to all that portion of the spring which intervenes between the pad-attachment in front and the opposite sacro-iliac symphysis in rear, but that the portion extending from the latter point to the opposite side of the pelvis should be so far softened as to admit of adjustment by being permanently bent. Three inches of the hinder extremity are left ductile in all the trusses of the full size; and thus the necessity of making an instrument expressly for each individual case (the great difficulty in the employment of spiral springs entirely of tempered steel,) is completely obviated, without sacrificing the accuracy of the adjustment on the one hand, or its permanency on the other.

It has been customary to curve downward the anterior end of the spiral spring, so that, when the part which lies across the back is horizontal, the front extremity may approach more nearly toward the abdominal canal. In Chase's inguinal truss this curvature does not exceed three-fourths of an inch, and its commencement is found far back upon the costae ilii when the instrument is applied; so that the spring, in passing forward from that point, winds downward below the anterior superior spinous process without encroaching too much upon the bellies of the glutii muscles or disturbing the proper position of the spring and strap on the back part of the pelvis. Any further increase of this curvature is attended with inconvenience, by giving the direction of the strap too much obliquity, and disposing the instrument to tilt upward in front; and such increase is rendered altogether unnecessary by the soft iron neck of the pad-attachment. In the last three inches of the anterior end of the spring there is another curvature, resulting from a slight torsion of the axis of the generating curve of the spring, which brings the flat side of this part of the spring into more complete correspondence with the surface of the hypogastric region, a matter of much importance to the comfort of the patient, and one giving additional security to the position of the instrument.

It has been customary, almost invariably, with truss-makers, to place the strap-button upon the plate or expansion which supports the pad; but Dr. Chase has very wisely affixed it to the anterior end of the spring: by which means the obliquity of the strap is much diminished, and the pelvis is enclosed by the instrument in a direction approaching very nearly to the circle, the strap lying altogether above the level of the block-slide, and the disposition of the instrument to tilt or ride upwards being reduced almost to nothing.

The committee consider the establishment of a fixed model for the triple curva-

ture of the spiral spring, and the position of the strap-button, as a highly important recommendation to the instrument under notice.

Of the Appendages. The perineal strap is never wanting in the inguinal truss of Dr. Chase. It is attached behind by means of a sliding loop, through which pass the spring and cover. Before, it is commonly secured to the strap-button; but each instrument is also provided with another button, made expressly for the perineal strap. This is seated on the lower extremity of the block-slide, and may be used to give additional security and force to the action of the block when the lower part of the abdomen is very prominent and loaded with fat. The back-pad is a very important appendage to the truss, giving great certainty to the position of the instrument, by protecting from irritation the spinous processes of the sacrum, and filling the interval between the spring and the integuments along the median line on the back of the pelvis. Some very important improvements have been made in the construction and mode of attachment of this pad. It is formed of a simple circular disk of tin, about four inches in diameter, covered with soft buckskin, and lightly wadded. A broad sliding loop of leather suspends it on the spring and cover, so that its position may be adapted exactly to the size of the patient and other accidental circumstances. This perfectly free mobility of the back-pad is believed to be a novel arrangement, and one of high practical importance; for it is found that the parts about the back of the pelvis are so intolerant of even slight pressure, when very long continued, that the subcutaneous fat becomes absorbed and the skin irritated by the mildest back-pad, if it be worn in one invariable position for many months consecutively. This difficulty is entirely obviated by an occasional change of position, produced by sliding the pad a little toward one or the other side; a change that is not attended with any loss in the security of retention, and which is accomplished more readily by the arrangement just described than by any other known to the committee.

Having thus analysed the several parts of the inguinal truss of Dr. Chase, the committee feel bound honestly to state their conviction that this instrument surpasses all others known to them in the accuracy and permanency of its retentive power in common inguinal hernia; a conviction fully sustained by all their practical observations of the action of trusses. The instrument is worn with so much comfort, that patients generally relinquish it unwillingly, and sometimes *absolutely refuse so to do*, even when pronounced well by the surgeon.

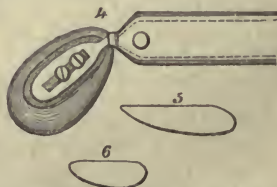
The committee find themselves unable to suggest any improvement, or to point out any defect of principle or construction in this truss as now employed by the inventor.

II. *Of the Vento-Inguinal Truss.*

Fig. 4. The attachment being in all respects similar to that in fig. 1, no references are required.

Fig. 5. Longitudinal section of the block.

Fig. 6. Transverse section.



The form of Chase's ventro-ingual block is so accurately adapted to that of the os pubis, that it has secured the bowel perfectly in every instance of ventro-ingual hernia in which it has been seen applied by the committee. The primary adjustment of the truss is considerably more difficult, and requires more time and skill in the worst cases of this accident than in the inguinal variety; but the ultimate success of retention does not appear to be less perfect when once accomplished. The pressure of this block upon the os pubis has been made a subject of complaint in only one instance, and the inconvenience then resulted from a slight mal-adjustment in the first application, which being corrected, the difficulty never recurred.

The only peculiarity of the Vento-Inguinal Truss of Dr. Chase consists in the

form of the block. In every other particular, it is identical with the inguinal truss. But, in the application of the instrument, it is necessary that the perineal strap should be secured, at its anterior extremity, to the button on the end of the block-slide, and not to that on the anterior extremity of the spring. To the complete instrument, as it has been actually employed during the last year, the committee may safely apply the same language used in concluding their remarks on the inguinal truss.

III. Of the Femoral Truss.

Fig. 7. The letters from *a* to *h*, inclusive, have the same reference as in fig. 1. *k*. A window in the anterior extremity of the main-spring. *l*. The iron neck of the block-slide, continued along the main-spring for some inches and seen through the window *k*. *m*. Two broad-headed screws of the spring adjustment, securing the flattened extremity of the iron neck to the main-spring, and, when loose, permitting it to slide on the main-spring.

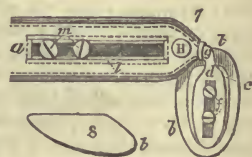


Fig. 8. A longitudinal section of the femoral block.

Of the Femoral Block. It is very difficult to describe the form of this block, and the committee will refer to the treatise of the inventor for the best description and an excellent woodcut representation of it. By considering the mechanical principles of its action, together with the only case fairly before the committee in which it has been employed, it is deemed safe to recommend it as preferable to any pad or block previously employed in this variety of hernia. It is calculated to preserve its position more accurately than the one before in use; it is not liable to become disturbed by the motions of the thigh; and it gives support in a direction which enables it to act at the greatest mechanical advantage. How far it may answer the special purpose of its construction, by entering under the fold of Poupart's ligament and acting almost directly on the femoral ring, the committee will not venture to judge upon the evidence of a single case. The report of Dr. Chase, as to its result in other instances, is favorable; but neither that gentleman nor the committee regard it as having acquired the highest degree of perfection of which it is capable. It will, probably, undergo further modification.

Of the Block-attachment. The extreme accuracy desirable in the adjustment of the small femoral block renders the mode of attachment a matter of great importance. Dr. Chase has succeeded in reaching, in this respect, a degree of perfection much higher than that attained by any of his predecessors. The relation of the femoral ring to the parietes of the pelvis varies in different individuals to a much greater extent than that of the abdominal canal, and its variations are not so nearly confined to one right line. The soft iron neck of the block-attachment in this truss is bent at a right angle, so as to place the long diameter of the block in a position perpendicular when the patient stands erect. In this position the motions of the block-slide, which are similar to those observed in the preceding trusses, adapt the block to the height of Poupart's ligament with great nicety; but to meet the peculiarities of individuals, in regard to the distance between the ring of the ilium and the femoral ring, another arrangement is necessary. There is a fenestrum, two inches in length, in the anterior extremity of the spring; and the soft iron neck, instead of being permanently secured to the spring, is elongated two or three inches, curved, flattened, and attached to the spring by means of two screws, which pass through the fenestrum, and, when loosened, play freely therein, so as to allow the block to approach or recede from the mesial line to any required degree. This double adjustment is simple, secure, and perfectly accurate.

There is no other peculiarity in the spring or appendages of this truss, but the perineal strap is always secured in front to the button on the bottom of the block-slide.

As regards the retentive power of the trusses which have been approved by the committee, it has been tested in various manners, and severely. Some of the patients, while wearing them, have followed the most trying labours of the harvest

field and the marble-yard; others have travelled hundreds of miles on horseback, over mountainous countries. The subject of the worst incurable case of ventro-inguinal hernia, which had destroyed his usefulness, notwithstanding his endeavours to retain the bowel by means of other instruments, has since resumed his labours as a stevedore and sailor; some have followed the chase, and leaped fences and dykes, gun in hand, &c.; yet, since the instruments were brought to their present high state of perfection, the committee know of no instance of protrusion under these exertions.

The committee have been unable to trace any distinct connexion between the superficial effects of these instruments, and the changes perceived in the tendinous margins of the hernial orifices noticed during the time of their employment.

The orifices of very large ventro-inguinal hernia are found to contract rapidly after the perfect adjustment of the block, so that a few weeks or months will sometimes suffice to reduce an opening which will receive three fingers, with the skin of the abdomen inverted to less than one half its original area.

While this contraction is taking place, the tendinous margin of the ring appears to increase very gradually in thickness, and the impression is produced that the substance of the tendon is enlarged by an intestinal deposition. This deposition is decidedly soft at first; but, though rendered by degrees more firm and resisting, it does not distinctly assume the well marked characters of the purely fibrous expansion in which it is formed, within any period yet determined, at least so far as can be ascertained by the sense of touch. In common inguinal hernia, even when they have become nearly direct, and in ventro-inguinal hernia of recent date, or moderate extent, the contraction and thickening continue on the increase until the affected ring is often rendered smaller, and, sometimes, much smaller than in the normal condition.

After all that has been stated, the committee feel themselves fully warranted in the following conclusions:

1. The retentive power of solid blocks is, *cæteris paribus*, superior to that of soft pads in the treatment of hernia.

2. The chances of radical cure depend upon the perfection and permanence of the retention.

3. The perfection and permanence of the retention depend—first, upon the mechanical action of the instruments; and, secondly, upon the power of the parts affected to bear that action without danger of physiological accidents of sufficient importance to interfere with the treatment.

4. All the instruments with solid blocks, contrived before the recent inventions of Dr. Chase, are decidedly liable to important mechanical objections.

5. The instruments of Dr. Chase have effected the permanent and accurate retention of the intestines in every case of hernia observed by the committee, without material inconvenience to the patient, and often under trials more severe than are usually ventured upon by those who wear other trusses; trials which would be imprudent with any other apparatus known to the committee.

6. If we except the Femoral Truss, these instruments have stood the test of much practical application without superinducing any physiological accidents of sufficient importance to interfere with the treatment.

7. The mechanical principles upon which the femoral truss is constructed appear highly ingenious and promising; and unless this instrument should be found hereafter to be productive of important physiological accidents, it must take precedence of all other modes of treating this variety of the disease. No such accidents are yet known to have been produced by its employment; but the committee have not enjoyed the opportunity of personal inspection in a sufficient number of cases to determine general results, nor do they deem it proper to receive evidence from any other quarter in discharging the trust reposed in them by the society.

The committee are induced by the foregoing conclusions to recommend, in strong terms, the instruments of Dr. Chase to the confidence of the profession, as the best known means of mechanical retention in hernia, and as furnishing the highest chances of radical cure.

All the individuals who have relinquished the use of the trusses approved by the

committee, after having worn them for six months or more, and who have been afterwards examined by a member or members of the committee, have been subjected to the necessary tests, and are believed to be radically cured in the sense of the foregoing definition. A still larger number who are yet under treatment give promise of a similar result; and those who refuse finally to relinquish the instrument on the advice of their surgeon, present, in the firmness of the rings, and in the absence of protrusion under exertions performed when the trusses are temporarily removed, very strong grounds for believing the cure to be radical in them also.

The time required for the radical cure of an ordinary case of ventro-inguinal or direct hernia in the adult, appears to be from twelve to eighteen months. It is probable that the bowel, in common inguinal hernia, is rendered secure in a shorter time, but prudence has prevented the earlier relinquishment of the truss except in a very few cases. The orifice in umbilical hernia, appears to contract somewhat more slowly, but all the varieties recover much more rapidly in childhood.

American Journal of Med. Sciences. Aug. 1837.

IMPROVED FEMALE SYRINGE. BY HEBER CHASE, M.D.

THE superiority which this instrument possesses consists in its perfect adaptation to the anatomical form of the external organs of generation.

A, the cylinder, is about five inches in length, with a caliber of one inch. Projecting from its lower extremity, B, at an angle of about eighty-five degrees, is a tube of one inch and a half in length and six lines in diameter, and terminating at C by a male screw in the shield D, now to be described. The shield is of a conoid form, produced considerably near the truncated summit, and laterally compressed; about four inches in length, half an inch in diameter at the apex, and has about three inches vertical and two inches transverse diameter at its base. The superior extremity of the vertical diameter rests against the cylinder of the instrument, while the inferior extremity is carried backwards and downwards so as to press on the perineum a few lines posteriorly. Upon the extremity of this shield is placed an ivory tube, E, extending one-third of its whole length. This tube, from its connexion with the shield at C, is gradually increased towards its extremity, and terminates in a diameter of ten lines, where it is perforated by from twelve to fifteen holes all around its bulbous extremity. The ivory tube may be removed from the shield at C, where it is attached by means of a screw. The shield itself may be removed from the instrument in the same manner, and at nearly the same point.

Directions for Use.—The bulbous extremity of the instrument should be introduced into the vagina, and carried, backwards and upwards, nearly or quite to the os uteri, the base of the shield closing the vagina at its orifice. When the contents of the syringe are thrown into the vagina, the fluid, of whatsoever nature, is projected not only against and around the os uteri, but cleanses also, by means of the numerous orifices in the bulb, the other parts of the canal, while the shield prevents its escape.

The syringe is made of the usual material, pewter, with the exception of the bulbous extremity of the shield which, as before stated, is of ivory. In manufacturing the instrument, care should be taken that the shield be highly finished, and the holes in the bulbous extremity be made smooth, so that no friction upon the internal parts may follow its use. Ivory is far preferable to bone or other substances for forming the bulb, from the facility with which it may be polished.

American Journal of Medical Sciences. February, 1837.



ABSTRACT OF AN EXPERIMENTAL INVESTIGATION INTO THE FUNCTIONS OF THE EIGHTH PAIR OF NERVES. BY JOHN REID, M.D.

[A SHORT account of the enquiry the chief results of which we are about to present to our readers, was laid before the medical section of the British Association at its late meeting in Liverpool; and we are happy to be now enabled to give such an abstract of it as may convey an idea of its value. We owe this gratification to the kindness of the author, who has transmitted to us a copy of the original paper, which appears, we believe, in full, in the January number of our able contemporary, the Edinburgh Med. and Surg. Journal. The difficulty of attaining satisfactory conclusions on this intricate subject is allowed by all, and by none more completely than by Dr. R. himself. He has, however, brought to his task qualifications of no ordinary kind: the most accurate anatomical knowledge and operative skill, the most careful and candid observation of phenomena, the most judicious discrimination regarding their causes, and the most philosophical spirit in reasoning upon them. That our praise is exaggerated, no one, we venture to predict, will affirm after reading the original paper. It is incumbent upon all experimentalists, who arrive at results different from those usually obtained, to point out, as far as they have the means, the causes of the discrepancy; and this the acuteness of Dr. R. has enabled him to do in a great proportion of the instances in which he questions the accuracy of the conclusions of other physiologists. We feel the greatest confidence in the accuracy of his inferences, not only on the grounds already mentioned, but because he never carries them a step further than his facts absolutely warrant; always reserving for future investigation any points which he does not regard as fully determined. The number of the experiments, also, and the correspondence of their phenomena *under similar circumstances*, are a strong guarantee for their accuracy; whilst the care taken to ascertain the precise nature of the operation performed by *subsequent minute dissection*, often disclosed unexpected causes for apparent diversity in the results. It is only necessary for us further to state, that the names of Dr. Alison and Dr. Sharpey frequently present themselves as additional testimonies to the accuracy of Dr. Reid's statements.

The following brief account of the results of each set of experiments is principally derived from the recapitulations made by Dr. R. himself in the original memoir. We reserve for future notice some of the more interesting questions regarding the associated actions in which these nerves are concerned. We are not the less willing to lay these results of Dr. Reid's investigations before our readers, because they tend to invalidate some opinions formerly expressed by us: we can have only one object, the attainment of truth.]

GLOSSO-PHARYNGEAL NERVE. From twenty-seven experiments upon this nerve, the following conclusions are drawn:

1. That it is a nerve of *common sensation*, as indicated by the unequivocal expression of pain by the animal when the nerve is pricked, pinched, or cut.
2. That the extensive muscular movements of the throat and lower parts of the face, which follow the mechanical or chemical irritation either of the pharyngeal branches themselves or of the trunk in which these are united, depend, not upon any influence extending downwards along the branches of the nerve to the muscles moved, but to a reflex action, transmitted through the central organs of the nervous system. This is clearly proved by the following statement: "It was repeatedly observed that, when the nerve was cut across, irritation of the lower end, or that in connexion with the muscles, was followed by no movement; while, on the other hand, irritation of the cranial end was followed by as powerful convulsive twitches as when the nerve was entire." It is also shown that this irritation is conveyed by the pharyngeal branches only; that the pinching of the lingual branch gives rise to no muscular movements; whilst these effects are strongly produced by irritation of small twigs of the pharyngeal division.
3. That irritation of this nerve immediately after death is not followed by any muscular movements, when sufficient care is taken to insulate it from the pharyngeal branch of the par vagum; whilst, at the same time, irritation of the divided extremity of the latter nerve will produce these movements.

4. That the pharyngeal branches of the glosso-pharyngeal possess endowments connected with the peculiar sensations of the mucous membranes upon which they are distributed, though it is not yet possible to specify in what these consist; but that this cannot be the sole nerve upon which all these sensations depend, since the perfect division of the trunk on both sides does not interfere with the perfect performance of the function of deglutition.

5. That it may participate with other nerves in the performance of the function of taste; but that it certainly is not the special nerve of that sense, since, after the perfect section of both trunks, the sense of taste is sufficiently acute to enable the animal readily to recognize bitter substances; and that the sensation of thirst does not entirely depend upon it.

PNEUMOGASTRIC NERVE. Pharyngeal Branches. From the previous and other experiments, it also appears:

1. That these branches are the motor nerves of the constrictors of the pharynx, the stylo-pharyngeus, and the muscles of the soft palate, and are called upon to act in obedience to the stimulus conveyed by the glosso-pharyngeal and other sensory nerves.

2. That they have little or nothing to do with sensation, no decided indications of suffering being produced by irritating them.

3. That section of this nerve greatly impairs the power of deglutition; the animal retaining, however, the means of forcing small morsels through the pharynx by the action of the muscles of the tongue and neck.

Œsophageal Branches. The following appears satisfactorily ascertained regarding the function of these branches:

That the continuity of these nerves with the spinal cord is necessary for the action of the Œsophageal muscular fibres, which do not act by direct stimulation, as Haller supposed; and that a clear case of *reflex action without sensation* is thus established.

On the *Gastric branches*, Dr. Reid's observations are not yet concluded, but we believe that they will probably lead to a similar result.

Laryngeal branches. From numerous experiments on these nerves, Dr. Reid draws the following interesting and novel conclusions:

1. That the superior laryngeal furnishes one muscle only with *motor* filaments, viz. the crico-thyroid.

2. That it furnishes all, or nearly all, the *sensitive* filaments of the larynx, and also some of those distributed upon the *mucous surface* of the larynx.

3. That the inferior laryngeal, or recurrent, furnishes the sensitive filaments to the upper part of the trachea, a few to the mucous surface of the *pharynx*, and still fewer to the mucous surface of the *larynx*.

4. That, when any irritation is applied to the mucous membrane of the larynx in a healthy state, this does not excite the contraction of the muscles which move the arytenoid cartilages, by acting directly upon these through the mucous membrane; but that this contraction takes place by a reflex action, in the performance of which the superior laryngeal is the excitor, and the inferior laryngeal is the motor nerve.

Cardiac branches. It seems probable, from Dr. Reid's experiments, that these branches form one of the means by which the heart's action is influenced by the state of the cerebro-spinal nervous system; since sudden destruction of the brain did not produce so great an effect upon the motion when these nerves had been divided as in animals in which they were entire. It would seem, however, that the sympathetic is the medium of communication for impression upon the spinal cord.

Pulmonary branches. Dr. Reid fully confirms the statements of other experimentalists, that respiratory movements continue after the section of the pneumogastric nerves; though he believes that they ordinarily convey the stimulus to the medulla oblongata by which these movements are excited. He has fully proved that the continued movements cannot be of a voluntary character as Dr. Hall supposes; and seems inclined to refer them to the action of the sympathetic, in which view we think that he is borne out by comparative anatomy, since the

two nerves are much more closely united in many of the lower animals than in man. Dr. R. doubts whether the mucous membrane of the trachea and bronchi is sensitive to the extent stated by Brâchet; and has given a very interesting account of the changes produced in the lungs by section of these nerves, to which we are prevented by want of space from referring more particularly.

SPINAL ACCESSORY NERVE. Dr. Reid denies that this can be regarded as specially a respiratory nerve, and states that its *external* branch seems to resemble in its functions the filaments of the cervical plexus, with which it anastomoses freely; whilst the *internal* branch assists in moving the muscles of the pharynx. Its other endowments Dr. R. intends to make the subject of future experiments.

MEDICAL RELIEF UNDER THE NEW POOR-LAW.

THIS subject engrosses so much of the attention of the profession at present, and we have received so many communications respecting it, that we think it necessary to state that it is our intention to consider it in detail in an early Number. One of the subjects to which the attention of Parliament will be directed during the present session will be the errors that have crept, more or less, into every part of the Poor-law Amendment Act, and especially into that part of it relating to the medical relief of paupers. Deeming this Act one of the *most* important that has ever been passed by the governing body of any nation, although its large views cannot be accomplished except in connexion with other means of raising and improving the character and condition of the labouring classes, we have refrained from any hasty condemnation of its defects, and have deeply regretted to see its intentions and probable effects so mistaken or so misstated, not only by many public men for party purposes, but, as regards our own profession, by many of its members, and many who profess themselves the best friends of medical men. It is evident to us, as to all, that useful modifications are required, and may easily be made, in those portions of the Bill which relate to medical attendance on paupers; but these modifications have often been urged without temper, and sometimes without consideration. When the whole subject comes regularly before us, we shall examine it freely and impartially, with a sincere regard for the welfare of the profession, but with small respect for their prejudices, and with no dread of mere clamour. In the mean time, we may say that it appears to us that the establishment of fixed salaries in place of the system of *tenders*, the subdivision of districts, and the allotment of them with a more constant regard to the residence of the medical man, are much required. That the remuneration ought to be just and regular, no one will be disposed to deny. The interests of medical men, (than whom none gain a livelihood with more care, labour, and anxiety,) demand these changes; and they are no less demanded by a real humanity, which excludes the spurious humanity from which pauperism has derived so much direct and indirect support.

Although, in the foregoing observations, we have spoken of the defects as having crept into *the law*, the truth is, they are almost exclusively chargeable on the manner in which this has been administered by the boards of guardians. It is also too true that not a little of the harshness of the details of which medical men now complain, and justly complain, have originated in their own jealousies and want of mutual confidence. When this law was first brought into operation, if all the members of the profession in the different districts had met, and consulted cordially and candidly together,—had agreed to sacrifice their own little individual advantages for the common good of the profession,—and, in their intercourse with the Boards, had shewn that they consulted only the *general interests* of the medical body and the *welfare of the poor*, we are persuaded that matters would have worn a very different aspect from what they do at present. It must never be forgotten that this great law was devised, not for the advantage of the medical profession, but for the improvement of the condition of the poor; and if, in the attainment of this paramount advantage, some of the more favoured classes of society suffer temporary loss or inconvenience, surely there is some compensation in the philan-

thropic and patriotic feelings which attend the sacrifice. It would, indeed, be most strange if the members of the medical profession,—a profession which has always been honorably distinguished for its large and active benevolence,—should hesitate, on such an occasion as the present, to participate, in some slight degree, in those inconveniences and privations which at present bear so hardly on their poorer brethren; but which, it is confidently believed, will eventually be productive of incalculable benefit to their order.

MEDICAL CERTIFICATES TO ASSURANCE SOCIETIES.

A GREAT feud has arisen between medical men and the Assurance Societies. A careful man, regardful of advancing time, and the claims of those who will survive him, wishes to secure to them the payment of a certain sum after his death, by the payment of an annual premium as long as he lives. Numerous societies will undertake to do this for him, on the chance of his being likely to live as long as most men of his years. For the purpose of ascertaining this point, important to both parties, the Societies employ and pay a medical referee. Thus far there is nothing objectionable or doubtful. But they also demand and receive the certificate of the good man's medical attendant, while the good man often forgets to pay a fee for it, or offers to pay one and the fee is refused. The medical man cries out against the office, because he gets no fee. Now the question is, is the office in fault? We think not. It is assumed, by those who consider the Office in fault, that the Company is the party which profits, either exclusively or principally. Now this is altogether a mistake, betraying gross ignorance of the principles of assurance. Both parties profit; and, when the Company is founded on sound principles, profit equally. The Company does not seek the party requiring assurance, but the party the Company; and, if the latter does not choose to submit to the regulations of the former, he may refrain from claiming its assistance. A certificate is deemed essential by the Company, and it is incumbent on all claimants of assurance to furnish it. The Company is prepared to pay its own medical officer, when the party comes to be examined: why it should also pay for what is the claimant's affair, not its own, is surely any thing but obvious. We are, therefore, of opinion that medical men are wrong in calling out so loudly for their fees from the Offices; and think that, if they receive them at all, they ought to receive them from the patient who seeks the assurance. Whether such fee should be demanded, or whether this good office should be done gratuitously, is another question, which men will decide according to their feelings. If no visit is required to be paid, and no new examination of the patient is necessary, and all that the physician or surgeon has to do is to fill up the blanks in the ordinary printed circulars, we do think that not only the common rights of citizenship and acquaintanceship, but the honour of the profession and the interest of the professors alike require that no fee should be demanded or received, under such circumstances.

OBITUARY.

DR. JOHN MACKINTOSH, OF EDINBURGH.

ON the 28th of October, at Edinburgh, Dr. John Mackintosh, long known as a popular and successful medical teacher in that city. Dr. Mackintosh was the second son of Captain Mackintosh, of the 4th regiment of Foot, and was born at sea during the return of his mother from America, whither she had accompanied his father on service. John Mackintosh was destined for the medical profession at a very early age, or rather of his own accord formed the resolution of studying medicine, and could distinctly trace his predilection for the healing art to his having fractured his leg when a child. During the confinement consequent on that accident, he was so impressed with the usefulness of surgery, that he determined to make it the business of his life. After passing through the usual routine of education necessary for the student of medicine, Mr. Mackintosh was apprenticed, in 1804, to Messrs. Bell, Wardrop, and

Russell, at that time in high repute in Edinburgh as general practitioners and operating surgeons, and who in a great measure monopolized the surgical practice in the Royal Infirmary. Having finished his studies in 1808, he was immediately afterwards appointed a medical officer in the Royal Artillery, and served abroad in that respectable corps, principally in the West Indies and South America, and in the army of occupation in France some years after the battle of Waterloo.

In 1821, Dr. Mackintosh commenced practice as a physician in Edinburgh; and, in 1822, published a work on Puerperal Fever, with the view of elucidating the true nature of the dangerous malady which had recently committed dreadful ravages in Edinburgh and Leith. In 1823, he began a course of lectures on Midwifery and the Diseases of Women and Children, and proved a successful teacher of that department of medical science. In 1825, in compliance with a numerously signed requisition from the medical students then in Edinburgh, he commenced lecturing on the Principles of Pathology and Practice of Physic; and, since 1829, has been one of our most successful and popular teachers.

Dr. Mackintosh's greatest work, that on the Principles of Pathology and Practice of Physic, was first published in 1828, and has been extremely popular among students and junior practitioners, both in this country and in America. We gave a pretty full account of it in our Fifth Number.

Dr. Mackintosh was an ardent cultivator of medical science. His zeal in the cause of pathology is proved by the extent and variety of his splendid museum; and, what is particularly worthy of remark, his pathological researches were generally prosecuted with the view of being made subservient to the improvement of the practice of medicine. His zeal in the cause of science, as well as his philanthropy, were strikingly shewn during the prevalence of cholera in Edinburgh; no one having made such strenuous exertions in attending the sick and in attempts to throw light on the nature of the disease.

It is impossible to speak too highly of Dr. Mackintosh's qualifications as a practical physician. Acute powers of observation, presence of mind, gentleness, combined with resolution in the sick-chamber; whilst, in the consulting room, candour and openness to conviction, from whatever quarter it came, were the characteristics of his liberal mind.

Dr. M. possessed in a high degree most of the essential qualities of a good and successful teacher, although these were intermixed with striking defects; which also accompanied him, in some degree, beyond the walls of his lecture-room. He was too boastful and confident in his own opinions, and did not hesitate rashly to disparage others from whom he differed; but, with all this, there was an earnestness of purpose, and even a prevailing tone of generosity of feeling, which had an excellent effect upon his pupils; and, if he was boastful, his deeds were always at hand to support him, although not exactly to the extent which he claimed. With a good deal of rashness and occasional lack of discretion in his general conduct, he combined great activity and acuteness; and it must be admitted that no one has appeared for many years in the medical school of Edinburgh who produced so salutary an effect in awakening and stimulating the zeal both of teachers and students. He inspired the latter with enthusiasm for their profession, and made them active thinkers, instead of passive recipients of lifeless precepts.

Dr. Mackintosh was an honorable and an excellent man, and most estimable in all the relations of life: he was a warm-hearted, generous, and faithful friend, and one of the best of husbands and fathers; but, owing to his ardent temperament, his superabundant self-esteem, and his want of prudent caution, he had the misfortune to excite considerable hostility against him among his medical brethren, and to alienate some of his intimate friends. It is but justice to Dr. Mackintosh, however, to state that most of the differences of this kind originated in misconception or misrepresentation, as it was not in his nature to injure even an enemy.

Dr. Mackintosh was married early in life to Miss Mackenzie, of Bayfield, Rosshire, who survives him, together with one daughter and four sons.*

* The principal part of the above notice is taken from an article in *The Scotsman* newspaper, of November 8, 1837.—EDS.

DR. WILLIAM WEST, OF DUBLIN.

At Dublin, on the 8th of October, 1837, William West, M.D., in the thirty-fourth year of his age. In recording the premature death of this amiable and excellent gentleman, we bear a willing testimony to his great talents and acquirements as a scholar and physician. He was one of the most esteemed of our contributors, and our counsellor and assistant in the difficulties of some of the northern languages. We borrow the following short notice of him from the *Dublin University Magazine*, for November, 1837.

"As a student of modern languages, Dr. West's success was, in Ireland at least, unrivalled. His accurate and familiar acquaintance with the most intricate and least-studied dialects of northern Europe had earned for him a reputation as a linguist which was not confined to the circle of his native shores. By many learned men on the continent his opinions were sought after with anxiety, and attended to with respect. We have a melancholy satisfaction in acknowledging the obligations under which he frequently placed this Journal, by contributions, the place of which it is impossible to supply. Every day was adding alike to his knowledge and his reputation, when his distinguished career was brought to a close in an early grave."

In the university, Dr. West had borne the highest honours of the under-graduate course. At a later period of his life, the Royal Irish Academy conferred on him the high distinction of electing him a member of their council. In private life, universally respected and esteemed, few men had more of warmer friends; none, perhaps, ever made fewer enemies. The daily press, of all parties, has borne honorable and abundant testimony to the general regret which was occasioned by his premature decease. From the many tributes to his memory which the melancholy event called forth, we select the following from the columns of the *Dublin Evening Mail*:

"When we consider the estimation in which this individual was held as a sound and erudite scholar by the most learned men of the day, the ardent zeal with which he prosecuted science, and the premature age (only thirty-four) at which he was cut off, it is not too much to say that his loss was a public one, and one that must be deeply deplored by the world of letters. The promises held out by his collegiate success were more than realized in the distinction his labours in medicine, philology, geography, and botany subsequently acquired for him. Of these subjects, the science of language occupied his especial attention; and so accurate and profound was his knowledge in this branch of literature, that in matters of abstruse research his assistance was urgently sought after by the most eminent philologists, both at home and abroad. His capabilities in this respect were practically and disinterestedly applied to rendering the most recent discoveries in medicine throughout Europe available to his professional brethren at home. The purity of mind, singleness of purpose, and almost childish simplicity of thought in the ordinary affairs of the world, so often the characteristic accompaniments of true genius, were leading features in Dr. West's character. He may be said to have fallen a sacrifice to his too-ardent pursuit of science, as the fatal fever which terminated his short but distinguished career was caused by close application, whilst in a delicate state of health, in preparing for the British Association an elaborate critical illustration, through its primitive dialects, of the ancient geography and history of Gaul and the British Isles."

G. R. TREVIRANUS, OF BREMEN.

GOTTFRIED REINHARD TREVIRANUS was born in Bremen, February 1, 1776, of a respectable mercantile family. He was the eldest of eight children, three of whom only survive him, and one of whom (Ludolf Christian) is the distinguished professor of botany at Bonn, and was the able coadjutor of his brother in many of his works. G. R. Treviranus received his scholastic education in Bremen, where he shewed early talents and a decided inclination for science, although his family wished him to follow the mercantile profession. Having, however, selected medicine, he repaired to Göttingen, and pursued his studies from 1792 to 1796, with that indefatigable zeal which marked his whole life. He early shewed a taste for physiology, and indeed,

in 1795, published anonymously a physiological essay, entitled "Ueber Nervenkraft und ihre Wirkungsart." In September, 1796, he took his degree of doctor; his thesis being on his favorite subject, viz. "De emendanda Physiologia." Shortly after his return to Bremen, he married; and, although he gave himself to the practice of medicine, he did not at first like it, on account of the interruption it created to his studies. Still he followed it, as well on account of its emoluments as from its intimate connexion with his cherished pursuits. He accordingly divided his time as follows: In the early part of the forenoon he visited his patients, and gave the remainder of it to dissections and microscopical observations; the spare hours of the afternoon he devoted to reading; those of the evening to composition,—seldom or ever leaving his native town. In this quiet and philosophical manner he passed forty years, happy in his pursuits; in the prosecution of which he may be said to have died, as only ten days before his death he visited some patients, and busied himself in correcting the proofs of one of his physiological writings.

Soon after his return from the university, viz. in 1797, he was appointed professor of medicine and mathematics in the Lyceum of Bremen. The same year he published his first work, the first part of his "Physiologische Fragmente;" and which was followed by numerous well-known and most valuable publications, either in the journals or separately, during the remainder of his busy life. The following are his principal works:

1. Physiologische Fragmente. Two vols. 8vo.—Hannov. 1797-99.
2. Biologie, oder Physiologie der lebenden Natur. Six vols. 8vo.—Götting. 1802-1822.
3. Ueber d. innern Bau der Arachniden.—Nurnb. 4to. 1812.
4. Beiträge zur Anatomie und Physiologie der Sinneswerkzeuge des Menschen und Thiere. Fol. 1828.
5. Beiträge zur Aufklärung der Erscheinungen und Gesetze des organischen Lebens. Two vols. 8vo. 1831-33.
6. Vermischte Schriften, anatom. und physiol. inhalts, (with L. C. Treviranus.) Four vols. 8vo. 1816-1820.

From the year 1824 until his death, Treviranus edited, in conjunction with his distinguished friend Tiedemann, the celebrated "Zeitschrift für Physiologie," of which five volumes have been published, and in each of which one or more of our author's excellent papers appeared.

Treviranus long laboured under a morbid state of the lungs, which had for several years before his death often put on a serious character, especially during winter. It was an attack of the same affection, a low nervous pneumonia, which terminated his life on the 16th February, 1837.

Of the great merits of Treviranus as a physiologist, it is hardly necessary to speak: they are universally known and acknowledged. He possessed at once a philosophical and a classical mind; and he was as distinguished for rigid honesty as for unwearied perseverance. He was much and justly valued as a physician. He was no less estimable in all the civil and social relations: he was an excellent husband and father, a true and faithful citizen.*

DR. F. F. G. EGGERT.

FRANZ FRIEDRICH GOTTLÖB EGGERT was born at Eisleben, on the 15th August, 1778. He studied medicine, first at Jena, and afterwards at Leipzig, where he graduated in 1802. From this time till 1816, he practised at Querfurth; and from 1816 until his death, which took place on the 23d August, 1836, in his native town of Eisleben. Besides many practical and critical articles in the medical journals, Dr. Eggert was author of the following works: 1. Ueber die Wassersucht. Leipzig, 1817. 2. Ueber das Wesen und die Heilung des Croups. Hannover, 1820.—3. Die Organische Natur des Menschen. 2 Bde. Leipzig, 1828.—4. Der gewaltsame Tod ohne Verletzung. Berlin, 1832.

* Sachs's Almanach, 1838.

BOOKS RECEIVED FOR REVIEW.

ENGLISH.

1. The Philosophy of Health; or, an Exposition of the Physical and Mental Constitution of Man, with a View to the Promotion of Human Longevity and Happiness. By Southwood Smith, M.D. &c. Vol. II.—London, 1837. 12mo. pp. 448. 7s.
2. Guy's Hospital Reports. No. V. October, 1837. Edited by G. H. Barlow, M.A. L.M. &c., and J. P. Babington, M.A. &c.—London. 8vo. pp. 240. 6s.
3. Elements of Physiology. By J. Müller, M.D., Professor of Anatomy and Physiology in the University of Berlin. Translated from the German, with Notes, by William Baly, M.R.C.S. &c. Illustrated with steel Plates and wood Engravings. Part I.—London, 1837. 8vo. pp. 428. 9s.
4. A concise Treatise on Operative Surgery, describing the Methods adopted by the English, Continental, and American Surgeons; selected for the use of junior Practitioners and Students. Illustrated by Plates. By W. P. Cocks, Surgeon.—London, 1837. 8vo. pp. 375. 14s.
5. Elements of Anatomy. By Jones Quain, M.D. Fourth Edition, revised and enlarged. Illustrated with steel Plates and Woodcuts. — London, 1837. 8vo. Part I. pp. 388, 12. 22s.
6. Elements of Chemistry; including the recent Discoveries and Doctrines of the Science. By the late Edward Turner, M.D. Sixth Edition; enlarged and revised by Professor Liebig and Wilton G. Turner. Part I.—London, 1837. 8vo. pp. 410. 7s.
7. A Discourse on some of the Diseases of the Knee-joint; delivered before the Massachusetts Medical Society, at their annual Meeting, May 31, 1837. By G. Hayward, M.D., Professor of Surgery in Harvard University.—Boston, 1837. 8vo. pp. 28.
8. Memoranda on difficult Subjects in Anatomy and Surgery; being a Pocket Companion for Students preparing for the College of Surgeons. By Robert Druit, M.R.C.S.—London, 1837. 48mo. 2s.
9. Remarks on the Memorial of the Town-Council of Edinburgh, respecting the Professorship of Medicine and general Pathology. By the present Professor.—8vo. pp. 43.
10. Report of the Proceedings at the first annual Meeting of the Southern District Branch of the Provincial Medical and Surgical Association, held at Winchester, June 5, 1837.—8vo. pp. 30.
11. Transactions of the Medical Society of the State of New York. Vol. I. II. III. —Albany, 1832-1837. 8vo. pp. 372, 388, 362.
12. Hints to Mothers for the Management of Health during the Period of Pregnancy, and in the Lying-in Room; with an Exposure of the popular Errors in connexion with those Subjects. By Thomas Bull, M.D., Physician-Accoucheur to the Finsbury Midwifery Institution, &c.—London, 1837. 12mo. pp. 174. 5s.
13. On the Use of Auscultation and Percussion in the Diagnosis of Diseases of the Organs of Respiration and Circulation; with Directions for the Employment of Inspection, Succussion, Palpation, and Mensuration of the Thorax. By Julius Wolf, M.D.—London, 1837. 8vo. pp. 200. 6s.
14. A Conspectus of the Pharmacopœias of the London, Edinburgh, and Dublin Colleges of Physicians; being a Practical Compendium of Materia Medica and Pharmacy. By A. T. Thomson, M.D. F.L.S. &c. Tenth Edition.—London, 1837. 12mo. pp. 190. 5s. 6d.
15. Cases of Hidrosis, or Hidrotic Fever; with Remarks. By J. C. W. Lever, M.R.C.S.—London, 1837. 8vo. pp. 24.
16. Outlines of Comparative Anatomy. By R. E. Grant, M.D. &c. Part IV., containing Digestive Organs, Chyliferous and Sanguiferous Systems.—London, 1837. 8vo. pp. 80. 4s.
17. Practical Surgery; with 120 Engravings on Wood. By Robert Liston, Surgeon.—London, 1837. 8vo. pp. 494. 22s.
18. The Works of John Hunter. Edited by Mr. Palmer. Vol. III. 8vo. 17s. 6d.
19. Treatise on Hernia: comprising the Surgical Anatomy, Operative Surgery, and Treatment of that important Disease in all its Forms; as also a newly proposed Operation for the Relief of strangulated Hernia. By M. W. Hilles.—London, 1838. 12mo. pp. 52. 2s.
20. First Principles of Surgery; being an Outline of Inflammation and its Effects. By G. T. Morgan, A.M., Lecturer on Surgery in Aberdeen. Part II.—8vo. pp. 207. 5s.
21. Prize Thesis, on the Presence of Air in the Organs of Circulation. By J. R. Cormack, M.D.—Edinburgh, 1837. 8vo. pp. 56. 1s. 6d.
22. Essay on the Classification of the Insane. By M. Allen, M.D.—London, (no date.) 8vo. pp. 212; with Plates. 6s.
23. Changes produced in the Nervous System by Civilization, considered according to the Evidence of Physiology and the

Philosophy of History. By Robert Verity, M.D.—London, 1837. 8vo. pp. 79. 5s. 6d.

24. Obstetric Plates, with Explanations, selected from the Anatomical Tables of William Smellie, M.D. (Republished.)—London, 1837. 8vo. pp. 32; Plates, 12. 5s.

25. Institutes of Surgery: arranged in the order of the Lectures delivered in the University of Edinburgh. By Sir Charles Bell, K.H.G. F.R.S.S. L. and E., Professor of Surgery in the University of Edinburgh. In two vols. Vol. I.—Edinburgh, 1838. 8vo. pp. 352. 7s. 6d.

26. A Table, exhibiting the Results of Observations by the Thermometer, Barometer, Hygrometer, and Rain-gauge, &c., from 1829 to 1836, inclusive, at Exeter. By T. F. Barham, M.B.—One sheet, folio. Exeter, 1837.

27. A Letter to the Inhabitants of Ceylon on the Advantages of Vaccination. By J. Kinnis, M.D.—Ceylon, 1837. 8vo. pp. 28.

28. The Question concerning the Sensibility, Intelligence, and instinctive Actions of Insects. By David Barham, M.D., one of the Radcliffe Travelling Fellows, &c.—Paris, 1837. 8vo. pp. 54.

29. On the Diseases of the Rectum. By James Syme, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh.—Edinburgh, 1838. 8vo. pp. 138. 5s.

30. Remarks on the Treatment of Fractures of the Extremities without the Aid of Splints: with Cases. By J. F. Burke, M.R.C.S.—London, 1837. 8vo. pp. 39. 2s.

31. The British Medical Almanack, 1838; with a Supplement. Edited by William Farr.—London, 1837. 12mo. pp. 224. 3s.

32. Powers of the Roots of the Nerves in Health and in Disease. Likewise, on Magnetic Sleep. By Herbert Mayo, esq. F.R.S.—London, 1837. 8vo. pp. 39.

33. A Lecture on the Nature and Cultivation of the Medical Profession; intended as a Guide to Students. By G. T. Morgan, A.M., Lecturer on Surgery in Aberdeen.—London, 1838. 8vo. pp. 28. 1s.

34. Practical Remarks on the Diseases of the Skin, on the external Signs of Disorder, and on the Constitutional Peculiarities during Infancy and Childhood. By W. C. Dendy, Surgeon to the Infirmary for Children, &c.—London, 1837. 8vo. pp. 153. 6s. 6d.

FOREIGN.

1. Rede zur Feier des 43sten Stiftungstages des Königlich Med. Chir. Fred. Wilh. Instituts, am 2ten August, 1837, gehalten. Von Dr. J. F. K. Hecker, Königl. Professor der Heilkunde.—Berlin, 1837. 8vo. pp. 19.

2. Ueber Virilencenz und Rejuvenescenz

thierischer Körper: Ein Beitrag zur Lehre von der regelwidrigen Metamorphose Organischer Körper. Von Dr. C. W. Mehliß praktischem Arzte in Liebenwerda.—Leipzig, 1838. 8vo. pp. 114.

3. Lehrbuch der Geburtshülfe zum Unterricht für Hebammen. Von Dr. J. C. Stark, &c.—Jena, 1837. 8vo. pp. 292.

4. Die freie Hansestadt Bremen und ihr Gebiet in topographischer, medizinischer, und naturhistorischer Hinsicht geschildert, von Ph. Heineken, M.D. &c.—Bremen, 1837. 8vo. pp. 215.

5. Arsberättelse om Svenska Läkare-Sällskapets Arbeten. (Eight Volumes.)—Stockholm, 1825–1832. 8vo.

6. Acta Solennia quibus tertium Jubilæum sacrorum in regno Daniæ reformatum, Die xxxi. Oct. An. 1836, in Aula Academica celebravit Universitas Regia Hauniensis.—Haunia, 1837. Fol. pp. 84.

7. Observationes aliquot ad Physiologiam Ganglii Ophthalmici pertinentes. Dissertatio inauguralis quam conscripsit Henricus Wendt, Hafniensis.—Kilia, 1834. 4to. pp. 22.

8. Die Influenza oder Grippe, nach den Quellen historisch-pathologisch dargestellt. Eine gekrönte Preisschrift. Von Dr. Gottlieb Gluge.—Minden, 1837. 8vo. pp. 167.

9. Ueber das Seebaden und das Norderneyer Seebad. Von Dr. Carl Mühy, Lehrer der Physiologie, &c. zu Hannover.—Hannover, 1836. 8vo. pp. 184.

10. Darstellungen und Ansichten zur Vergleichung der Medicin in Frankreich, England, und Deutschland. Von Dr. C. Mühy.—Ib. 1836. Pp. 283.

11. Der Arzt im Menschen, oder die Heilkraft der Natur. Von Dr. G. F. Ch. Greiner, Hofmedicus und Stadtphysicus zu Altenburg.—Altenburg, 1827, 1829. Two Vols. 8vo. pp. 484, 487.

12. Die Vergleichende Osteologie des Schläfenbeins. Zur Vereinfachung der herrschenden ansichten bearbeitet von Eduard Hallmann. Mit iv. Kupfer tafeln.—Hannover, 1837. 4to. pp. 130.

13. Zur Vermittelung der Extreme in der Heilkunde. Von Th. Stürmer, M.D. &c.—Leipzig, 1837. 8vo. pp. 448.

14. Anthropologischer Beitrag zur Erfahrung der psychischen Krankheit, &c. C. P. Möller, M.D. &c.—Mainz, 1837. 8vo. pp. 507.

15. Physiologisch-Therapeutische Untersuchungen über das Veratrin. Von Dr. Fred. A. Forcke.—Hannover, 1837. 8vo. 146.

16. Periodologie oder die Lehre von den periodischen Veränderungen im Leben des gesunden und kranken Menschen. Von Dr. A. M. Baumgarten Crusius.—Halle, 1836. 8vo. pp. 457.

THE
BRITISH AND FOREIGN
MEDICAL REVIEW,
FOR APRIL, 1838.

PART FIRST.
Analytical and Critical Reviews.

ART. I.

A History of the Inductive Sciences. By the Rev. W. WHEWELL, M.A. Fellow and Tutor of Trinity College, Cambridge; President of the Geological Society, &c.—*London*, 1837. Three Vols. 8vo. pp. 437, 534, 624.

THE time, we would fain hope, is now gone by when attainments in knowledge of every kind, not immediately connected with medical practice, were looked on by the public, and even by many of the profession, with a suspicious eye; and when the man of science, who devoted his leisure hours to the combination of amusement and instruction afforded by his laboratory or his study, was thought on that account less worthy of confidence than the frivolous partaker in public gaieties. A better feeling on this subject is gradually gaining ground; and we could point to many distinguished individuals who have shewn that scientific pursuits are by no means incompatible with professional eminence and reputation. The extended course of education now required by our corporate bodies is of itself sufficient to impart no trifling amount of general knowledge to those who fully avail themselves of its opportunities; and, knowing how important is the influence of our profession on the tone of society at large, and how much it has the means of improving the taste for knowledge among all classes of the community, we cannot but anticipate a still farther change in the estimate formed by the public of its requirements.

We do not urge the attainment of general scientific knowledge merely as a matter of expediency: we deem an acquaintance with the principles of physical science essential to every one who is not content with treading the beaten path of medical practice, and who cherishes the higher object of extending its boundaries and diminishing its perplexities. We well remember to have heard it remarked, by one whose long life has been spent in the acquirement and diffusion of professional knowledge, that, when physiology and pathology should be disencumbered of theo-

ries which are physically impossible, there would be some hope of their advancement. It is, in fact, only by the accurate separation of physical from vital actions in the living system that we can expect to arrive at any such knowledge of the latter as will enable us to classify and generalize the phenomena presented to our notice. No one can attempt this, with any prospect of success, who is not conversant with the laws of inorganic matter, and who is not prepared to carry his conception of their application to an extent still wider than he can discern from the eminence they have hitherto attained. In proportion to the elevation and comprehensiveness of our generalizations, do we find the boundaries which at present divide the sciences disappear; just as the aeronaut, in enlarging his horizon, successively loses sight of the divisions which the art of man or the hand of nature has interposed to separate from each other, estates, provinces and kingdoms.

It is quite evident to every discriminating enquirer, that the phenomena of which the sum constitutes what we know of life, are generally of a very complex nature; that certain vital properties peculiar to organized structures are essentially concerned in the production of most of them; and that the common physical properties of matter are also frequently brought into operation: but that there is a class of actions, in the production of which both physical and vital properties partake, and in the analysis of which there is, consequently, great difficulty. It is, in fact, one of the most important problems in physiology, to determine how far the results of physical laws can be modified by the conditions supplied by the operations of vitality; and it requires a mind thoroughly imbued, not only with the principles of general science, but also with that philosophical spirit which alone can render the knowledge of them available, to pursue the enquiry with the probability of success.

It is not one of the least among the advantages afforded by the cultivation of general science, that, if properly directed, it exercises a beneficial influence on those faculties of the mind, the training of which is essential to the successful pursuit of medical knowledge. The same acuteness of observation, the same cautiousness of inference, the same discrimination in classifying facts, the same sagacity in generalizing them, and the same skill in developing the application of such generalizations, are required in the sciences of life as in those of inert matter.

It is far from our wish to encourage that discursive spirit, which, by directing its attention to too many objects at the same time, is unable to gain a distinct view of any. We feel convinced that, when once fully engaged in the practice of his profession, no one has a right to devote any considerable portion of his time to extraneous pursuits: but even then, if a good foundation have been laid in earlier life, we believe it to be in the power of every one to keep pace with the attainment of laws, and to acquire the knowledge of their application, not only in one or two departments of science, but in all to which his attention has originally been directed. For, be it remembered that every step in generalization is in reality an advance in simplification, and that every law is the comprehensive expression of a vast number of facts, which had previously to be individually retained in the memory; so that, whilst the ingenuity and industry of scientific enquirers are constantly developing new facts, and thus apparently increasing the complexity and extent of each branch of

knowledge, these very facts, combined by the talents of the philosopher with others previously ascertained, conduct him to the discovery of general laws, which include not only the phenomena upon which they were based, but others previously supposed to be beyond their pale, and which lead, by simple deductive reasoning, to results hitherto unexpected.*

To those who agree with the views expressed in the preceding remarks we can strongly recommend the work of Mr. Whewell, as affording much instruction in a useful and attractive form. A mere history of any science is to most persons a "flat, stale, and unprofitable" performance; but, in proportion as it develops the origin, progress, and effects of successive doctrines, the bearing of successive discoveries on the subsequent attempts at generalization,—in short, in proportion as it becomes a philosophical history, does it become generally interesting and valuable. The object proposed by Mr. Whewell is to point out the consecutive steps by which the different sciences have arrived, by the process of induction, at the respective elevations which they have already attained; and, keeping this definite end in view, he necessarily disregards all the events which have no immediate bearing upon it. As there have been few systems of philosophy, however, without some foundation in truth, (most of them being the result of a too-rapid generalization of a limited number of facts,) each one of these has brought prominently forward a certain amount of knowledge, which has in the end been serviceable towards the perfection of science. Hence, in such a history as we are considering, the hypothetical dogmas which have at different times exercised a powerful sway over the human intellect, are not overlooked; and the most correct estimate of the value of each may be formed by considering the effect which it has had in widening the legitimate boundaries of knowledge, and in contributing towards the establishment of the truth by the overthrow of other systems still more erroneous. The mode in which the writer has executed the task he has proposed for himself, is such as might be expected from the distinguished station which he holds among the cultivators of physical science. If we should hereafter think it necessary freely to express an opinion to the disadvantage of the portion dedicated to Physiology, we shall do it with much regret that an author of so high and deserved a reputation should have in any degree disparaged it by venturing to treat of the philosophy of a subject with whose details he is not conversant. We think that Mr. Whewell need not have gone beyond the limits of his own university in search of a writer fully competent to assist him in his plan; and we cannot but believe that, although a single mind may carry into such a work more uniformity of design than could

* We are happy in being able to strengthen our remarks on this subject by the authority of M. Magendie, who says, in one of his recent lectures, "I am so strongly convinced of the necessity of a knowledge of physical science for the comprehension and practice of medical art, that I cannot too strongly urge the study upon you. Not that I pretend that a man cannot be a good practitioner without the acquirements of MM. Thenard, Arago, Poisson, &c.; but I maintain, that you ought not to be unacquainted with the *principles* of the exact sciences. They alone can unveil a mass of phenomena, of which the operation would otherwise be inexplicable; they alone can assist the science of medicine in freeing itself from the trammels in which it has been confined by ignorance and the mania of systematising."—*Leçons sur les Phénomènes Physiques de la Vie*, tome ii. p. 27.

be possessed by a plurality, there must, of necessity, be great inequalities in the execution.

Before entering upon this particular topic, however, we shall briefly follow our author through the subjects of his first two volumes, which are devoted to the exact sciences; and we hope that our statements will not be considered irrelevant to the ultimate object of the present article. Our first quotation shall be from the general introduction, which contains some thoughts peculiarly applicable, as it appears to us, to the study of physiology.

"To the formation of science two things are requisite: facts and ideas; observations of things without, and an inward effort of thought; or, in other words, sense and reason. Neither of these elements, by itself, can constitute substantial general knowledge. The impressions of sense, unconnected by some rational and speculative principle, can only end in a practical acquaintance with individual objects; the operations of the rational faculties, on the other hand, if allowed to go on without a constant reference to external things, can lead only to empty abstraction and barren ingenuity. Real speculative knowledge demands the combination of the two ingredients; right reason, and facts to reason upon. It has been well said, that true knowledge is the interpretation of nature; and thus it requires both the interpreting mind and nature for its subject; both the document and the ingenuity to read it aright. Thus, invention, acuteness, and connexion of thought are necessary, on the one hand, for the progress of philosophical knowledge; and, on the other hand, the precise and steady application of these faculties to facts well known and clearly conceived. It is easy to point to instances in which science has failed to advance in consequence of the absence of one or other of these requisites: indeed, by far the greater part of the course of the world, the history of most times and most countries, exhibits a condition thus stationary with regard to knowledge. The facts, the impressions on the senses, on which the first successful attempts at physical knowledge proceeded, were as well known long before the time when they were thus turned to account as at that period. The motions of the stars, and the effects of weight, were familiar to man before the rise of the Greek astronomy and mechanics; but the 'diviner mind' was still absent; the act of thought had not been exerted, by which these facts were bound together under the forms of laws and principles. And, even at this day, the tribes of uncivilized and half-civilized man over the face of the earth have before their eyes a vast body of facts of exactly the same nature as those with which Europe has built the stately fabric of her physical philosophy; but, in almost every other part of the earth, the process of the intellect by which these facts become science is unknown. The scientific faculty does not work. The scattered stones are indeed there, but the builder's hand is wanting. And, again, we have no lack of proof that mere activity of thought is equally inefficient in producing real knowledge. Almost the whole of the career of the Greek schools of philosophy, of the schoolmen of Europe in the middle ages, of the Arabian and Indian philosophers, shews us that we may have extreme ingenuity and subtilty, invention and connexion, demonstration and method; and that yet out of these germs no physical science may be developed. We may obtain by such means logic and metaphysics, and even geometry and algebra; but out of such materials we shall never form mechanics and optics, chemistry and physiology. How impossible is the formation of these sciences without a constant and careful reference to observation and experiment; how rapid and prosperous may be their progress when they draw from such sources the materials on which the mind of the philosopher employs itself; the history of those branches of knowledge for the last three hundred years abundantly teaches us." (Vol. i. p. 9.)

If, as we shall presently endeavour to shew, the true science of physiology is at present in its infancy, the ends to be attained not generally understood, and the most satisfactory means of pursuing them not fully determined, its cultivators cannot do better than take warning by

the errors which the history of general philosophy brings prominently before them. We regard this science as at present slowly undergoing that revolution which Bacon, Galileo, and Newton concurred to effect in general physics; the ruins of exploded systems have clung so pertinaciously around it, that not even the vast intellect and unwearied perseverance of Haller could succeed in developing its true form and proportions; and, until it is entirely disencumbered of their remains, we do not expect that it will make any decided advance. Some passages, in the part of Mr. Whewell's work which relates to the physical philosophy of the Greeks, might be made, by a slight alteration in terms, equally applicable to the school of physiology which is but just passing away.

"The radical and fatal defect in the physical speculations of the Greek philosophical schools was, that, though they had in their possession facts and ideas, *the ideas were not distinct and appropriate to the facts*. There is no difficulty in perceiving that, for each class of facts, there is some special set of ideas, by means of which the facts can be included in general scientific truths; and that these ideas, which may thus be termed *appropriate*, must be possessed with entire distinctness and clearness, in order that they may be successfully applied. For example: one of the facts which Aristotle endeavours to explain is this,—that, when the sun's light passes through a hole, whatever be the form of the hole, the bright image, if formed at any considerable distance from the hole, is round, instead of imitating the figure of the hole, as shadows resemble their objects. We shall easily perceive this appearance to be a necessary consequence of the circular figure of the sun, if we conceive light to be diffused from the luminary by means of straight rays proceeding from every point. But, instead of this appropriate idea of rays, Aristotle attempts to explain the fact by saying that the sun's light has a *circular nature*, which it always tends to manifest. And this vague and loose conception of a circular quality, employed instead of the distinct conception of rays which is really applicable, prevented Aristotle from giving a true account even of this very simple optical phenomenon." (P. 81.)

The causes which thus retarded the progress of physical science among the Greeks have been perpetuated in physiology down to the present time. There are still men, aspiring to the title of philosophers, who are content to refer all the operations of life which they cannot explain by physical laws to the "Vital Principle;" a term which conveys about as definite an idea as the "circular nature" of Aristotle; instead of viewing them as the results of vital properties by whose action they are produced, (just as the physical properties of matter give rise to its physical changes,) and of which the characters and laws are as open to investigation as those of the more evident qualities, although more difficult of attainment owing to the complexity of the combinations in which the results are presented to our examination.

In considering the claim of physiology to be regarded as an inductive science, we shall take occasion to point out what we believe to be the legitimate method of pursuing it; since there are many who imagine that, in the investigation of vital and physical phenomena, entirely distinct courses are to be pursued, and comparatively few who are aware how similar are the tracks in both; the differences, which all perceive, affecting more the preparatory steps and arrangements than the actual object or direction of the journey. In our enquiry into the laws which regulate the complex phenomena of nature, (or, rather, the general expression of the conditions under which they occur,) our first object is to collect a sufficient number of instances, having an obvious relation to one another, with a view of determining the circumstances common to all. These

instances are furnished to us by *observation*; and, where the phenomena are so simple that their antecedents are uniformly the same, they only need to be associated a sufficient number of times for the mind to be satisfied of the constancy of the relation. In general, however, even where a particular antecedent and consequent present such an invariable relation as necessarily excites the idea of causation, several concurrent circumstances influence the general result so far as, at first sight, to give it a degree of uncertainty; and the value and influence of each of these circumstances has to be determined before we can regard our knowledge of the conditions of the phenomena as complete. Still more frequently does it happen that, of several antecedent phenomena, it is difficult, or even impossible, to fix upon the single one which possesses an invariable relation with those which are the subjects of analysis. Here it is, therefore, that the process of the *artificial* production of phenomena, termed *experiment*,* is so useful, by enabling us to make new combinations of the antecedents, in such a manner as to test every condition which can affect the general result. It is obvious that the information thus attainable will frequently be precisely that which is deficient in our previous collection of observations; and it will be found that, in proportion as any science affords facilities for thus "asking questions of nature," the enquirer in that particular department will have the greater probability of becoming acquainted with her secrets. Another circumstance on which the attainment of general principles in any branch of science greatly depends, is the facility or difficulty it presents in the comparison of phenomena. Where the changes, occurring in a number of individual instances, are so evidently similar in character as to imply an identity of cause, there is comparatively little difficulty in deducing the general law of the effects; but, in more complex instances, where the operation of the real cause is, as it were, masked by the influence of concurrent conditions, or where (as often happens in physiology) the effects of the same apparent cause are totally different according to the instruments through which it operates, it is obvious that there will be great difficulty in the first stage of the inductive process,—that of the classification of phenomena,—so great, indeed, that it may be regarded as one of the principal obstacles to the advancement of those branches of science in which it presents itself.

The phenomena of the equilibrium and motion of inorganic bodies, comprehended in the sciences of *Statics* and *Dynamics*, will illustrate the influence of facility in the comparison of instances and in the application of experiment, in leading to the knowledge of general laws. The principles which have been attained by these means are so comprehensive that a process of simple deduction enables us to predict or account for all the phenomena which can occur; the only difficulty under which philosophers still labour in regard to them arises from their ignorance of

10 * We are aware that this definition of *experiment* does not altogether conform either with its etymology or with the sense in which it is commonly used. It is, however, one which designates its strictly limited application in physical philosophy; and we shall hereafter point out the utility of drawing a broad line in physiological research between those cases in which it is our object to ascertain phenomena as they actually and normally occur, and those in which we endeavour to produce new subjects for observation by a change in the conditions of the phenomena.

the molecular constitution of bodies, which renders the application of these laws to liquid and aeriform media still somewhat uncertain and difficult. The laws of statics and dynamics may themselves be comprehended in still more general expressions; and all the truths of both sciences may be deduced from a single analytical formula, of whose application the law of gravity (the extension of which from terrestrial matter to the solar system, and more recently to the universe, constitutes the grandest illustration of the inductive philosophy,) is but a single instance.*

In Chemistry, too, the direct resemblance of certain groups of phenomena renders their classification (the preparatory step to induction) a matter of little difficulty; whilst, at the same time, the facility of experiment enables us to fill up the voids which observation alone would leave; so that the simple *quantitative* expression of which its facts are usually capable, and the variety of combinations of causes and conditions so easily obtained by experiment, have led to the discovery of laws of a high degree of generality, which are probably themselves subordinate to others yet to be discovered. It is a remarkable consequence of the rapidity of generalization which led to the establishment of the law of definite proportions, that "its subordinate laws,—those which limit its generality in particular cases, which diminish the number of combinations abstractedly possible, and restrain the indiscriminate mixture of elements,—remain to be discovered."† Since the researches of Faraday have proved the identity of chemical and electrical attraction, it would seem that the science of chemistry can scarcely be regarded as having a distinct existence, and that it must ultimately merge in those of electricity and dynamics, which are themselves probably to be included in a single general expression.

In the sciences of Light and Heat, it is remarkable that the most recent discoveries, and the general views founded upon them, seem to do away altogether with the idea of these principles as distinct, imponderable, but material agents. With respect to *light*, indeed, the fundamental doctrines of the undulatory theory may now be regarded as fully established; and it is only in their application to an immense mass of phenomena that any delay can take place, owing to the laborious nature of the process, and the intricate mathematical reasoning required. The undulatory theory of *heat* is found capable of answering all obvious objections, and may be regarded as on its trial, to be confirmed or modified by future discoveries, and especially by an enlarged knowledge of the laws of the polarization of heat.

The present condition of the science of Electricity is peculiarly interesting to the physiologist. The immense number of new and unexpected phenomena detected in recent times by the industry of experimental enquirers, have been made subservient to the discovery of general laws, which not only include the facts upon which they were founded, but others at first sight of an entirely different character. In this manner not only voltaic, atmospheric, and animal electricity have been proved to be but forms of common electricity; but even magnetic phenomena appear to result from a peculiar application of the same powers, or, in other words, a peculiar manifestation of the same properties. Here, then, we have effects of a

* Whewell, vol. ii. p. 121. † Herschell's Preliminary Discourse; p. 306.

most dissimilar character, occurring as the necessary results of a common cause operating under a variety of conditions; and we cannot but hope that a corresponding simplification may, at some future time, be effected in physiological science, by the comprehension of those vital properties which are at present our ultimate facts (and whose laws cannot be said to be yet established,) under one general expression. Moreover, the progress of discovery in electrical science, and other allied branches, renders it probable that electricity is no longer to be regarded as a distinct, material, though imponderable, agent, but that it is rather to be looked on as a general *property* of matter, like attraction of other kinds; and that the actions which we witness, and the powers which are developed, are nothing more than the manifestation of this property, according to the conditions in which the material bodies concerned in them are placed. "It cannot be denied," says Mr. Whewell, "that the theory of the electric fluids affords a plausible explanation of facts; but it may fairly be questioned whether it is *necessary*, and the analogy of light and heat (and especially the polarization of the latter,) has done much to shake the theory of the electric fluids as a physical reality." The most beautiful generalization, however, of which this branch of science seems capable is that recently proposed by Mossotti, who has rendered it highly probable that the phenomena of statics and dynamics, with those of the molecular actions of bodies, and those of all the forms of electric power, may be included under the same general expression.

If the views here stated as to the non-existence of a distinct electric fluid, and the possession of electric properties by all matter, should prove correct, we cannot but consider them as having a most important bearing on physiological speculation. Our analysis of the functions of living beings, or of those changes whose sum constitutes their life, terminates in referring them all to certain properties possessed by their component structures, excited to action by the relations in which they are placed to each other and to external agents. Some of these properties we know to be *physical*, since they are equally manifested by dead and living organized tissues; others are of a different character, being essentially *vital*, and necessarily implying the pre-existence of a living system, by which inorganic matter has undergone the process of organization, and in which alone these properties can be manifested. The existence of these vital properties must for the present be regarded as our ultimate facts in physiology; but we may reasonably enquire whence they are derived. It is commonly said that an organized body, in assimilating and organizing the nutrient matter, by which the changes necessary to its existence are maintained, communicates or superadds to it, at the same time, those vital properties of which it was itself previously possessed. A logical exception might be taken to this form of expression, since we can scarcely regard that as communicable which has not a distinct existence; and, if we understand the term *property* aright, it merely expresses the relation between matter, in some particular form or state, and the percipient mind. But, passing by this consideration, we may advantageously enquire into the analogy on which the view just stated has been often supported. There is no more difficulty, it has been argued, in conceiving how vital properties may be superadded to organized matter, than in understanding how magnetic properties may be communicated to iron: but the

latter process appears to be really of a nature different from what is commonly supposed. The so-called communication of magnetic properties to iron is nothing more than the production of a change in the condition of the metal by which the electric properties, previously existing in that as in every form of matter, are manifested, and caused to give rise to magnetic powers. A little consideration will shew that we cannot become cognizant of any property of matter without some change being effected, either directly upon our organs of sense, or upon them through the medium of its action with some other material body. Every such change, therefore, requires certain conditions, without which the property cannot be manifested; and, until any form of matter has been placed in the conditions requisite to develop a particular property, we have no means of judging either of its presence or absence. It is perfectly correct to say that organized matter exhibits itself to the mind in a relation totally different from that of inorganic matter, and that properties apparently new are thereby manifested. But no one can assert that there does not exist, in every particle of matter, the capability of exhibiting vital actions, when placed in the requisite conditions; in other words, when made a part of a living system by the process of organization. It is only the complexity of the conditions required to manifest it which prevents our recognizing this capability as a common property of matter, or at least of those forms of it which we know by experience to enter into the composition of organized structures.

Of all the branches of science whose phenomena are dependent upon simple physical principles, that of Meteorology is the most obscure and apparently uncertain. Although the changes which become the subjects of observation are constantly occurring under our notice, the difficulty of controlling and artificially combining the agents concerned in them prevents us from deriving much assistance from experiment; whilst the complexity of the conditions under which similar effects occur, and the variety of results which may arise from very slight modifications of the same cause, render such a mode of enquiry peculiarly necessary.

When we turn from the inorganic world to contemplate the living kingdoms of nature, we at once perceive a vast difference in the objects of our investigation; and we may perhaps be led to suppose that this change requires that our mode of philosophising should be varied in accordance with it. But a little reflection must convince us that if the inductive system be really founded upon the relation between the human mind and the world in which man is placed, it must be as applicable to the discovery of general laws in one department as in another; and that, although each of the different steps may be individually modified according to the character of the objects upon which our reasoning is founded, the general plan of the whole must be the same, in whatever particular channel our labours are directed. We have it on record that Newton, after the noblest effort of human reason that philosophy has perpetuated, conceived the idea that the laws governing the structure and functions of living organisms might be discovered by the comparison of their similar organs and functions, as were those of the inorganic world by the study of the uniformity of its phenomena. (“Idemque dici possit de uniformitate illâ quæ est in corporibus animalium.”) Until, however, the principles of philosophical induction are thoroughly understood, the peculiar combi-

nations in which vital phenomena present themselves to our notice, their apparent dissimilarity from the changes which we witness in the world around, and their obvious adaptation to particular ends, might lead us astray into the labyrinth of unprofitable speculation with regard to the presiding agencies by which they are governed; and the slightest acquaintance with the history of physiology will convince us that this has been the case up to a very recent period, and that in fact the legitimate objects of investigation and the true mode of pursuing them are only now beginning to be understood.

We shall connect our views on the study of physiology as an inductive science, with some criticisms on Mr. Whewell's history of this branch of philosophy, on which, as we have before hinted, we cannot bestow the same approbation that is due to the portion of the work devoted to the physical sciences. The author has not attempted to give even a general sketch of the progress of the science, "as such," he says, "would suit neither my powers nor my purpose;" and in the following remarks, he explains more fully his grounds for avoiding the task.

"Like most other ancient sciences, its career has been one of perpetual though variable progress; and as in others, so in this, each step has implied those which had been previously made, and cannot be understood aright, except we understand them. Moreover, the steps of this advance have been very many and diverse; the cultivators of anatomy have in all ages been numerous and laborious; the subject is one of vast extent and complexity; almost every generation has added something to the current knowledge of its details; and the general speculations of physiologists have been subtle, bold, and learned. It must, therefore, be difficult or impossible for a person who has not studied the science with professional diligence and advantages, to form just judgments of the value of the discoveries of various ages and persons, and to arrange them in their due relation to each other. To this we may add, that, though all the discoveries which have been made with respect to particular functions or organizations are understood to be subordinate to one general science, the philosophy of life, yet the principles and doctrines of this science nowhere exist in a shape generally received and assented to among physiologists; and thus we have not, in this science, the advantage which in some others we have possessed, of discerning the true direction of its first movements, by knowing the point to which they ultimately tend; of running on beyond the earlier discoveries, and thus looking them in the face, and reading their true features." (Vol. iii. p. 379.)

Fully admitting that physiology has not advanced as a science to the same level with astronomy, or optics, or electricity, we cannot agree with Mr. Whewell as to the impossibility of truly estimating the discoveries and doctrines of past times. We certainly do not fully understand the value of insulated facts, until they are made the foundation of general laws; but we may have a clear perception of their importance in leading to such inferences, although from various retarding causes the laws themselves have not yet been clearly brought into view. Mr. Whewell has subsequently very justly remarked:

"We are by no means to confine ourselves to the positive discovery, and reject all the less clear and certain speculations. To do this would be to lose most of the chances of ulterior progress; for though it may be that our conceptions of the nature of organic life are not yet sufficiently precise and steady to become the guides to positive inductive truths, the only way in which these peculiar physiological ideas can be made more distinct and precise, and thus brought more nearly into a scientific form, is by this struggle with our ignorance or imperfect knowledge. This is the lesson we have learned from the history of physical astronomy and other sciences. We must strive to refer facts, which are known and understood, to higher principles, of which we cau-

not doubt the existence; and of which, in some degree, 'we can see the place, however dim and shadowy may be the glimpses we have hitherto been able to obtain of these forms. We may often fail in such attempts; but without the attempt we can never succeed."

With regard to the different physiological hypotheses, which have abounded from the time of Pythagoras to the present epoch, the remark we have already made respecting the various systems of philosophy is peculiarly applicable; each hypothesis was the consequence of a hasty generalization of a limited number of facts, and consequently was of use in bringing those facts prominently forward; so that whilst no one had a solid foundation, no one could be regarded as utterly futile. Now that it is universally acknowledged that physiology, like other sciences, must be built up of the materials supplied by observation only, and that the erection of the edifice must be preceded by the demolition of the ruins of the showy but unstable structures of former times, we cannot but consider ourselves quite competent to pronounce upon their merits, since the utility of each to the final consummation will be made apparent by the number and variety of the facts it has brought into view. Thus the chemical and mechanical physiologies of the seventeenth century have long since died a natural death; but each sect contributed much to the advance of our knowledge of the animal economy, and many important truths might be adduced in support of their propositions. The sect of the vitalists originally promulgated doctrines scarcely less erroneous than those to which we have just alluded; and although physiologists have not yet been able to agree in fixing the boundaries of physical and vital actions in the living body, there are none who do not allow that both mechanical forces and chemical powers are in constant and extensive operation. Nor do we feel any greater difficulty in estimating the merits of the system of Brown, who was certainly the first to give clear expression to the truth that the actions of life depend upon the excitement of vital properties by external stimuli; but while this important principle is made the foundation of modern physiology, the pathological system which its author connected with it, has disappeared almost as rapidly as it was raised. Surely, with regard to all these systems and doctrines, we are fully competent in the present day to pronounce an opinion; and as to those of more recent date, we do not apprehend that any difficulty will much longer exist, since the multitude of observers everywhere engaged in applying and testing them, will speedily disclose their true value.

Declining to treat of the general progress of physiology as an inductive science, Mr. Whewell has thought it sufficient "to trace the establishment of some of its more limited but certain doctrines;" and with this intention he has given a brief outline of the discovery of the connexion of nerves and muscles as organs of voluntary motion, by Galen and other ancient anatomists; and of the discovery of the circulation by Harvey and his predecessors. To these he subsequently adds a notice of the discovery of the motion of the chyle, and of some of the theories of digestion; an examination of the process of reproduction (his remark on which, "that it offers to us laws and principles which include both the animal and vegetable kingdoms," might with equal correctness be extended to every one of the organic functions), and a sketch of the discoveries respecting the nervous system from the time of Galen to those of Bichat,

Bell, and Mayo. None of these subjects are, we conceive, treated in such a manner as to give a correct idea of their real progress.

The principal error into which Mr. W. appears to us to have fallen, is one by no means peculiar to him; and we shall take the more pains to expose the fallacy, as clear ideas on this subject are peculiarly necessary to the distinct conception of physiology as an inductive science. The *doctrines*, as Mr. W. terms them, of the circulation of the blood, of the motion of the chyle, of the connexion between nerve and muscle, and of the functions of particular nerves, are not *laws* even of a low degree of generality, but *facts* on which general laws are to be founded. Physiology, it is to be recollected, is the science of life; not the knowledge of the functions of the human body only, but the accumulated and classified amount of all the changes occurring in living organized beings. If we were perfectly acquainted with the structure and functions of any one organism, we should have a collection of *facts* to be included with those derived from the study of other dissimilar organisms, in general expressions, which, to be of real value as the foundation of higher inductions, must be of *universal* application. Every living being, therefore, may be looked upon as a collection of phenomena, each of which may, when properly understood, contribute to the establishment of some general law governing either the development of the material structure or the functional changes to which that structure is subservient; and it is obvious that until these phenomena are correctly and certainly ascertained, they cannot be made available for any scientific purpose, although hypotheses may occasionally be advantageously erected upon them, the conformity of whose results with known facts may afford support to the original supposition. Now it is in the difficulty of correctly ascertaining facts and of satisfactorily observing phenomena, that the true obstacle to the progress of physiology as a science is to be found. Though the structure of the human body has been carefully and minutely examined by so many thousands of anatomists, how many points are still uncertain, and how much yet remains to be discovered! and yet this structure is but one of those groups of *instances*, which must be collected from the many hundred thousand species of plants and animals which the naturalist believes to exist on the surface of the globe, before we can have sufficient data for the establishment of the most comprehensive and universal laws regulating the development of living organisms.

The difficulties which present themselves in the observation of the facts which it is the object of the science of anatomy to ascertain and generalize, are as nothing to those which beset the path of the enquirer into the changes which living beings perform and undergo during the whole period of their existence. These may be practically divided into the normal and abnormal, the former class being usually designated physiological, the latter pathological. It is obvious that this use of the term physiological is much more restricted than the sense in which we have already employed it; and perhaps it would be convenient to confine it to its usual acceptation, and to avail ourselves of the term biology to signify the science of life in its most extended sense. It may be surmised, however, without improbability, that pathological as well as physiological phenomena are referrible to the ultimate vital properties of organized tissues; and that, instead of requiring a distinct set of laws for their expla-

nation, they will prove to be results of these primary causes, as necessary as those perturbations of the planetary system which appear at first sight to interfere with the law of gravitation, but which a more enlarged conception of the application of that law includes amongst its inevitable consequences. The sum of all the phenomena which constitute the life of an organized being, is to be regarded as a collection of *facts*, of which each must be stated in a separate and concise form, before it can be made the subject of any general expression founded upon similar facts derived from the study of other living beings.

There are a few *laws* of a low degree of generality which might be deduced from the study of a single organism: thus the human physiologist might fairly infer that all muscular fibre possesses the property of irritability, because he is able to prove the existence of this property in every one of the numerous fibres which enter into the structure of the human body; and extending his views to the remainder of the animal kingdom, he would probably find that wherever muscular fibre exists, it possesses the same capability of contracting on the application of a stimulus. But were he to attempt to generalize still further, and to assert that every tissue manifesting this property possesses the characteristic structure of muscular fibre, (which tissue alone manifests it in man and the higher animals,) he would find himself widely astray from the truth; since a knowledge of the anatomy of vegetables and of the lowest animals shows that nothing analogous to the muscular structure exists in tissues which distinctly exhibit the property. We cannot, therefore, look for an explanation of this property in any law of the connexion of a particular function with a certain form of structure, which will not include all the tissues found to be possessed of it. Now the great difficulty in physiological investigation results from the complexity of the combinations in which vital phenomena present themselves, and their dependence upon one another to a degree that almost entirely precludes their separate examination. Were we able to ascertain *facts* regarding the changes which take place in the interior of the living body as easily as the astronomer observes the place of a planet, or the chemist the decomposition of a salt, there is no reason whatever to prevent these facts being generalized in the same manner and to the same degree with those of the physical sciences. The chemist, when desirous of ascertaining to which of the ingredients in a given mixture a particular effect is due, places each separately in the conditions required to produce the result; but the physiologist finds that the attempt to insulate any one organ, and reduce the changes performed by it to definite experimental investigation, necessarily takes away or considerably alters those very conditions under which alone its functions can be normally performed, and that he is totally unable to produce similar changes by new and artificial combinations of elementary materials. Frequently, too, the changes when they might otherwise be ascertainable, are of a kind imperceptible to our means of observation; and as we only know of these by the effects they produce on others, there must be considerable uncertainty with regard to their nature and even their very existence. Thus we only know of the nervous influence supposed to be transmitted along the motor nerves, by its effects in producing muscular contraction; as to the nature of these changes in the nervous matter in which the propagation of the influence consists, we

cannot form even a probable guess; and the nervous influence supposed by some to be essential to the organic processes of nutrition, secretion, &c. is only suspected from the sympathy which evidently exists between these processes and certain states of the nervous system.

Another difficulty in the investigation of the phenomena of life arises from this; that whilst, in considering the objects and changes with which the inorganic world presents us, we are able to connect the external or evident characters of bodies with those properties which we know them by experience to possess, and thus to predicate the latter from the former, we are totally unable to follow the same course with regard to organized structures, owing to the extraordinary differences in the properties possessed by tissues, which, as far as our means of observation extend, present exactly the same external characters. Hence it is that we see so many dissimilar phenomena occurring under circumstances apparently the same, and so many changes of the same nature referrible to causes which would seem so different. It is this which has given to all the sciences of life a character of such great uncertainty: we cannot doubt that the laws of nature are as constantly regulating the changes to which living beings are liable, as those of dead matter; but from the apparent inapplicability of ascertained laws to novel cases, and the impossibility of accurately acquainting ourselves with the conditions upon which our application is founded, the physiologist and the medical practitioner are alike baffled when they attempt to predict even the most common phenomena as the result of their operation.

We cannot but perceive, therefore, that the difficulty of ascertaining *facts* in physiology obliges us frequently to adopt opinions and speculations as the foundations of our reasoning; and that these opinions are themselves open to peculiar sources of fallacy. Where, however, a fact regarding the connexion of a particular organ with a special function is definitely ascertained, it may fairly be classified with others of the same nature, so as to serve as the foundation of the inductive process; but care must be taken to include *all* the facts having a relation to the subject, in order that our induction may be really valid. It is by a process of this nature that the essential or elementary structure of all secreting organs is found to be a membrane minutely reticulated with blood-vessels and permeable to fluid; the peculiar arrangement of the surface of this membrane, its convolution into the tubular structure of glands, and the presence of a reservoir and excretory duct for the secreted fluid, being all matters of secondary importance not influencing the general result.

The mass of facts relating to the structure and changes of living organisms, which are collected by the sagacity and ingenuity of observers and experimental enquirers, must be classified and arranged before they can become subservient to the purposes of science; and this object is accomplished in different ways according to the laws of which the philosopher is in search. Thus the physiologist, whose aim it is to discover the ultimate vital properties of matter and the laws of their operation, considers the *individual* changes which in their totality make up the life of an organized being, and, arranging them into groups termed functions according to their similarity with each other and their obvious tendency to the same end, pursues his enquiry into the nature and exercise of each, through all the forms of organized beings in which it is manifested. The

object of the naturalist, on the other hand, is to arrive at the laws which regulate the combination and structure of the organs on which the functions depend; and he therefore, viewing each organism in its *totality*, arranges similarly formed beings under the same group, placing as the character common to the whole the points in which they agree, and leaving the subordinate differences to be added to this common character, in order to express the qualities of an individual. This classification (resembling that of the physiological changes into functions) is but a step towards the establishment of general laws by which the structure of the organized kingdoms of nature is regulated. These laws, expressing the manner in which organs are combined and adapted to each other, the relative development or simplicity of each, the modifications which the primary types or elementary forms of each may undergo according to the circumstances in which the being is to be placed, and various other conditions of its formation, it is the object of the naturalist to ascertain; and any mode or system of classification which he may adopt is valuable in proportion as it keeps the establishment of these laws in view, and facilitates the accumulation of the knowledge upon which they must be founded. The connexion between these two branches of investigation is so intimate that neither can be pursued with probability of success, without a considerable knowledge of the data and principles upon which the other is founded; and he will evidently be the most likely to arrive at the discovery of important general truths in either, who includes the whole of the phenomena of life in one extensive survey. The physiologist refers to the naturalist for instances in which a function is performed on the same general plan, but under a great variety of circumstances, as manifested by the adaptation of the structure of the organ to the medium of existence, (e. g. the formation of the respiratory membrane into lungs or gills;) whilst the naturalist refers to the physiologist to assist him by the examination of the function and development of an organ in determining its real character, to which the consideration of its form and structure alone might not lead him. The natural system of botany affords a beautiful example of this kind of investigation; and there can be little doubt, from the advances recently made, that some of the most important laws, regulating the structure of living beings and the combination of their organs, will be speedily disclosed to view.

The objects of physiological enquiry are twofold: first, to ascertain the changes actually taking place in the living system; and second, to determine the conditions of those changes; and hence to arrive at the general laws of vital action. Both of these classes of enquiry we regard as principally dependent upon observation for their materials, and the inferences to be deduced from it are, we conceive, much more definite and certain than those which experiment affords; but it will be seen that we comprehend under the former term many kinds of investigation which are usually considered experimental, and that there are in physiology many circumstances which render a purely experimental enquiry less available than in any other branch of science.

The physiological observer, then, has for his first object to determine the nature of the series of changes of which the sum constitutes the life of any living system. His most simple and direct means of observation, namely, his unassisted senses, inform him of those obvious changes only

which in some instances mark the relations between the being and the external world, and in others are to be regarded as the results of the processes taking place within the system. Thus a physiologist, content with simply watching the growth of a plant in its natural situation, would see little beyond its successive increase in size and the development of its different parts: its absorption of nutriment from the soil or the atmosphere would not, under ordinary circumstances, become apparent to him; and, in fact, the only obvious relations between the living system and the external world would be evinced by the influence of light and moisture upon the leaves and flowers. With regard to animals, simple observation would exhibit to him not only the results of the organic functions made evident by the increase and development of the bodily frame, but those changes which immediately connect them with the external world, the ingestion of aliment, and the discharge of excrementitious matter. He would also perceive the manifestations of the animal power of locomotion, and might infer from the analogy of his own actions that sensations were being produced, and were giving rise to mental processes. These, then, are all the means of information which unassisted observation can afford; we may conceive them to be such as brutes possess, and we can readily understand how small must be the real amount of knowledge to which they can lead.

But the ingenuity of man has enabled him to devise methods of adding greatly to this meager outline, by contriving methods of more exact observation, and of thus discovering what would otherwise be imperceptible to the senses. We will take for illustration one of the simplest possible of these aids. No one, on merely looking at a plant in active vegetation, would suppose that its leaves are constantly exhaling a large quantity of fluid from their tissue; but, by holding a polished surface in their neighbourhood, it is found speedily to become dimmed with moisture. This *observation*, then, (for it is nothing more,) leads to the knowledge of the function of exhalation. Again, a plant vegetating under common circumstances would not be suspected of constantly taking up a large quantity of fluid by its roots; but, if these be placed in a limited quantity of damp earth, so that the loss is made perceptible, the moisture will be found to disappear from the soil much faster than by ordinary evaporation. Here is another observation of the simplest kind bringing us to the knowledge of the function of absorption; and though in such a case the alteration in the conditions necessary for the observation might cause the process to be regarded as an *experimental* one, a little reflection will shew that the change is not in the body whose functions we are examining, (for we are anxious to preserve these unaltered,) but in the external conditions made favorable for our observations; just as the chemist, to observe the colour of a solution, would transfer it from an opaque vessel to a transparent one.

Having, then, arrived at the knowledge of these functions by simple additions to our ordinary means of observation, a further refinement of our methods will add to our acquaintance with them: thus, to continue our former illustration, the judicious employment of the balance will enable us to determine the quantity of fluid absorbed and transpired by a particular plant, and the proportion it bears to its surface. The researches of Hales on this subject, and, in like manner, those of the

various physiologists who have investigated the gaseous changes produced by vegetables, have usually been denominated *experimental*; but it appears to us that the term is no more applicable to them than to the measurements of the altitudes and distances of the heavenly bodies, performed by the astronomer. It is true that, in the latter case, the objects are entirely beyond the reach of the observer, and his aim is to adapt his measurements and calculations to convey a correct idea of their true relations; whilst, in the former, the objects are certainly under his control, but he endeavours to preserve them as much as possible in their natural conditions, and simply to ascertain the changes they effect on the surrounding fluid or æriform media, by measuring the quantities of these media which are lost or added. Whenever any change in the conditions of the living system takes place, (as when a plant becomes sickly from inclosure in a confined atmosphere,) the observation becomes valueless for the purpose it was intended to serve.

The physiological observer may employ other means of assisting his senses, by which he is enabled to detect changes inappreciable without such aid. Thus, he avails himself of the microscope for examining the capillary circulation, and endeavours to place the part employed for observation as nearly as possible in its natural relations; avoiding the influence of gravitation and of the loss of fluid from neighbouring divided vessels, both of which have an evident effect on the movement of the nutritious fluid, in plants as well as animals. Other means of magnifying changes of various kinds, which would be otherwise inappreciable, might be easily referred to: thus, the use of the thermo-electric multiplier has enabled MM. Becquerel and Breschet to detect minute alterations of temperature during muscular action; in which observation it was requisite to guard against various errors arising from change in the bodily condition, which might have interfered with the correctness of the result; and, according to the statement of Mr. King,* multiplication of insensible motions by means of a lever enabled him to detect venous pulsation, even after allowing for the disturbance which might be occasioned by a neighbouring artery. We speak of all these as observations rather than experiments, because their object is to ascertain the natural changes going on in the living body under their normal conditions; and any means we may employ of rendering them perceptible to the senses are just as independent of the changes themselves as are the preparations made by an astronomer to view and measure the heavenly bodies, by erecting his telescope, and rendering it fit for use by uncovering its glasses. As soon as our methods of observation do affect the conditions of the change, the result is attended with more or less of uncertainty, in proportion as we are ignorant or aware of the extent and consequences of the influence produced.

But the physiological observer does not confine himself to the simple collection of phenomena externally presented to his notice, and which are often, in fact, the mere results of those which take place in the living system: he endeavours, by an examination of the interior of that system when in a state of activity, to ascertain the nature of those on which the former are dependent. He will be more or less successful in discovering

* Guy's Hospital Reports, vol. ii. p. 108.

these, in the inverse proportion to the derangement of the train of vital actions which shall have been created by the violence he is compelled to inflict; since it is evident that, in the examination of the normal changes of any one part, it is essential that the functions of the whole system shall go on with as little disturbance as possible, owing to that mutual dependence of which we have already spoken as one of the greatest obstacles to physiological enquiry. It is in this manner that the absorption of the chyle, the motions of the heart and alimentary canal, and various other interior phenomena of a similar kind, have been discovered; and we think it quite evident that such researches cannot be denominated *experimental*, since no new conditions have been brought into action, but the original ones are preserved as far as circumstances will permit.

We have already remarked that there is a series of changes in the living body, with which no means of observation that we at present possess can make us directly acquainted. We, of course, allude to those which take place in the nervous system. No one doubts that when a stimulus, either external or internal, excites contraction in a voluntary muscle, it is through the medium of an organic change of some sort, propagated along the nervous cords. But, if we examine the foundation of this opinion, we shall not find it in any phenomena with which the normal condition of the system presents us: it is derived by inference from *experiment*; that is to say, a change in the relative conditions of nerve and muscle, effected by intercepting the influence supposed to be propagated along the former, or by exciting a new influence by various stimuli applied directly to itself. We might, for the sake of argument, object that these data are insufficient to afford a positive conclusion, since the wound necessary for the division of the nerve might of itself produce the effect: it would be easy, however, to shew, by a parallel experiment, that this is not the case in the present instance. Still we could only positively infer the transmission of nervous influence, by causing it to act upon a part to which it previously was not conducted; an experiment obviously beyond our power to perform, except by bringing together the two ends of a divided nerve. In another case, moreover, the impossibility of obtaining such a positive inference leaves us in uncertainty if we trust to experiment alone: we refer to the question of the innervation supposed by some to be conveyed to the organs of nutrition and secretion. Here the complications of the experiment are so great, and so liable to affect the result, that we very much doubt if any satisfactory conclusions can ever be drawn from it.

It will be seen, then, that our knowledge of the changes occurring in the living system is founded upon *observation* only; except where we cannot take cognizance of the changes themselves, and can only infer their existence from the inaction which ensues when the organs to which we attribute them are abstracted or insulated. We shall now enquire what is the most legitimate mode of pursuing the second branch of physiological enquiry,—the conditions of these changes.

It is obvious that experiment will be here more available; but even here judicious observation may prove, in a majority of cases, an effective substitute for it. The first subdivision of this investigation will evidently be the examination of the dependence of the vital functions upon external agents; a subject on which the researches of Dr. Edwards have shed so much light. Yet we do not call the researches of Dr. Edwards entirely

experimental, since his aim was, in many instances, to determine the regular sequence of the vital functions, in conformity with the usual changes of age, reason, &c. They were experimental whenever the animals were placed in entirely new conditions, and means taken to observe the influence of those conditions; and it was in these cases that the greatest care was necessary to guard against fallacy in the results, by taking the average of a large number of instances. It is in experiments of this kind that we can most successfully imitate the chemist or mechanical philosopher, in testing the conditions necessary to a particular result, by excluding each in succession; as when Dr. E. shewed that the presence of oxygen in the atmosphere is not immediately essential to the excretion of carbonic acid from the lungs; and that the want of alteration usually perceived in the quantity of nitrogen in respired air is due to the equality between its absorption and exhalation. There are some instances in which the same mode of enquiry is applicable to the conditions of internal changes; as when the first sound of the heart was shewn to depend principally upon muscular action, by the successive exclusion of all the other suspected causes.* But, in this case, it is to be observed that the effect whose conditions were to be examined was a merely physical one, and that these conditions could be closely imitated by artificial means.

Far different is it with regard to the conditions of the purely vital functions. As we have before remarked, the attempt to insulate any of these, and make it the subject of special examination, destroys at once the capability of its performance; and we cannot imitate it by any new combinations of matter. Hence experiment can conduct us very little farther in this enquiry than the determination of the dependence of the functions upon one another; and we believe that all which it has yet detected (especially regarding the functions of the nervous system,) may be reduced to this or the former place in our classification. And, as we have before hinted, many of the results obtained from it are liable to great uncertainty, from the general functional derangement which almost inevitably attends the performance of the necessary operations, arising out of the closeness of that bond of union which links together, in the higher animals especially, all the changes concerned in the maintenance of the vitality of the system. In the lower classes of animals, where this connexion is much less decided, and in vegetables, where it is still looser, we can insulate particular organs, and study the conditions of their actions to a much greater extent; and hence these are the most favorable subjects of experimental research on the organic functions.

But the physiologist is fortunately not confined within these limits: the ever-varying forms which the study of animated nature submits to his observation present him with manifold combinations of causes and concurrents, whilst the phenomena they exhibit shew him the results of these combinations. He may, therefore, advantageously avail himself of them for the purpose of ascertaining what are the conditions essential to the occurrence of any particular change, and what are accidental, or at most have a secondary influence upon it. The chief obstacle to his progress in this mode of investigation arises from the difficulty of correctly observing the phenomena of whose causes or laws he is in search; since the very change

* See vol. ii. p. 598, and Transactions of British Association, vol. v.

of circumstances which may be essential for observation would be liable to derange the train of vital actions, which it is his object to observe in their normal state. Moreover, he is perplexed by the immense number and variety of the phenomena which are thus presented to his notice, and the dissimilarity of the effects which a very slight, in some instances an inappreciable, change in the conditions produces. This perplexity, however, is of the nature of the *embarras des richesses*; and we agree with the observation which has recently been made, "that it must be the fault of our mode of study if we do not arrive at some tolerably definite conclusions" from these most abundant data.

Respecting the method in which these data are to be classified, when once accurately substantiated by observation, we have already entered into some detail; and we shall now conclude the subject by one or two illustrations of what we regard as the superiority of observation over experiment, in elucidating disputed and important questions in physiology.

It is well known that a controversy has long existed regarding the dependence of the capillary circulation on the heart's action; and the attempts made to determine it by experiment have never been satisfactory to either party. We shall see that we may easily arrive at a definite result by observation only. In vegetables, the vital circulation (that of the elaborated sap*) is entirely capillary, and is kept up without any central organ of impulsion; and observation of the influence of light, heat, &c. on the development of the foliaceous organs proves that the afflux of crude sap to them in the spring is the effect of the vital processes there performed. In the lowest of the animal kingdom possessed of a distinct circulation, such as the Echinoderma, the system is destitute of a central organ of impulsion; and we find this evolved in ascending to the articulated and molluscous tribes. It is difficult to observe the circulation in insects, or in such transparent Entomostraca as the *Monoculus Pulex*, without coming to the conclusion that, though a contractile cavity exists, the movement of the fluid is mainly independent of its pulsations.† The higher we ascend in the animal scale, the more do we observe the principle of centralization in its functions becoming apparent; and accordingly, in the Vertebrata, we find the power of maintaining the circulation less diffused through the system, and more concentrated in the heart. Still, however, various phenomena indicate, to those not indisposed to appreciate them, that capillary action has an independent influence, although its nature may not yet be understood; and that, even in man, the circulation is not altogether dependent upon the heart.

A very analogous question has long been agitated regarding the dependence of the organic functions upon the ganglionic system of nerves. The obstacles attending any endeavour to determine it by experiment are too obvious to need indication; but we shall briefly point out what knowledge we may attain from observation. In vegetables, we have an example of the performance of all the organic functions without anything analogous to nervous influence; since all attempts to prove the possession by them of a nervous system, under any form, have signally failed; and these

* See vol. iv. p. 27.

† It appears not improbable that the non-development of a distinct organ of impulsion, so remarkable in the articulated classes as compared with the Mollusca, may be the natural counterpoise to the high degree of capillary power (if we may be allowed the expression,) which is created by the very active nutritive processes of these classes.

attempts were only excited by an indisposition (the obvious remnant of former dogmas) to admit the possibility of any vital actions being independent of this system. As to the existence of a nervous system in the lowest animals, we shall not make any positive statements, since the difficulties which attend the minute examination of their tissues are much greater than those which the vegetable anatomist encounters. The simplest form of nervous system which we meet with appears to be connected rather with the locomotive than the nutritive apparatus; and it is now universally admitted that the double nervous cord of the Articulata, and the cyclo-ganglionic system of the Mollusca, represent the cerebro-spinal system of the Vertebrata; and that these classes possess the separate rudiments of a distinct visceral system of nerves. Believing that the office of the latter is to harmonize and connect the different organic functions more completely than can be effected by the circulating apparatus, which is their only bond of union in plants, and to bring them into relation also with the functions of animal life, we should expect to find it developed in proportion to the general concentration and specialization of the functions, and to the development and predominance of the animality; and so accordingly it is. If, on the other hand, the organic functions had been *dependent* upon it, we should have expected that it should have been developed according to their predominance; which observation absolutely contradicts.

We must now return to Mr. Whewell's book, from which we shall draw one or two illustrations of the views which we have been propounding. By classing the knowledge of the circulation of the blood, of the motion of the chyle, &c. among "the more limited but certain doctrines of physiology," Mr. W. appears to us wanting in "clear ideas" as to the true import of these discoveries. To those who have followed us through the preceding remarks, we think it will be evident that the knowledge of the circulation of the blood in man holds just the same rank in physiology as the fact that a stone unsupported falls to the ground, in general physics. In its practical applications, the former fact is of the highest importance; but, as contributing to the establishment of any general law, it is valueless until combined with others. We shall suppose that Harvey, who established the existence of a general circulation in the higher animals, and Hales, who so ingeniously investigated the same function in plants, had thence inferred that circulation was a function common to all organized beings, and necessary to their existence,—they would have committed the common error of generalizing falsely upon a limited induction; for a more extensive acquaintance with comparative anatomy and physiology would have informed them that the object of the circulation being merely to convey the absorbed nutriment to the respiratory organs and to the tissues it is to maintain, there is no occasion for any motion of fluid in those beings (some of which occur both in the animal and vegetable kingdoms,) which absorb equally from the whole surface, and which possess no special respiratory apparatus. In fact, the development of the circulating system, and the complexity of its function, bear a strict relation with the degree of specialization of the absorbent surface, or, in other words, with the confinement of the absorbent power to some particular part of the general surface, or of its internal or external prolongations.

Mr. Whewell has very properly cited the laws of vegetable morphology

as illustrative of the generalization of which physiological facts are susceptible; and he justly remarks, that the terms "*metamorphosis* and *development*," which are here employed, convey ideas entirely different from any of those to which the physical sciences have led us in our previous survey; and are, in short, genuine *organical* or *physiological* ideas; real elements of the philosophy of life." There is certainly no science which has more need of definite terms than physiology, involving as it does so many abstract ideas and speculative opinions; and there is assuredly none which has been more retarded by the variety of senses which have been attached to the same word by different authors. It is much to be desired that the explorers of this important and extensive domain, could fix upon a set of definite land-marks, by the aid of which they could render their discoveries and prospects intelligible to each other and to their respective followers. What the present state of chemistry would have been without a settled nomenclature might not be altogether an unprofitable speculation; and although we do not consider that such a reform is needed in physiology as that which Lavoisier effected for chemistry, it is particularly desirable that there should be no room for hesitation as to the meaning to be attached to commonly received terms in any particular instance.

The last chapter of Mr. Whewell's work consists of an exposition of the author's views on the employment of final causes in physiological research; and we are surprised to find that he considers the philosophical pursuit of physiology as inconsistent with the recognition of their value. But surely, whilst the study of final causes is of great value in leading to the *discovery of facts*, the search after general laws to be based on these facts must be totally independent of them; and the higher we advance in the attainment of generalizations, the more exalted our ideas necessarily become of that designing mind which planned and adapted the organized as well as the inorganic creation. The vagueness of Mr. Whewell's ideas regarding the nature of physiological discoveries, has pervaded his opinions on the use of final causes; and he has consequently looked with a suspicious eye upon all attempts to philosophise independently of them. Now we shall quote, from no less an authority in physical science than Mr. W. himself, what we regard as a very proper estimate of their value in that department of investigation; and we confess that we cannot discover any distinct line between physics and physiology, that necessitates such a variation in the mode of research.

"Final causes are to be excluded *from physical enquiry*; that is, we are not to assume that we know the objects of the Creator's design, and put this assumed purpose in the place of a physical cause. We are not to think it a sufficient account of the clouds that they are for watering the earth, (to take Bacon's examples,) or "that the solidness of the earth is for the station and mansion of living creatures." The physical philosopher has it for his business to trace clouds to the laws of evaporation and condensation; to determine the magnitude and mode of action of the forces of cohesion and crystallization, by which the materials of the earth are made solid and firm. This he does, making no use of the notion of final causes: *and it is precisely because he has thus established his theories independently of any assumption of an end, that the end, when after all it returns upon him and cannot be evaded, becomes an irresistible evidence of an intelligent legislator.* He finds that the effects, of which the use is obvious, are produced by the most simple and comprehensive laws; and when he has obtained this view, he is struck with the beauty of the means, by the refined and skilful manner in which the useful effects are brought about; points different from those to which his researches were directed." (*Bridgewater Treatise*, p. 353.)

It appears from Mr. Whewell's History of Physiology that he regards the adaptation of means to ends as so prominent a characteristic of organized beings that he considers no investigation into the laws of their structure can hope to be successful which does not keep this doctrine steadily in view. But we would ask him, with all possible respect, whether the object of his Bridgwater Treatise was not to demonstrate the harmony of means and ends in the structure of the universe, and thence to prove the existence of a designing mind? He has successfully shown that this adaptation is not less complete than that of the parts of a single organized being to one another. And would he not totally disregard, notwithstanding, the study of final causes in seeking for the laws of which that adaptation is the necessary consequence, although the observation of the facts upon which he establishes these laws might have been suggested, and the phenomena themselves brought to light by the perception of this general harmony and adaptation? We cannot point to a clearer instance of the necessity of a similar course in physiological investigation than that afforded by the generalization which has been effected in vegetable morphology, the laws of which Mr. Whewell allows to be firmly established and recognized. What would have been the situation of this science at the present moment, if the philosophic botanist had adopted the final cause or function of the several parts of the flower as his guide in investigating the laws of their structure, instead of tracing that structure through all its regular and irregular forms with a total disregard of its function? In considering the laws of the vegetable kingdom, we have abundant opportunities of observing how diversified, both in their forms and uses, are the various types which the same rudiments may assume; and that even when undeveloped, these rudiments appear as the necessary results of these laws, and assist man in the attainment and comprehension of them. No one, therefore, ought to be presumptuous enough to affirm that, though he has discovered an evident purpose in a particular structure, there may not be some other, less obvious, but really more important; nor, when he is altogether at fault as to the design for which some apparently useless part may have been created, has he any right to say that no object was to be fulfilled by it.

We cannot, therefore, see any ground for the indignation with which Mr. W. regards the speculations of M. Geoffroy St. Hilaire, on account of their neglect of what he seems to consider the *end* of physiological research, rather than the *means*. We do not pretend to know what may be the private opinions of that author with regard to the wisdom and power of the Creator; but we do assert, that the legitimate idea conveyed by his expression, "I ascribe no intention to God, for I mistrust the feeble powers of my reason—I observe facts merely, and go no further," has no more irreligious tendency than the passage we have quoted above from Mr. W.'s Bridgwater Treatise. We go further, and assert that the higher and more general the laws regulating the structure of animals which the physiologist can attain, the more will the contemplative mind be struck with the vastness of that designing Mind, which, in originally ordaining them, could produce such harmony and adaptation amongst their innumerable results. To use another very forcible expression of Mr. Whewell's, "the notion of design and end is transferred by the researches of science, not from the domain of our knowledge to that of our ignorance, but merely from the region of facts to that of laws."

To avoid all chance of being misunderstood in these views, we think it will be sufficient to adduce, in illustration of them, one of the most obvious and simple adaptations everywhere presented in the structure of animals,—that of the muscles to the skeleton. We constantly find, in pursuing our anatomical enquiries, that, for the advantageous attachment of muscles to bones, some particular form of the latter is provided; and that, where much power or a particular direction is required, a considerable prominence is given to the point of attachment. The teleologist would say, with apparent truth, that each of the bony processes was intended for the attachment of a muscle; and would thus be led to infer the form and direction of certain muscles of extinct animals, from the prominences which existed on their bones. He might go further, and say that the formation of the prominence is occasioned by the existence of the muscle; and might allege, in support of his view, the well-known fact that the osseous points of attachment are strongly developed in those persons who have much exercised their muscular system. On the other hand, the philosophic anatomist, fully acknowledging the adaptation between the osseous and muscular systems, would disregard it for the time, whilst seeking for the laws regulating the development of these systems; which laws he would seek to deduce from the observation of all the forms of each, both normal and abnormal. Thus, he would find that each of the important processes in the human skeleton exists as a separate bone in some of the inferior animals; and that the complicated muscular system of man gradually simplifies itself as he descends towards less specialized organisms. Supposing that the physiologist has succeeded in establishing such laws “independently of any assumption of an end, that end, when after all it returns upon him, and cannot be evaded, becomes an irresistible evidence of an intelligent Legislator.” For we would fearlessly ask the opinion of any candid and reflecting person, whether it does not imply a far higher degree of Creative Wisdom and Power to suppose that, in the establishment of the laws of osteology and myology, (themselves probably subordinate to some higher principle,) all the results of each were foreseen and harmonized, so that every muscle, developed in accordance with the laws of *its* system, should find an attachment in the osseous process resulting from the action of the laws of *its* system,—than that the formation and adaptation of each separate muscle and each individual process required a distinct effort of creative skill?

We shall bring this subject to a close with a few observations on the most advantageous mode of studying the science whose present state and prospects we have been considering. It is but too true, that, in consequence of the limited amount of physiological knowledge which it has been hitherto thought necessary to communicate in most of the medical institutions of this country, few but university graduates have acquired any comprehensive view of the science; and of these no small proportion have been satisfied with receiving the information imparted to them, and retaining those portions only which they felt to be interesting or believed to be practically important. Recent changes, made in some of the metropolitan schools, encourage the hope that physiology will now rank no longer as an unimportant and therefore neglected branch of medical education, and that the knowledge of the functions of the human body in health will henceforth be regarded as a necessary preliminary to the

investigation of the changes which it undergoes in disease. To the student who seeks merely to acquaint himself with what is already known in this branch of science, we have little advice to offer; since the perusal of the numerous works to which he has ready access, will afford him as much knowledge as he will remember, and a great deal more than he will be likely to profit by. But we hope that all of the rising generation are not of that class; and that there may be many amongst them earnestly desirous of aiding in the advance of a science unsurpassed in interest, scarcely rivalled in importance, and almost illimitable in the extent of the field it presents for cultivation.

It is quite evident that no one can advantageously commence the study of physiology without a tolerably complete knowledge of human anatomy, both general and special. Those details, however, which are peculiarly connected with physiological inference may perhaps be not improperly deferred until the time when their application tends to implant them on the memory. Either conjointly with, or subsequently to, the study of the human organism, we recommend that a general knowledge of comparative anatomy be acquired; and though the magnitude of the task may alarm the student, he will find that if he avoids devoting much attention to details of external form, and endeavours to make himself acquainted with the general development of each system, the pursuit will be easy as well as delightful. It will not be amiss to acquire at the same time a knowledge of the structure of vegetables; not only because we find there expressed in another and frequently a simpler form, the anatomical facts which it is difficult to trace in animals, but because the attainment of the laws of morphology in flowering plants, and their progressive extension to the cryptogamia, may advantageously serve as our guide in the more intricate pursuit of similar generalizations in the animal kingdom. We have already stated our belief that a knowledge of the principles of general physics is essential to the successful cultivation of physiological science; and when all these preparatory steps have been taken, the student will enter upon its study with no small advantages.

Whatever may be thought of the expediency of commencing the study of anatomy by investigating the structure of the simplest organisms, a plan which has many advocates, we are decidedly of opinion that this course is essential in physiology, and that the student who adopts it will be saved the necessity of unlearning many erroneous notions which he would unavoidably imbibe from the premature study of the human functions. In the pursuit of general physiology he will learn what are the essential conditions of life; he will see the changes indispensable to its support manifested in their simplest circumstances; and he will be able to ascertain what structures are necessary to their performance, and what additions and modifications these may undergo to suit the various purposes of their existence. He will acquire, also, the great advantage of making observation a substitute for experiment; the former means, wherever it can be employed in physiology being decidedly preferable, (as we hope we have successfully demonstrated,) both in the certainty and satisfactory nature of the conclusions which may be drawn from it, and in its freedom from those objections which every humane mind must feel to the infliction of unnecessary tortures upon beings endowed with sensations as acute as our own.

We have alluded, in the early part of this article, to the difficulty of

distinguishing the operation of vital and physical laws; and this we cannot but regard as a question to be completely determined before the laws of purely vital phenomena can be satisfactorily established. To analyze the phenomena in which physical laws are acting under conditions supplied by vital processes, and to trace the diversities from their usual mode of action occasioned by the existence of those conditions, appears to us therefore to be at present the most obvious method of advancing the science. We cannot but believe that the enquiry would ultimately terminate in referring all vital actions to properties as essentially connected with that form of matter which we call organized, as are the ordinary physical properties with inorganic matter.

One more question would then remain: is it possible that the physical and vital properties of matter, which are at present our ultimate facts or axioms, may be hereafter included within a more general expression common to both? On this subject we can only speculate; but the probability appears decidedly in the affirmative. We have already remarked upon the rapid progress of generalization in the physical sciences, rendering it probable that before long one simple formula shall comprehend all the phenomena of the inorganic world; and it is not perhaps too much to hope for a corresponding simplification in the laws of the organized creation, although this is necessarily retarded by the many obstacles which the nature of the subject presents to the philosophical enquirer. In proportion to our attainment of such generalizations, we rise from the domain of our ignorance to that of our knowledge; for, at every successive step, we are able to comprehend new relations between facts that previously seemed confused and insulated; new objects for what formerly appeared destitute of utility.

Every step, then, which we take in the path of generalization must increase our admiration of the beauty of the adaptation, and the harmony of the action of the laws we discover; a beauty and harmony in which the contemplative mind delights to recognize the wisdom and beneficence of the Divine Author of the universe. If we can conceive that the same Almighty *fiat* which created matter out of nothing, impressed upon it one simple law, which should regulate the association of its masses into systems of almost illimitable extent, controlling their movements, fixing the times of the commencement and cessation of each world, and balancing against each other the perturbing influences to which its own actions give rise,—should be the cause not only of the general uniformity but of the particular variety of their conditions, governing the changes in the form and structure of each individual globe protracted through an existence of countless centuries, and adjusting the alternation of “seasons and times, and months and years,”—should people all these worlds with living beings of endless diversity of nature, providing for their support, their happiness, their mutual reliance, ordaining their constant decay and succession not merely as individuals but as races, and adapting them in every minute particular to the conditions of their dwelling,—and should harmonize and blend together all the innumerable multitude of these actions, making their very perturbations sources of new powers;—when our knowledge is sufficiently advanced to comprehend these things, then shall we be led to a far higher and nobler conception of the Divine Mind than we have at present the means of forming.

ART. II.

Dissertatio de Methodo Endermatico. Auctore A. AHRENSSEN, M.D.

—Hauniæ, 1836.

A Dissertation on the Endermic Method. By Dr. AHRENSSEN.

—Copenhagen, 1836. 12mo. pp. 267.

WHEN we consider the multitude of systems and theories which have been alternately devised and repudiated, and the variety of modes by which skilful and sagacious men have endeavoured to counteract the influence of noxious agencies on the human system, we are forcibly impressed with the truth of the Hippocratic apothegm, "Experience is fallacious, and judgment difficult." And we are led to be cautious, in yielding assent to new doctrines, or adopting new methods in the healing art, until their intrinsic value shall have been determined, not merely by the author himself, but by the concurrent testimony of other competent judges. It seems to have been in this philosophical spirit that Dr. Ahrensen has made the endermic method the subject of diligent and persevering investigation; examining all things, but holding fast only what he has confirmed by experimental research.

The mode of introducing medicaments by the skin is of very ancient date. Thus, to go no further, we read in Celsus, that a person in good health, "*neque medico, neque iatrolipta egere*,"* wants neither a physician nor a rubbing-doctor. The *iatraliptic method*, as we infer from its derivation, consisted in applying medicinal substances to the sound skin; some medicines being found to produce more powerful effects in that way than when administered internally; and these not only of a vegetable nature and easily soluble, but even metallic and almost insoluble matters. Thus, Haller states that Berengarius was the first person to discover that mercury was capable of exerting its specific effects through that channel; and that Amatus Lusitanus had observed grave symptoms consequent upon the external use of arsenic. Hippocrates and Galen describe the aperient effects of baths impregnated with hellebore; and Avicenna extols the efficacy of a bath of oil in resolving spasm.

By reflecting on the multiplied authenticated instances of the therapeutic effects so produced, the idea suggested itself to MM. Lambert and Lesieur, two Parisian physicians, that these would be enhanced provided the epidermic layers were previously removed, and the medicinal agent brought into direct contact with the cutaneous absorbing apparatus. This they denominated the *endermic method*, and the results of their original researches are recorded in a memoir which they published on the subject, in the year 1824. To an abstract of these Dr. Ahrensen has appended the subsequent observations of himself and others in the same field, so classed, grouped, and methodized as to form a comprehensive view of the whole.

After premising some observations on the structure and functions of the skin, in accordance with the recent discoveries of Breschet, Gurlt, and Wendt, the author proceeds:

* Lib. i. cap. i.

“ Whence we see that the skin resembles, in many respects, ‘the internal mucous lining of the alimentary canal. Each membrane consists of the same cellular tissue; and each is connected, although principally the latter, with organs containing numerous vascular and nervous ramifications. Both abound in organs of secretion, differing among themselves, and adapted to the leading functions of each; whose use in the economy does not, indeed, differ so much as we should at first view imagine, inasmuch as they both serve to deplete the organism, by relieving it of effete or obnoxious matters. Both are, moreover, furnished with a protecting envelope, and, if it be thicker in the skin, we nevertheless find even that typified in the compact epithelium constituting the inner surface of the stomach of granivorous birds. The analogy becomes still more palpable when we take into account the circumstance that mucous membrane, exposed for a length of time to the influence of the atmosphere, acquires the appearance and texture of skin; and that hairs have been detected growing from the mucous coat of the intestine.” (P. 14.)

The author notices the arguments in proof of cutaneous absorption, based on the conclusive experiments of Bradner Stuart and Westrumb; and refutes the opinion of those writers who pretend that the therapeutic effects ascribed to cutaneous absorption ought to be exclusively referred to nervous agency or pulmonary inhalation. The transmission of substances through the skin is accomplished by the capillaries alone, by simple imbibition, or by imbibition conjoined with endosmosis; and rarely, unless some morbid irritation coexist, as Westrumb has endeavoured to shew, are substances at once taken up by the lymphatics, and conveyed into the current of the circulation. In the digestive canal, the ingesta fitted for assimilation, after undergoing certain changes, are received by the lymphatics, and whatever part withstands this necessary decomposition enters the blood in the way above pointed out, but more readily and promptly, inasmuch as the epithelium or mucous lining is more delicate than the epidermis, and capillaries are there present in greater number. According to Müller, saline solutions applied to the corium penetrate the capillaries in one second of time.*

The first step in the endermic method is the removal of the cuticle. This may be effected either with a common blister, covered over with a poultice to allay pain; with caustic ammonia, united with axunge; by means of a steel blade, heated by immersion in boiling water; or by the combustion of alcohol or ether, on a portion of lint of the requisite size. The selection of the spot for application will depend, in a great measure, on the nature of the remedy and the seat of the disease. Hence, morphia ought generally to be applied to the head; strychnia, near the vertebral column; belladonna, to the eye; emetics and laxatives, to the abdomen; diuretics, to the region of the kidney.

The medicines best suited for endermic use are such as exist in a concentrated form, as the alkaloïds, and are of a soluble and unirritating nature. They should be sprinkled on as a dry, impalpable powder. If the dose be very minute,—that is to say, considerably under a grain,—some gum or starch may be triturated along with it, in order to secure uniform diffusion. Remedies endowed with irritant qualities, as squill, kermes mineral, sulphate and muriate of quina, ought to be mingled with a little axunge or gelatine. With the exception of morphia and strychnia, the quantity employed ought considerably to exceed that

* Handbuch der Physiologie. Coblenz, 1834. Tome i. p. 233.

prescribed internally. The manner in which medicaments are to be brought into contact with the corium varies according to circumstances. In the first place, the epidermic vesicle is to be detached with a forceps, and the contained serum gently wiped away with a bit of soft sponge or lint. Nor is the lint to be allowed to remain on the raw surface for a certain time, as some propose, with the view of absorbing any fresh-formed serum, since its presence invariably creates irritation. Should there be any coagulable lymph deposited, it must, in like manner, be cleared off. These preliminaries over, the application may be made, unless the excoriation be inflamed, or speckled with small ecchymosed points, threatening gangrene.

"Powders, little irritant, are to be lightly dusted on the denuded corium, from their paper envelope. When applied on a sloping part of the body, they ought previously to be formed into a paste with water; a measure which aids absorption, as I ascertained by experiment: for, having selected some cases on purpose, I applied the muriate of morphia in one set, in the form of dry powder; in the other, moistened with water. On removing the bandages, after the lapse of twelve hours, I found, in the former, the fourth part of the mass still remaining unchanged; while in the latter it was wholly absorbed. The application made, the part is to be covered with a piece of lint, thinly spread with cerate, retained in apposition by means of strips of adhesive plaster and a bandage." (P. 62.)

The author remarks, that some writers on this subject recommend a simpler sort of dressing. It consists in covering the excoriated surface with a portion of wet blotting paper; which, on drying, adheres so firmly as to supersede the necessity of plasters or bandage. But, as its separation is apt to occasion pain and hemorrhage, and thereby prove detrimental to the subsequent application, it should in no case be resorted to without interposing a piece of waxed paper. The intervention of gauze has been found advantageous in employing acrid substances, as squill. An appropriate apparatus has been devised by M. Lesieur for remedies in the gaseous form. It is a vessel furnished with two opposite tubes fitted with stopcocks; the one for the admission, the other for the egress of the gas. On renewing the application, the medical attendant must first carefully remove any pseudo membrane formed on the denuded surface from the coagulated serum, which is often so thin and transparent as to be hardly visible to the naked eye.

In the endermic use of medicines, we must distinguish the local and primary from the general and secondary effects, in order to obtain a true estimate of their curative powers in any given disease. The local phenomena supervene promptly, and vary according to the physical properties and amount of the remedy. Frequently they consist of itching, with a slight sense of heat and tingling; sometimes, however, of violent agonizing pain; together with redness, more or less diffused, owing to the injection of the small vessels with blood. Various other symptoms have been enumerated by Lembert and Richter, none of which our author ever witnessed.

"The patients, they say, are seized with a sense of burning, darting pain or heat, which the last-named author affirms, if in the first instance the remedy applied be narcotic, extends itself in a radiating manner from the denuded point along the tract of the principal nerves in the vicinity, producing an universal elevation in the temperature of the body. Lembert, on the other hand, states that the painful sensations are

first of all transmitted to the adjunct visceral cavities, and then, pursuing the tract of the vessels and nerves, spread over the body at large." (P. 69.)

The above-described effects form, as it were, the transition to the general effects, and last from ten or fifteen minutes to two or three hours.

The action of endermic remedies cannot be explained on the principle of sympathy; since it has been found that kermes mineral, sprinkled over a raw surface behind the ear, provokes expectoration; and belladonna, applied in the same manner to the foot, dilates the pupil. Neither can we shew, on the ground of sympathy, why affections of organs very different may be induced by different remedies applied to the same point of the skin. That there are instances in which the nervous system is immediately influenced by endermic remedies, is abundantly demonstrated by the sudden sedative effects, local as well as general, exercised by certain narcotics, before the vascular system has had time to be affected. Hence the opinion entertained by Lemberth that medicines must be absorbed, either in whole or in part, ere any result can ensue, falls to the ground on his own shewing that sulphate of quina can check the paroxysm of ague in the course of ten minutes; that is to say, long before any adequate absorption can have taken place. It must not be supposed that we deny the agency of absorption; we wish merely to point out that there is a twofold action with narcotics, as proved by physiology and exemplified in the endermic method. For, if a suitable dose of morphia, or any of its salts, be directly applied to the diseased part, in order to allay topical pain, its anodyne effect is almost instantaneously perceived; at all events, half an hour before general symptoms, as somnolence and vertigo, occur: whereas, with the same application to some more remote part, the assuaging of the local pain is synchronous with the development of general symptoms.

The influence of absorption is, indeed, intimately connected with this branch of therapeutics; and whatever tends to diminish or augment that function will reciprocally interfere with the efficacy of endermic remedies. Hoffmann* has furnished some useful practical information on the subject, which is deserving of notice in this place. He observes, that it proceeds with greatest rapidity in individuals of a sanguine temperament, who have a soft and delicate skin, and in women and children; during evening and night; and in a moist warm temperature. It is most favoured when the functions appear to be all in equilibrium: wherefore, under any predominant affection of an organ or system, as anormal activity of the circulation, excessive plasticity of the blood, (as in inflammation, suppuration, pregnancy,) or great heats, high electric tension of the atmosphere, it is interrupted, limited, or hindered. It is likewise greater while the stomach is empty than during the process of digestion. The preponderance of any secretion, as in diarrhœa, running piles, partial sweats, diminishes absorptive energy, according to the same authority; but both physiology and pathology are in direct contradiction to such an assumption. Magendie's experiments clearly proved that, if the mass of circulating fluids be lessened, absorption is augmented in a corresponding degree.

Soemmering and Magendie assert that local inflammation diminishes

* Hufeland's Journal, January, February, 1833.

the force of absorption. Our author, on the contrary, maintains that slight inflammation or irritation is hardly, if at all, prejudicial.

“For I am convinced that, by reiterated application to the same blistered surface, remedies act more powerfully; and surgeons are well aware of the fact that many chronic tumours not only decrease in volume, but that even the organs in which these had been contained become atrophied by inflammation artificially established.” (P. 72.)

Guided by the light of physiology, we at once see that the endermic inspersion is best performed in the evening, or else in the morning, ere the intestines be loaded with ingesta. Indeed, so close a relation subsists between the digestive canal and the skin, that, the more the former is tasked, the feebler will the function of the latter be.

In order to realize the benefit of a moist and warm temperature, Lambert conjoins the use of the tepid bath before the inspersion be made: “perhaps, says Dr. Ahrensen, a large poultice laid upon the denuded spot may in like manner prove advantageous, though in a less degree.” He expresses surprise that compression and electricity, two agents of acknowledged utility in promoting absorption, should not as yet have been made subservient to the endermic method.

In reference to toxicology, we learn from the method in question, that morphia is the best antidote for strychnia, that musk and camphor counteract the narcotism of morphia; and that prompt ablution and detersion of the part, together with the application of the cupping-glass, will in most cases suspend and avert any baneful consequences.

The endermic plan of treatment is necessarily very circumscribed, inasmuch as it only admits of medicines of which a minimum dose will produce a powerful effect, and which are susceptible of ready absorption, or of exercising a decided effect on the superficial nerves. Hence the narrow range of *materia medica* it involves can derive extension alone from the future progress of chemical discovery. With the exception of narcotics and antispasmodics, whose curative virtues endermically administered are fully attested, but slender reliance can be placed on any other medicines.

“On this ground it appears that nervous are almost the only maladies which are completely under the control of the endermic method, and that only in as far as they are of dynamic origin and independent of organic disease: hence, the more local the affection, and the more superficial the portion of the nervous system implicated, the more rapid and efficient will be the result of the treatment. For this reason it is, that patients labouring under local neuralgia, paralysis, spasms, and periodical complaints, will be most speedily and certainly relieved.” (P. 93.)

It may be granted, however, that some other remedies which act upon particular systems, as diuretics for example, will occasionally work a cure when endermically employed. Where there is a difficulty or impossibility of introducing medicine in the ordinary way, as in trismus or in affections of the fauces or œsophagus, which preclude access to the stomach, and in irritable states of that viscus, the endermic method is indicated. Its main advantages, however, are thus portrayed by Dr. Ahrensen.

“1. Medicines may be applied directly to the affected part, and by the consequent suppuration will act more or less as derivatives. This is of importance, as regards patients suffering from neuralgic pain and local spasm, as these may be immediately alleviated by narcotics without the patients experiencing any bad effects from the latter.

"2. As the stomach and intestinal tube are exempted from the irritation which may follow medicinal ingesta, these, in their turn, escape decomposition from the secreted fluids they would encounter in the first passages. Thus are their effects undisguised; a point of importance in reference to the researches of the physician and the pharmacologist. Lesieur unphilosophically pretends that we shall be able to discriminate in this way the active constituents of remedies, by submitting the residuum to chemical analysis.

"3. The odour, taste, or perverse action of particular medicines offers no contra-indication here, since they can be exhibited without the knowledge of the patient. This is especially useful in the case of children, provided the medicine be not of too irritant a nature.

"4. This method enables us to extend the employment of dangerous remedies by administering much larger doses than we could venture upon internally, seeing that the means of obviating their deleterious effects are more within our reach.

"5. Where medicines given internally, from long continuance, cease to act, and where we cannot safely augment the dose, we may advantageously resort to the endermic plan, as a means of mitigating suffering in chronic and incurable diseases."* (P. 94-6.)

Among narcotics, the salts of morphia hold the first place. The acetate, sulphate, and muriate have been employed; but the two last are preferable from their superior solubility. Their endermic application is attended by an itching, or smarting, and sense of heat, which seldom lasts beyond a quarter of an hour. In sensitive females, however, it will sometimes continue for four or five hours. According to Richter, acetate of morphia does not produce either inflammation or increased secretion in the denuded skin; but Dr. Ahrensen, on the contrary, states that, if the salt be frequently applied, suppuration ensues, and a new epidermis is formed on the fourth or fifth day. The general effects resulting from their external, differ from those following their internal use, both as regards time and manner of acting. Lembert somewhat poetically compares their rapidity of operation to the swiftness of lightning; and Trousseau and Bonnet expressly state, that phenomena were manifested in two minutes' time after the endermic application, which took days according to the usual way, partly owing to the decomposing power of the stomach, partly to the prevailing absorption of the skin.

"Such prompt effects were seldom observed by Hoffmann, who reckons the interval of time to range between ten minutes and six hours. Although I do not dispute the greater celerity of the endermic plan, I have in no case witnessed symptoms indicative of the agency of morphia earlier than five minutes, (and there the large dose of a grain or a grain and half had been employed;) and frequently have I known two hours elapse ere the general symptoms made their appearance. The variation in time depends not only on the sensibility of the patient and the dose of the narcotic, but, as above mentioned, on the nature of the disease and the site of the application. If the organism be saturated, so to speak, with morphia from protracted use, a minute quantity then administered will act with greater energy and speed." (P. 102-3.)

Some writers contend that the excellence of the endermic application of morphia consists in its affording relief without the brain being consensaneously involved; but it is seldom that the brain remains free, and that only where a small dose, not exceeding the fourth or the half of a grain, has been applied directly to a part remote from the head. Otherwise,

* A case in point is related by Hoffmann, who, by alternately administering anodynes by the mouth and skin, succeeded in a remarkable manner, in relieving the tortures of a woman labouring under carcinoma.

we have, in addition to the narcotic symptoms, signs of cerebral disturbance. These may be of a mild form, as cephalalgia, vertigo, slight stupor, with pupils dilated or contracted, and nausea; or they may assume a more serious character, as great stupor, bilious vomiting, lively delirium, and tremors of the limbs remaining for days after; succeeded by more or less irritation of the vascular system, which, however, Lembert and Hoffmann deny; and, lastly, the secretion and excretions become altered in different ways. Morphia, as is well known, is very apt, when given internally, to nauseate the stomach; and Dr. Ahrensen ascertained that, if the endermic dose exceeded three quarters of a grain, squeamishness was induced; and, with a grain and a half, vomiting, constipation, ischuria, eruption of sweat, pruritus of the skin about the nose, and an occasional papular efflorescence.

The dose of morphia ought, in the first instance, to be small, if used near or on the head, not exceeding one-fourth or half a grain in adults; for children, one-sixteenth of a grain will suffice. If not found to disagree with the patient, it may be gradually increased to two, three, or even five grains. It has been found useful in the following diseases: Traumatic tetanus (conjoined with antiphlogistic treatment, if there be signs of inflammation present;) delirium tremens; mania; hysteria; spasmodic dysphagia; spasmodic cough; spasm of the eyelids; neuralgia—frontal, temporal, alveolar, and intermittent; pleurodyne; venereal pains; cancer; chronic rheumatism, when unattended with fever, of the kind termed fibrous, and of a local description, or in the instance of acute articular rheumatism confined to a single joint; chronic bronchitis; vomiting and cardialgia; dysuria; ague.

The remedy next in order is belladonna.

"It was first applied by Lembert; but, as it occasions, in the pure form, both irritation and violent pain, which Richter has known to last for an hour, it is always advisable to temper it with lard. It is said to affect the voice in a peculiar way, giving rise to a kind of involuntary stammering; and, if the quantity applied be large, and not far distant from the head, the pupil of the opposite side is usually dilated. Bally observed dilatation of the pupil, even when applied to the back of the foot. We find also delirium with loquacity, (the tongue, however, is slow,) with universal tremors; bilious vomiting, colic pains, diarrhoea, and abdominal distention, have been noticed by the same author." (P. 159.)

According to Gerrhard, belladonna, endermically used, is anodyne and antispasmodic, and diminishes the mucous secretion of the bronchi. Richter found a liniment, consisting of a scruple of the extract to two drachms of *aqua lauro-cerasi*, smeared over the denuded corium, answer well in irritable states of the air-passages, spasmodic vomiting, and gastrodynia. The extract of hyoscyamus has been endermically prescribed in a similar formula to the preceding. The extract of belladonna is generally employed, the root also sometimes. It is rich in atrophine.

Extract of stramonium occasions great pain. It is said to prove serviceable in obstinate neuralgia and sciatica; and Gerrhard has applied a scruple at a time to the perineum. Crocus, in a dose of six grains, is reported to have cured a case of intermittent occipital neuralgia.

Strychnia in the pure state was first endermically applied by Lembert and Lesieur; in consequence, it is said, of the favorable result which followed the insertion of some extract of *nux vomica* into a wound, in a case

of palpebral paralysis under the care of M. Marjolin. The salts of strychnia, being more soluble, are for that reason preferable to the alkali itself, in an endermic point of view.

"If we look to the local effects of strychnia, we shall find them much more energetic than those of morphia; the pain following the application is burning and pungent, lasting about half an hour or an hour. Where blisters have been applied to the paralyzed limbs or the vertebral column, the raw surface inflames under the use of the remedy, and affords a copious suppuration, so much so as to require a repetition of the blister on the sixth day. Whatever way the remedy be used, its general effects are equally violent, and always directed to the medulla spinalis and ganglionic system. Endermically administered, we can persevere longer in its use, and convey a greater quantity into the system, with less danger of poisoning the patient." (P. 165.)

After some days' continuance, strychnia frequently causes painful spasmodic contractions of the extremities, resembling tetanus, disorder in the first passages, and vomiting. According to Richter, when the dose amounted to three grains, the patients complained of obstinate constipation, anorexia, heat of stomach, and squeamishness. From an experiment made by our author, we are authorized in considering strychnia a cumulative medicine. Its prolonged use sometimes gives rise to a peculiar modification of the sensibility, so that the characteristic spasms are apt to recur some time after the patient has left it off, on his taking violent exercise, or doing any thing to quicken the circulation of the blood. The endermic dose of pure strychnia should not, at the commencement, exceed half a grain, and of its salts one-fourth of a grain. It has been used with advantage in partial paralysis, especially in the paralysis from lead, and wherever we have no reason for suspecting the existence of organic disease. In hemiplegia, it has occasionally proved serviceable; as also in amaurosis, depending on paralysis of the optic nerve or retina, and in symptomatic amaurosis from some lesion of the fifth nerve; in hysteric aphonia and stammering; in prosopalgia, and other neuralgic affections; in chorea sancti viti. It is a curious circumstance that universal tremors often supervene contemporaneously with the melioration of the disease under treatment.

Veratrine and aconitine, from the intolerable pain they determine, are inappropriate for endermic use; but their therapeutic virtues can be adequately obtained by the *iatroleptic* method: our readers are well aware how much they have been employed in this way, since the publication of Dr. Turnbull's work.

Musk is the antispasmodic standing first on the endermic list. It has been administered in doses of six, ten, or fifteen grains, with good effect, in asthma. Other substances in the same class, castor, assafœtida, oxide of zinc, have been likewise tried, but do not seem to have answered.

Among endermic febrifuges, quina, together with its sulphate and muriate, stand forth pre-eminent.* Of the local effects of the latter we are told that, "soon after the inspersion of the powder, a violent burning pain ensues, which annoys the patient from fifteen minutes to an hour; nay, sometimes several hours; the skin acquires a red blush round the

* Hufeland (*über einige Kinderkrankheiten*, Leipzig, 1792,) relates instances of the efficacy of cinchona frictions in ague; which is confirmed by Auber and Chrestien. The *iatroleptic* powers of the muriate of quina, in infantile intermittents, are instanced by Michaelis. (*Magnusson de Methodo Endermatica*, p. 13.)

denuded spot, which, on the following day, is covered with a soft, yellow crust, dotted with white points, and consisting of particles of the salt imbedded in coagulable lymph; frequently tedious ulcers are left, which do not heal before several weeks."

Richter tells us he has known the skin deeply eroded by the action of the salts of quina. The pain is obviated by mixing the powder with axunge or sulphate of morphia. Its general effects are to increase the heat of the surface of the body, and the muscular and nervous energy. The salt of quina ought to be applied to the epigastrium during the apyrexia in ague, in the quantity of four or six grains. The blister should not be put on during the paroxysm.

"From a careful survey of numerous cases detailed by me, it clearly appears, that not only simple intermittent fevers frequently yield under the endermic use of this remedy, and that with a less dose than if internally exhibited; but complicated cases are more manageable in the same way." (P. 225.)

It has been also employed with benefit in intermittent nervous ailments. Iodide of quina, piperinum, salicinum have been severally put to the test of experiment, but without affording any satisfactory results.

Expectorants constitute the next class of endermic remedies which claim our attention. Kermes mineral, sesqui-sulphide of antimony, applied to a blistered surface on the lower part of the neck, (*quere*, the suprasternal fossa?) or to the chest, serves as an expectorant, in the dose of a grain, in pulmonary catarrh, in chronic bronchitis, and, alternated with morphia, in phthisis. It is a powerful topical irritant, and causes severe pain; hence it should not be employed in children. Its acrid qualities may be abated, however, by combination with axunge. Powdered kermes, according to Lemberg, tends to concreate into hard incrustations on the corium, and their removal is not unattended with pain. Dr. Ahrensen, on the other hand, has seen rather a copious suppuration in consequence.

No reliance can be placed on the endermic effects of emetina, or tartarized antimony; yet Lettsom witnessed diaphoresis and diarrhœa, and Brera* vomiting, produced by inunction of the latter medicine.

Diuretics. Squill has been endermically administered in the form of the powder of the bulb, and of extract. The dose of the former is ten or twenty grains, twice or thrice a day; of the latter one or ten grains. The remedial effects are manifested, whether the substance be applied to the abdomen, lumbar region, or the arm.

"The local effects of both preparations are extremely irritant, but chiefly the extract; for the pain resulting from the powder subsides in general in from sixty to ninety minutes, while that from the extract may abide eight or ten hours. There is diffused redness, followed by inflammation and suppuration, in the place of application, which prevents the repetition of the remedy for more than two or three days consecutively to the same blister. Others, I believe, have persevered longer. The general effects do not differ materially from those usually attributed to its internal exhibition; some writers are of opinion that irritation of the alimentary canal is thus avoided; others, again, deny this; as M. Huss, who has known nausea and urgent vomiting incidental to its endermic application. Its diuretic virtues are established by the endermic method, both in my own experience and by that of others. It also promotes expectoration and transpiration." (Pp. 242-3.)

Its employment is indicated in anasarca and hydropic collections;

* Anatripsologie v. Brera, Wien 1800, p. 170.

but, owing to the vehemence of its local effects, it ought never to be prescribed, except the first passages be disordered.

Digitalis may be endermically prescribed, in the pulverized form, to the extent of eight or ten grains in the day. The denudation of the corium ought to be as near as possible to the affected organ.*

"The local effects of digitalis are sharp pains, which continue from fifteen minutes to half an hour, but are rarely so troublesome as to require the suspension of the medicine. The excoriated surface begins, in the course of a few days, to discharge matter, and in a fortnight is quite dried up. Its protracted use determines, as Raciborski has observed, a green coloration of the derma, resembling *nævus*, which abides for weeks, unless repeated washing has been resorted to. Its general effects, in whatever way administered, are nearly identical,—namely, depression of the vascular system, augmentation in the secretion of urine, together with the absorption of extravasated serum: these are seldom manifested before the lapse of two or three days, and, as happens with its internal exhibition, persist after the cessation of the remedy. Its external administration seems to exert a greater influence on the functions of the skin, as evinced by the increased transpiration; and is unattended by vomiting and diarrhoea, or any derangement of the alimentary canal or brain, incidental to its internal exhibition. And these circumstances are not unimportant; as, Andral uniformly found that, where digitalis disagreed with the stomach, the vascular system was little depressed." (P. 249-250.)

This remedy is chiefly indicated in those cases where we desire to make a powerful impression on the heart and vascular system; as in simple hypertrophy of that organ, or in hypertrophy associated with valvular disease; Bouillaud extols its employment in tertian ague.

According to Gerhard and Coster, iodine endermically applied, exerts a diuretic action. This our author refuses to admit.

Concerning purgative medicines, we find, from the concurrent testimony of most authors on the endermic method, that jalap, elaterium, colocynth, croton oil, and gamboge, irritate more or less the part to which they are applied, but do not determine any cathartic action;† as to rhubarb, it is perfectly inert. The remaining endermic remedies of which Dr. Ahrensen treats are aloes and calomel. Aloes applied to the denuded skin causes burning heat, and subsequently promotes healthy suppuration, and is laxative. A dose of ten grains is followed by an evacuation from the bowels in the course of half an hour, and by six or eight more during the next eight hours. The watery extract of aloes, applied to the epigastric region in the quantity of two to six grains, has been found effectual in relieving obstinate constipation where other remedies have failed; and in icterus.

Calomel, on account of its desiccative qualities, requires to be previously mixed up with sugar or axunge. The sense of heat following its endermic application is very transient. In the dose of three or four grains, it produces, it is said, a copious alvine discharge. The antisiphilitic virtues of calomel thus administered for a length of time, in the quantity of one-sixteenth to two grains, are very questionable.

"Corrosive sublimate, which, from its caustic nature, appears ill adapted to the

* The *iatroleptic* use of squill and digitalis was first noticed by Auber and Brera. (*Op. cit.*)

† Yet, Van Swieten comments upon the perils from hypercatharsis, to which persons exposed themselves in his time, by applying plasters and ointments to the epigastric region in order to move the bowels, containing *colocynth*, *scammony*, *aloës*, and *jalap*.

endermic method, has been nevertheless found useful by Bally in syphilitic exostosis and osteocopic pains. Reynaud cures bubo by applying to the blistered surface a compress moistened with a solution, containing ten grains of sublimate to an ounce of distilled water." (P. 267.)

On taking a retrospect of the results heretofore obtained by the endermic treatment of disease, most sensible persons will, we think, agree with us, that the few advantages it may offer are hardly sufficient to countervail the uncertainty and inconvenience with which it is fraught. If we except, perhaps, morphia, strychnia, squill, and aloes, no other medicines remain of which we can predicate any positive therapeutic effect; and the annoyance or irritation concomitant even upon their application, in many instances, will militate against any general introduction of the practice.

It would be doing injustice to Dr. Ahrensen to close this analysis of his treatise without expressing our high opinion of its merits. He has shown much industry and judgment in selecting his materials, and in appreciating the positive and relative value of the remedies treated of. His book, like most of those that issue from the medical school of Copenhagen, bears the stamp of good sense and sound learning.

ART. III.

Elements of Medicine. Vol. I. *On Morbid Poisons.* By ROBERT WILLIAMS, M.D., Senior Physician of St. Thomas's Hospital.—London, 1836. 8vo. pp. 342.

THE first volume of this work (the only one yet published) is devoted to the consideration of certain diseases, regarded by its author as both contagious and infectious,—viz. Typhus, Scarlatina, Morbilli, Variolæ, Varicella, Erysipelas, Pertussis. Any one at all conversant with the diversity in the phenomena of these diseases, with the various hypotheses which have prevailed respecting them, and with the different modes of treatment which have been employed in correspondence with such hypotheses, will receive with no little willingness, and perhaps be disposed to scrutinize with too little care, an attempt to explain them consistently with well-known facts, with which they appear to hold the closest relationship. Such is the object of Dr. Williams, as stated in his preface.

"The course, the symptoms, and the pathological phenomena of typhus, as well as of many other diseases depending on the action of a morbid poison, have been determined with so much accuracy, that little hope remains of new facts being added to those already discovered. But, although we are thus in the possession of the necessary data, little progress has been made in deducing a sound theory, or in determining any general rule of treatment in those instances for which no known specific remedy exists. It appearing, however, to be an almost demonstrable truth that, when a given disease could be shewn to depend on the agency of a poison, in whatever manner generated, the laws and treatment of such affection must necessarily follow those of poisons generally, it remained to prove this hypothesis. The result is now submitted to the public; and, if the argument shall be considered as established, it must be considered that contagion is as important an agent in medicine as gravity in mechanics, or electricity in chemistry. The opportunities which a large hospital affords have made it a duty to deviate, in some instances, from the usual routine of practice, and to reduce the treatment to the simplest forms; and the results of these

experiments, especially in fever, erysipelas, and scarlet fever, have been so remarkable, that it is submitted, whether at present they ought not to be adopted as the basis of our practice?"

In a carefully written introduction, of which we shall give a brief analysis, Dr. Williams has stated the facts, and some of the reasonings, on which his theory is founded. Others, of course, are derived from the consideration of the diseases with which the volume is occupied; and these diseases we shall mainly consider as illustrative of the hypothesis promulgated, particularly when any mode of treatment is suggested which appears to be more worthy of employment than that generally adopted.

Certain diseases are produced by morbid poisons. The laws governing these do not greatly differ from those of poisons generally. Poisons, of whatever nature, are subjected to certain general laws: they have a *definite specific action*; a *period of latency*; the *phenomena vary according to the dose and predisposition of the individual*. Of the first law, jalap, acting on the mucous membrane of the intestines, is an illustration. Digitalis acts on an organ, the heart; strychnine on a system of organs. More generally, two or more membranes, or organs or systems of organs, are affected; as by elaterium, antimony, opium, &c. Poisons may co-exist in the same system, and their actions may be simultaneous on the same tissue: e. g. opium and digitalis, jalap and mercury, produce their respective effects at the same time. Some poisons are cumulative. The *latency of poisons* is evinced by opium, &c. When a medicine acts on more parts than one, a considerable space of time may elapse, after it has affected one organ, before it affects another: thus, mercury affects the bowels for many weeks before its ultimate action on the salivary glands. Oxalic acid and arsenic afford familiar examples of the effect of *dose*. The larger the dose, or the greater the intensity of the poison, the more rapid is its action, and the less the probability of finding any specific trace of disease. Mercury, in its various actions, is a familiar example of the influence of *predisposition*. Habit exercises on some an influence, but not on others. The state of the constitution has also a powerful influence on the action of poisons; a circumstance which needs no illustration, as it occurs to every one in daily practice. Generally, medicines may be said to act with a power proportioned to the debility of the subject. Still, states of disease, though greatly debilitating, render the constitution insusceptible to the action of even powerful remedies; e. g. typhus fever and vinous stimuli, opium and tetanus, &c.

The general laws observable in the actions of morbid poisons are, for the most part, precisely similar to those which govern medicinal substances, or only differ in a few minor points. They have their *specific actions*; *latent periods*, while their phenomena vary with *the dose, and with the predisposition of the patient*. The *specific actions* are shewn by the distinctions between small-pox and ague, ague and syphilis, &c. Like medicinal substances, their actions are variously limited; some affecting only one membrane, or organ, or system of organs; whilst others involve two or more membranes, or organs, or systems of organs. Tinea capitis, bronchocele, whooping-cough, measles, scarlatina, ague, &c. may be mentioned as illustrations of the foregoing statement. Morbid poisons co-exist in the same individual, and even produce their specific

effects on the same membrane at the same time. Thus, erysipelas exists with syphilitic eruptions; measles with small-pox; syphilis with whooping-cough, &c. &c. Morbid poisons have also a *latent period*, which requires no illustration. When they act on more tissues or organs than one, their actions are sometimes simultaneous, but more commonly consecutive; and much time often elapses between each successive attack. Typhus and syphilis are examples of this. Poisons, which usually act on a plurality of membranes, sometimes exhaust themselves on one, without affecting the whole series; e. g. rubeola sine catarrho. There appears to be no absolute order in the series of membranes affected by morbid poisons, when more than one is so affected.

The effects of medicinal substances vary with the dose. With morbid poisons, the living body is the only measure which we possess of the strength of the dose. It is impossible to speak of this law, except in conjunction with the temperament or susceptibility of the patient; but, thus considered, the law holds equally good with respect to morbid as to other poisons. There can be no question but that the paludal miasmata of tropical countries greatly exceed in intensity those of more temperate climates; and accordingly, in many cases, hardly a trace of disease is to be found after yellow fever; so intensely severe and rapid is that disorder. But, in the Walcheren fever, a disease perhaps equally fatal, but more chronic and of less intensity, enlarged livers, disorganized spleens, and dropsy marked every case. It may be stated as a law, that, when a morbid poison acts with its greatest intensity and produces its severest forms of disease, fewer traces of organic alteration of structure will be found than when the disorder has been of a milder character. Generally, but there are exceptions, morbid poisons act with an intensity proportioned to the feebleness of the patient.

Such are the chief points of agreement of the laws of morbid poisons with poisons generally. The following are the chief points of difference:—There is no authentic cumulative poison. The quantity of a morbid poison necessary to produce disease is often so small as to be scarcely appreciable. When the quantity is sufficient to produce the specific disease, the result is determined by the temperament or constitution of the patient. The faculty which the body possesses of generating, to an immense extent, a poison of the same nature as that by which the disease was produced, is one wholly unknown to medicinal poisons; as also is that law by which the susceptibility of the constitution, when acted upon by some morbid poisons, becomes exhausted; the body being incapable of again being similarly affected. Lastly, climate or season does not influence the action of vegetable or mineral poisons; but they not only modify the intensity of morbid poisons, but influence also their specific actions. Typhus does not exist at the equator. In different seasons, in this country, it is attended with different structural lesions. It is important to remember, in the study of morbid poisons, that absolute uniformity of pathological phenomena, and consequently of symptoms, is not to be expected. It is sufficient for the purposes of science to have proved the general law, and to have determined the limits within which nature has bounded her deviations.

There is considerable ingenuity in this attempt to demonstrate (as stated in the preface,) “that, where a given disease could be shewn to

depend on the agency of a poison, however generated, the laws and treatment of such disease must necessarily follow those of poisons generally;” and the full consideration of the diseases to which this volume is devoted will afford further illustration of the author’s views. But we cannot yet agree with Dr. Williams that, granting the existence of morbid poisons, the laws and treatment of them necessarily follow from those of poisons generally. Certain points of resemblance between the effects of one and of the other have been shewn to exist; certain remarkable distinctions have likewise been demonstrated. The question arises, therefore, as to what amount of difference would contradict his conclusion? what degree of similarity is necessary to justify it? On the one hand, if we admit that, in possessing a specific and limited action, a latent period, and an action depending on dose and predisposition, they closely resemble one another; (and, with regard to the third point of similarity, it would be very easy to raise a question;) we must also admit that the protective influence afforded to the constitution by one attack and the peculiarity on which depends the multiplication of the poison to an unlimited amount, constitute very important points of distinction. Until we know whether these differences depend on the poisons themselves, or on the quality of their relations to the body; until, in fact, we know something about them with which we are not at present acquainted, we cannot be justified in the inference, that the same principles of treatment are applicable alike to the diseases produced by both. Instead of applying the term poisons to both, let us call them agents. The question would then stand thus: There are two classes of agents, differing in their chemical constitution, which, when taken into the body, produce certain effects. Some only of these effects resemble each other. It is not, therefore, a legitimate inference that the same laws and treatment are applicable to both. It is begging the whole question to term these minor points of difference. The cause of one of these minor points of difference *may* be that on which depends the appropriateness of this or that system of treatment. We are, therefore, of opinion that any inferential reasoning from poisons generally to morbid poisons, as it regards treatment, is not strictly applicable; but, in other respects, and particularly in the explanation of certain pathological difficulties, the analogy may be applicable, as we shall hereafter see; and not at all the less valuable may be the means recommended by Dr. Williams. We, however, must examine these simply with respect to their results, and as deriving no importance from the reasoning which has led to their employment.

Dr. Williams regards it as proved—(1) that morbid poisons act according to fixed and definite laws, modified only by the influence of climate, temperament, or the magnitude of the dose; (2) that they mingle with the blood, with which they continue in latent combination a certain but very varying time;* (3) that many of them are capable of coexisting in the same system. And two other laws result from the study of morbid poisons,—(1) that they are not acted upon by medicinal substances as long as they continue latent; (2) when they act on more tissues than one,

* The proof of their mingling with the blood is the fact, that some medicinal substances have been detected in that fluid,—e.g. iodine; and that glanders has been communicated, by injecting into the veins of a healthy ass the blood drawn from a glandered horse.

the remedy which is an antidote to their action on one is absolutely powerless when they affect another tissue, so that many different remedies are frequently necessary to combat the varying phenomena of the same disease.

We have a great objection to the frequent application of the term *law* to questionable doctrines in medicine, (and how few are not questionable?) and we can regard it as a law by no means established, that "morbid poisons are not acted upon by medicinal substances as long as they continue latent." An authority of some weight in such a question, from the frequent opportunity which he has had of observation, and from his attention having been particularly turned to the latent period of some diseases, has distinctly alluded to the treatment appropriated to this period; shewing, at the least, his opinion on the subject, (Dr. Marsh, vol. v. *Dublin Hospital Reports*.) It is a question of great interest and importance, although it is one which it must be almost impossible to answer; since the only evidence of a latent period having existed is when that period has terminated. And, with regard to the second law above stated, we would ask, is not mercury antidotal to a syphilitic sore upon the glans penis, and to syphilitic inflammation of the iris; and are these tissues identical?

The following is the author's classification of diseases caused by morbid poisons:

"I. *Diseases both contagious and infectious*.—Typhus, Scarlatina, Morbilli, Variolæ, Varicella, Erysipelas, Pertussis.

"II. *Diseases simply contagious*.—Variolæ vaccinæ, Syphilis, Gonorrhœa, Hydrophobia, Pestis, Cellulitis venenata, Cellulitis farciminosa, Tinea.

"III. *Diseases neither contagious nor infectious, but depending on miasmata*.—Febris palustris, Cholera indica, Dysenteria palustris.

"This catalogue," says Dr. Williams, "might perhaps admit of some additions, as Catarrh, Influenza, Angina parotidæa, &c.; but it has been thought preferable, until the arrangement shall have received the approbation of the profession, not to introduce any disease that might give rise to controversy." (P. 23.)

It will by some be thought that diseases have been introduced into the above arrangement which may give rise to controversy; but it is not our intention now to enter into the interminable question of contagion, and we shall therefore reserve the expression of our opinion on this classification until the author's views have been explained with reference to each disease included in it. The present volume is devoted to the first class only, and typhus fever stands at its head.

TYPHUS. Of this disease, the author says, "there is but one simple continued fever known in this country, and that is caused by the agency of the typhoid poison." In this opinion we are disposed to coincide, as far as the knowledge which we possess of the characteristics of fevers of the present day will allow such a decision. We have witnessed continued fevers in continental hospitals, in those of London, Edinburgh, and Dublin, and in other parts; and, from the common characteristics of them all, we should regard them as the same disease. What may appear exceptions to this rule will receive considerable explanation from Dr. Williams's subsequent remarks. But, whilst making this admission, we speak only of what our own experience and much of our reading have confirmed. We must always bear in mind what is termed the epidemic constitution, as well as the fact that such an observer as Cullen has

distinguished fevers of very different characters. The consequence of increased knowledge of the pathology of fevers has gradually been to diminish their recognized number; every variety of symptom or attendant circumstance having been at one time considered sufficient to justify a generic distinction. Dr. Williams has here generalized to the utmost, and we think with correctness. The existence of a typhoid poison does not admit of demonstration, but is inferred from the evident contagiousness of the disease, and from its capability of communication by fomites. The evidence is probably as strong as evidence can possibly be in favour of a point not capable of strict demonstration. Dr. Williams regards as the most probable hypothesis, that of the constant existence of the miasmata of typhus generally diffused in the atmosphere of certain countries. It is probably the hypothesis most consistent with our knowledge, but unfortunately it is only an hypothesis. This poison is subject to certain laws. It is capable of being diffused throughout the atmosphere, and, according to its intensity, of producing disease at certain distances. It lies latent a given period, gives rise to a certain series of phenomena, the course of which is modified by modes of treatment. With regard to the *mode of absorption* of the poison, Dr. Williams remarks, that "the typhoid poison, being diffusible in the atmosphere, is most easily introduced into the system by means of the mucous membranes, and more especially of the mucous membranes of the pulmonary organs. We have no direct evidence of the skin absorbing the typhoid poison; but it is so probable, that little doubt can remain on the subject." But, supposing that it was proved that in some cases the poison was absorbed, there are certain other cases, to which Dr. Williams does not allude, which would lead to the conclusion that the typhoid poison is by no means necessarily *absorbed* to produce its effects. These cases, too, are exceptions to the latent period. In the paper already alluded to, Dr. Marsh has collected several instances in which the first impression, on exposure to the effluvia of a fever patient, was on the organs of smell. He was himself so attacked with fever. When very much fatigued, he incautiously turned down the bedclothes of a fever patient, became sensible of a very disgusting smell, and felt suddenly oppressed and overwhelmed: a severe fever followed. Some curious instances of the same kind are mentioned in our last Number, p. 270, and most experienced practitioners have witnessed similar results. The inference from such cases would be rather in favour of a direct impression upon the nerves of the mucous membranes than indirectly by means of absorption. This may probably depend on the intensity of the poison. When extremely concentrated, it may produce its effects, as in the case of Dr. Marsh and Dr. Geary; when diluted, not until it has become absorbed. The period of *latency* varies much. We have referred to instances of sudden action; but "the extreme periods which the poison of typhus may lie latent vary from a few hours to a few weeks, or perhaps to a few months."

Its *coexistence with other poisons* is evident from its combination with scabies, syphilis, and erysipelas; "and the last is so frequent, that, in the London Fever Hospital, erysipelas is said to stalk from bed to bed, destroying the hope which had otherwise been entertained of the recovery of the patient."

The *pathological* changes of typhus are now so familiar to every one,

that it is needless to specify their variety in kind, in degree, and in situation. But "it is on points of this intricacy that a knowledge of the general laws of poisons enables us to reconcile discrepancies so apparent, and at first sight so fatal to the hypothesis of one cause; for it is apprehended that a reference to those laws will shew that the exceptions that have been mentioned are not greater deviations from their general law than are common to the laws of poisons generally."

"Is the dose of the typhoid poison in excess, and the disease rapidly fatal, we should naturally expect, as in the case of an excessive dose of arsenic or of oxalic acid, that the morbid appearances would be either trifling or altogether wanting; while, supposing the dose to be milder, we should equally expect much more extensive marks of the action of the poison. When we observe, also, the poisons of small-pox or of scarlet fever producing their specific eruptions, sometimes on the cutis, sometimes on the mucous membranes, and not unfrequently on both, we can hardly feel surprised that the poison of typhus may sometimes attack one of the constituent parts of the same membrane, sometimes another, and occasionally different combinations of those parts. It is admitted that we cannot determine the inexplicable modification the poison must have undergone to produce these various results; but the differences that have been mentioned in no degree disprove the unity of the efficient cause, nor are greater deviations than are common to the laws of poisons generally. Every pathologist, therefore, will be prepared to admit occasional and limited differences in the seat of the disease, as also of the pathological phenomena affecting those seats in typhus fever." (P. 45.)

The outline of the morbid anatomy of typhus is very well given, and is a very valuable part of the chapter; but, as it contains nothing new, it need not detain us. We may, however, notice that later experience has shewn that the specific morbid action of typhus upon the intestinal canal is not so exclusively as was supposed upon its mucous follicles. Although this form of disease may characterize every case, one year, it may be altogether wanting in another; while, in another, every constituent part of the membrane may be occasionally affected. Dr. Williams has made a considerable omission in not having attended to the morbid condition of the blood in typhus. It would appear (and from the author's hypothesis of the poison infecting the blood, it was well deserving his notice,) that there are changes in the blood in typhus, more or less characteristic, and without an allusion to which its morbid anatomy must be incomplete; such as, for instance, the diminished, and, in some instances, exhausted coagulability, &c.

Under the head *Treatment*, is considered the question, "whether there is any mode of arresting the fever, immediately on its formation?" The evidence in favour of such an action, which has been ascribed to cold affusion is considered, and is decided negatively. The benefit derived in the cases related by Dr. Currie is ascribed, not to any power possessed by cold of *stopping* the fever, but to the agreeable effects produced on the patient's feelings by a mode of treatment introduced when the Brunonian heating system was prevalent.

"If," says Dr. Williams, "we turn from this empirical practice of cold affusion to the great and leading doctrine of fever, which attributes this disease to the action of a morbid poison, it will be plain, if that hypothesis be correct, that cold affusion could not interrupt the course, though it might modify the symptoms. A poison circulating with the blood cannot be removed from the system by ablation of its surface. No person expects to stay the course of small-pox, of scabies, or of syphilis, by a similar application. We might therefore have predicated, *a priori*, that cold affusion could

not remove from the body the poison of typhus fever, and consequently had no power to stop the course of the disease, though it might modify the symptoms." (P. 71.)

Now, with regard to typhus, much is taken for granted in the foregoing paragraph, which must be proved before it can lay much claim to our belief. It is assumed, (1) that the typhoid poison circulates with the blood; (2) that, with respect to this particular remedy (cold affusion), scabies and syphilis stand on the same ground; (3) that the effect of cold affusion is merely ablation of the surface; (4) that it is necessary to remove the poison from the body in order to stop the disease.

The first point is not proved; and we have alluded to a class of cases in which it is not impossible that fever is generated by an impression on the nerves. With respect to the second, in our ignorance of the action of cold affusion, it would be as reasonable to say that no person expects to stop ague or syphilis by sulphur, scabies by quinine, or typhus by vaccination. The effect of cold affusion is probably something far beyond mere ablation of the surface, as we may conclude from Dr. Tweedie's remarks, quoted by the author, that "it gave great alarm to the patient, and the shock was in many instances injurious, &c.;" and, with regard to the fourth question, may not the action of cold affusion, instead of removing the poison from the body, be to render it again latent? Dr. Williams believes the poison to exist in the blood during the latent stage, without manifesting any effect; so that it is scarcely fair to require proof of its being removed from the system before he will admit the possibility of the disease being stopped. We are not the advocates of cold affusion, and we believe that in this we are supported generally by the profession; but we must protest against any positive inferences being drawn from such reasoning as we have above noticed. Emetics are likewise repudiated as inefficacious in stopping the disease; and the question, as to the known existence of any antidote or specific remedy to the poison, is answered negatively. With regard to mercury, it is truly observed, that the evidence of its utility is so conflicting as "plainly to prove that its exhibition has not diminished the mortality in typhus, and that its entire abandonment would in no case lessen the chances of the patient's recovery, but in many instances might perhaps increase them." Dr. Williams has entered largely into the question of bleeding in typhus. It is unfortunate that, except in a few instances, we have little more than the authority of great names in favour of or against the practice of bleeding. But,—when Sydenham's practice was influenced by the hypothesis that fermentation was the cause of fever, by increasing the bulk of the blood, and that it required to be diminished by venæsection; when others have bled from a purely mechanical notion of the phenomena of fever; and others from the supposition that it is always symptomatic of inflammation; when, at the same time, the results are stated only in general terms, or, at least, with none of the minuteness which the importance of the case demands—we can place no reliance whatever on the mere statement of opinion as to the utility of the practice. From a careful comparison of such evidence as exists, Dr. Williams concludes that, "since the evidence against bleeding in fever so greatly outweighs that in its favour, it seems demonstrated, and by the most extensive practical experience as yet before the public on any disputed medical question, that bleeding in the cure of fever is the exception and not the rule; and,

although that operation may be occasionally useful, it is only as a mode of treatment, and applicable to particular cases."

The rules of treatment, says the author, that should guide us are, first, that the disease has a course to run, and that there is a series of inflammations, modified by various circumstances, to be set up; that probably no art can prevent these inflammations; and that the general and specific actions of the poison are greatly increased by unnecessary depletion, and by whatever weakens the body.

It is considered as generally true that, although not marked by any decided symptom, in all cases in which the disease has existed but a few hours, inflammation of some part of the alimentary canal exists. In this state of the disease, a large number of experiments has been made at St. Thomas's Hospital, to determine the most beneficial modes of treatment, and with the following results:

"Medicines were found to have little effect, either in controlling or subduing the inflammation of the intestinal canal in fever, or even in controlling the diarrhoea. It remained, therefore, to try what effects an almost purely local treatment would produce, and whether, by means of soothing the intestine, we might not moderate the inflammation; and in this manner produce, both directly and indirectly, more sanatory effects. The most effective plan is that of enemata, consisting of barley-water and of syrup of poppies." (P. 84.)

The mode of treatment is as follows:—Ten grains or a scruple of rhubarb are given, at whatever stage the patient is admitted. The bowels, having been satisfactorily emptied, an enema, consisting of a pint of barley-water together with half an ounce of syrup of poppies, is given night and morning. This simple treatment has been continued until the patient is convalescent, and has been rarely complicated by the use of any medicine. Its success has been remarkable, compared with other modes of treatment, when the fever has been of any moderate degree of intensity; so much so, that in the years immediately before the cholera, out of sixty-three cases treated thus only one died. The cholera was preceded and accompanied by a very severe fever, and the treatment by enemata was by no means so successful, one being lost out of four or five. But still it has been considered, on comparing the deaths and recoveries under other modes of treatment, that this, by enemata, was, on the whole, most successful. Many attempts have been made to render this simple mode of treatment more efficient by the topical application of leeches, or blisters, or mustard poultices; but there is no more delicate point than the interfering with the course of fever by apparently so slight a remedy as topical depletion. The occurrence of delirium after the application of leeches, or else its aggravation, is so often witnessed, as distinctly to shew that in fever, in proportion as you debilitate the patient, even in the degree perhaps necessary to save a given organ, so is the nervous system laid only the more severely under the action of the poison. But there are cases where even violent delirium is kept up, apparently from sympathy with the diseased state of the intestinal canal: these cases are not uncommon. It is difficult to determine the cases where this susceptibility exists; but, if the patient be of a full habit, and young and flushed, and the pulse 110 to 120 in the first stage of fever, the application of leeches has in general been well borne, and given relief. On the contrary, when, as in the cholera years, the patient has been full and

bloated, the countenance purple, and the sputa streaked with blood, at the first onset of the disease, depletion by leeches from the abdomen, or any other part, has aggravated all the symptoms, and the patient has rapidly sunk.

As a general principle, it is considered, as shewn by a large experience, that, in mitigating the disordered states of the intestinal canal in typhus fever, there is no treatment so generally successful as that by poppy enemata, together with the application of mustard poultices to the abdomen, and occasional and moderate local bleeding. The immediate operation of the enemata is to remove from the inflamed part all that irritates it, and thus to place it under circumstances most favorable to a happy termination. The indirect effect is to produce a general glow over the whole body, to lull the brain, and to quiet the general as well as the local irritation.

In certain circumstances, the action of enemata is less beneficial: such is the case when the stomach or small intestines are the seat of disease, when the intestine is so irritable that they are immediately rejected: they are of no service when there is much meteorism, or hemorrhage from the bowels. "When the fever assumes a more complicated form, and the specific action of the poison falls on the membranes of the brain, it is probable that this inflammation has its course to run; and for a time will be but little influenced by any remedies." Moderate delirium without pain in the head will not, in one case out of twenty, require any other than the treatment by enemata; but, if severe, with injected conjunctiva, leeches may be applied: and when this form of disease is attended with a slow pulse, of forty or sixty, leeches are also required. Blisters, the author thinks, are little to be relied on. When, in cases more complicated, the lungs become affected, if the inflammation is confined to the bronchial membrane, it may generally be neglected, unless severe, when a few leeches or a blister may be required. But, should pneumonia occur, blood must be taken by cupping or from the arm; always, however, remembered the difference between such pneumonia and simple phlegmasia.

The above is an outline of Dr. Williams's mode of treatment.

"Success," it is observed, "is the only criterion in medicine; and certainly this has effected the cure of a much larger proportion of cases than any other mode I have witnessed." . . . "The simplicity also of the treatment by enemata, even supposing it not to be attended by more favorable results than any other mode, is a great recommendation; for it puts an end to the necessity of the practitioner following up symptom after symptom, all of them up to a certain period generally combated unsuccessfully: and thus prevents his being kept in a constant state of anxiety, and the patient in a state of perpetual annoyance. It is a mode of treatment that is applicable to every form of the disease, and should be adopted in every case, only omitting the syrup of poppies when the bowels become constipated. The enemata ought also to precede all local treatment of the head, for they often entirely relieve it; so that it is not right to make use of topical depletion, or other local application to that part, till after their exhibition; for otherwise we hazard the aggravation of every symptom, and often make that disease, which was before functional, now structural." (P. 93.)

The treatment by enemata is considered as affording great advantages to the system in contending against the sequelæ of fever; such as sloughing, and erysipelas.

We have already alluded to the disadvantages attending on the recom-

mendation of a method of treatment when it rests only upon the authority of individuals; and, however high the authority of Dr. Williams, it does not except him from the application of this remark. The few cases which are appended are of little or no use in assisting our judgment. With regard to the use of enemata *alone*, it is evident that they are trustworthy only in the milder forms of fever; respecting which the eternal question arises, as to the power of spontaneous recovery. The fact of only one case in sixty-three having died at one period, that of one in four or five having subsequently died, proves almost too much as it regards the efficacy of the enemata; for the inference is, the extreme mildness of the disease in the former case. But a table containing an analysis of these sixty-three cases, minutely drawn out; together with another, similarly exhibiting those cases in which the mortality was so considerable; these, contrasted with other analyses of the results of different modes of practice employed by other physicians at the same time, both in the Hospital of St. Thomas and that of Guy, would have afforded the sort of evidence which we require, and would have been of a very satisfactory character. As records are doubtless kept of all such cases in both these establishments, may we not express a hope that, in one of his future volumes, the author will, by publishing such an analysis, remove all ground for scepticism as to the curative powers of the plan of treatment above recommended? Our own disposition is entirely to think favorably of the treatment in many cases, and to recommend its employment: to believe, also, that in others it would prove entirely inefficacious; such, for example, as those in which the use of very large quantities of vinous stimuli has appeared to act like pouring life into the patient. It is, therefore, from the existence of doubts as to the efficacy of the above simple treatment in various cases of typhus fever, that we should have rejoiced in more abundant details.

SCARLATINA. The division of this disease suggested by Dr. Williams is as follows:—Scarlatina mitior (the *S. anginosa* of Willan); *S. gravior*, (corresponding to the *S. maligna* of Willan); *S. sine eruptione*, and *S. sine angina*, (the last being Willan's *S. simplex*.) In the *Scarlatina sine eruptione*, "the *specific* action of the poison is limited to one tissue, or to the mucous membrane of the mouth and fauces; the cutaneous exanthema being altogether wanting." There can be now no doubt of adding this to the other forms of the disease. We need not spend our time in characterizing these various forms; but our author's remarks on the dropsy, which sometimes forms a part of the disease, (one of the tertiary actions of the poison, as it is termed,) will be very acceptable to some of our readers. It usually occurs on the twenty-second or twenty-third day from the commencement of the disease, but in a very variable proportion of cases. It is generally limited to a disordered function of the cellular or serous membranes, but in a few cases it is preceded by inflammation of the pleura or peritonæum; and, in either case, languor, peevishness, constipated bowels, sickness, and vomiting, commonly precede it by several days. It very constantly begins by œdema of the face, and, in cases of considerable danger, often attacks no other part; when more general, it affects the hands rather than the feet: in a few cases, not only the face and the extremities, but the trunk and body generally, become anasarcaous; and effusion also into the abdomen, head, or chest, though not

commonly, is still occasionally seen. On the first appearance of the œdema, the urine is scanty and commonly turbid; and, although the quantity is small, there is a frequent desire to pass it: in this case, a pain is felt on pressing over the bladder. The urine, however, does not remain long scanty, but is secreted copiously, and continues turbid, from numerous small fibres floating in it: its chemical properties are considerably altered. In many cases, so early as the first or beginning of the second week of the fever, and sometimes later, even when no dropsy succeeds, the urine assumes a pale red or pinky colour, and resembles the water in which flesh has been washed, so that there can be no doubt of the unusual colour being caused by particles of red blood; and it has been remarked, that those who have this symptom are more slow to recover than those in whom it is wanting. But, although the red particles of the blood are absent, another constituent of the blood appears to be present in the serum, for albumen is deposited when the urine is exposed to a heat of 160°. F. The specific gravity of this urine is also lighter, varying from 1,011 to 1,017. The first appearance of œdema is usually accompanied by an acceleration of the pulse; and, if the disease terminates favorably, the œdema and accelerated pulse, in addition to the state of the urine, are the only very prominent features. But there are many sources of danger in dropsy after scarlatina; for effusion may take place into the ventricles of the brain or into the cavity of the chest, or the anasarca may become so universal and so excessive as to present a very formidable disease. When patients have died with dropsy, Mr. Hamilton conceives that he has seen the kidney, not only in the first stage of Bright's kidney, but also so far advanced in disorganization as to have been of a whitish cream or straw colour. These remarks, Dr. Williams thinks, require confirmation; for albumen is often found in the urine when the structure of the kidney is healthy; and no instance is known of recovery from dropsy with Bright's kidney, but recovery from dropsy after scarlatina is frequent.

The results obtained by the practice of bleeding, in the treatment of scarlatina, as well as by abstaining from it, are compared in the following table:

Of 121 treated at the Foundling Hospital, in 1786, by bleeding		19 died.
60	London Fever Hospital, in 1829	10 —
<hr/>		<hr/>
181		29
or nearly 1 in 6.		
Whilst of 200 treated by mineral acids and wine, &c.		2 died.
— 160	purgatives and emetics	16 —
— 50	ditto	3 —
— 45	ditto	1 —
— 100	mineral acids and wine	3 —
<hr/>		<hr/>
555		25

or nearly 1 in 22. From this it would appear, that the chances of recovery are diminished by the practice of bleeding in the ratio of nearly 4 to 1, as compared with the chances supposing the patient not to have been bled: but we require much more precise data before we can admit this

as a fair general inference. Many modes of treatment have been suggested. The endeavour of the author is to point out the different forms of disease to which different modes are more particularly applicable, and to establish some general rule of treatment.

To the scarlatina sine angina and sine eruptione, the *nimia diligentia* caution of Sydenham is strictly applicable. The S. mitior is distinguished from the S. gravior, by presenting an inflammation of a more sthenic character. The principle of treatment here appears to be, "to admit of a middle course, or of one less debilitating than by general bleeding, and less powerfully stimulant than by bark and quinine:" first, to tranquillize the stomach and to allay its inverted action, if vomiting exists, either by small doses of the sulphate of magnesia or by the effervescing draught, every four or six hours. If the tonsils have acquired the peculiar character which marks this form, (i. e. if they are enlarged, hard, and swollen,) ten or twelve leeches should be applied to the throat, which both relieve it and the accompanying cerebral disturbance. When the disease is severe, it may be necessary to repeat the leeches. The tonsils being relieved, the disease may be permitted to run its course, little influenced by medicine; saline draughts being allowed to refresh the patient. Subsequently, when the fever has declined, some tonic medicines may be desirable. In Scarlatina gravior, the tendency exists in different parts to run into mortification. A more stimulant plan of treatment is required; and, of stimulants, the author prefers wine, which requires to be continued until the patient is convalescent, and even perhaps for some time afterwards. There are cases in which it is difficult to determine, from the state of the throat, whether it be desirable to adopt the treatment by wine, or to apply leeches, or to permit the disease to run its course without interference. In these cases, it is perhaps better to err on the safe side, and to order wine, since the only inconvenience that can arise will be, that the tonsils may become swollen and tense; an occurrence easily remedied by withdrawing the wine and applying a few leeches. The principles of treatment recommended in the asthenic form of scarlatina are such, that we are surprised the author has not applied them to some forms of typhus fever. In both cases, the poison has acted by depressing to a great degree the powers of life; in one only does Dr. Williams commend the employment of vinous stimuli. In our own view of the case, the principle of treatment is identical; the degree of its applicability varying with each case.

There is nothing that need detain us in the chapter on *Measles*; and very little novelty in that on *Small Pox*: we shall therefore pass on to the following.

ERYSIPELAS. It is in the treatment of this disease that the author considers he has met with some of the strongest illustrations of the value of his principles of practice. It will be less generally admitted than with regard to the diseases which we have already noticed, that erysipelas should be considered as both contagious and infectious; but these qualities would appear to be shown by the same kind of proof in all. It spreads through the wards of an hospital if a patient suffering with the disease is brought into them.

"The infectious nature of erysipelas has on many occasions appeared so manifest, and the danger often so imminent, that the medical officers [of St. Thomas's] consi-

dered it an imperative duty to recommend to the governors the necessity of sacrificing the houses on the North side of St. Thomas's street, for the purpose of procuring a more complete ventilation. A month seldom elapses without some new and striking instance of the infectious nature of erysipelas occurring in the older parts of the building, and it has even spread this year to the new wings; for six patients in Anne's ward have been seized with this disease subsequently to the admission of two or three erysipelatous patients into the ward.' (P. 262.)

In our Fifth Number, we noticed (pp. 147, 148,) the views of Dr. Williams respecting the treatment of erysipelas. In the present volume he has entered fully into the question, still maintaining his previous views in favour of the tonic treatment. We say now as we have already said,—this is simply a question of experience. We object to all *à priori* arguments: but, if they were insisted on, we should instance Scarlatina mitior, the dropsy consequent on the action of the poison of scarlatina, pneumonia, &c., attending typhus, as justifying, according to our author's own practice, the use of depletion in a class of diseases to which, from his own *theory*, we should infer that such a practice was inapplicable. The question then occurs, whether erysipelas is, or is not, a disease similarly circumstanced to those above mentioned?

The first authority quoted, or rather misquoted, is Sydenham.

"He bled on the first day, purged the patient on the second, and again bled him on the third day; and, if these means failed, he bled twice more, interposing a day between each operation. Yet he adds, there are cases which require a very different treatment, for neither the evacuations, how frequently soever repeated, nor testaceous powders exhibited to sweeten the blood, at all avail when a noxious recrementitious matter lies deep in the skin, and cannot be removed but by such remedies as strengthen the tone of the blood, and are, consequently, proper to open the obstruction of the pores." (P. 275.)

This, however, is a misrepresentation; for the above quotation from Sydenham refers to other diseases than erysipelas, and is rendered applicable to erysipelas only by the omission of the part of the paragraph preceding that which has been above extracted. The part omitted is as follows: "I shall observe here, by the way, that, though not only this disease (i. e. erysipelas,) but the greater part of such as affect the skin, and are attended with some sort of eruption, in case they are of the chronic kind, readily yield to this method, and accordingly go off in a short time by repeated bleeding and purging; yet there are others that require a very different treatment. For neither the evacuations, &c.," (as we have just above quoted.) And again, after having mentioned medicines proper for *these* cases, (imagined by the author to be erysipelas,) Sydenham adds, "But the medicines above prescribed must by no means be used before sufficient bleeding and purging have been used, &c." We make this quotation for a double purpose: to show what really was Sydenham's opinion, and that the occupation of the mind by an hypothesis may lead it to understand a very negative as an affirmative. Cullen's evidence is in favour of bleeding, as a rule. Dr. Williams has given Mr. Lawrence's paper on Erysipelas (if, at least, he quote from the Fourteenth Vol. of the Medico-Chirurgical Transactions,) a somewhat unfair criticism. The evidence in favour of bleeding in erysipelas to be derived from that paper, is treated with little more fairness than is the opinion of Sydenham in the above-quoted misrepresentation. Why are "only seven cases of idiopathic erysipelas, and seven cases of traumatic

erysipelas," spoken of, when the paper contains the histories of eighty-eight cases, many more than fourteen of which bear directly on the treatment by depletion, so strongly recommended by its author? Dupuytren's opinion was that also of Mr. Lawrence. Dr. Williams has quoted five cases as given by Dupuytren. They do not appear to be favorable examples of the advantages of such practice; but, as it is not said whence they are taken, and as Dupuytren probably, in relating them, had some other object of instruction in view than that of the effect of bleeding in erysipelas, without the context we do not know what value to put upon them. It is hardly probable that Dupuytren would have recommended a practice of this kind from such cases as those quoted. Lepelletier says that, "*in the majority of cases*, the French practitioners are against bleeding in erysipelas." Blache and Chomel say that general bleeding has often no other effect than to blanch the eruption, without *notably* abridging its duration. Bally *abstained* from bleeding or leeching, considering them "as calculated to aggravate the symptoms, to accelerate delirium, &c." Drs. Fordyce and Wells, and Heberden, disapproved of bleeding. Mr. Pearson says that it is admissible *very rarely in large towns*, and that *a repetition of it will very seldom be necessary*. Whilst erysipelas was for two years epidemic in London, Mr. Bromfield says that the antiphlogistic treatment was *generally* fatal, while cordials were most efficacious. Dr. Butter speaks of it as absolutely injurious, and Dr. Willan, after stating that *all ancient writers*, except Galen, *recommend bleeding*, adds, "This practice must be evidently improper in the three forms of erysipelas last described; and even in the E. phlegmonodes, it does not *always* appear necessary." To these various opinions communicated by Dr. Williams, we add the following: Biett, speaking of active forms of erysipelas, says, "Bleedings are generally indispensable;" and adds, that they should be repeated according to the increase of the symptoms. (*Sur les Mal. de la Peau.*, p. 19.) In Hutchinson's cases (Vol. V. *Med. Chir. Trans.*) the incisions were attended with a loss of from fifteen to twenty ounces of blood; so that these cases must be regarded as affording some evidence of the advantages of moderate depletion. The cases were mostly of phlegmonous erysipelas of the lower extremities, in sailors. Of forty-one cases, it is said that not one was lost. Dr. Dobson's punctures, again, are attended with a considerable loss of blood. His practice appears to have been very successful, not only in his own hands, but in those of others; and, from our own experience, we can speak very highly of it in active cases of simple erysipelas.

Dr. Williams concludes that the weight of argument, as well as of authority is against bleeding. We are indisposed to attach any weight to the argument; and, if the authority of individuals be appealed to, it would be extremely difficult to say on which side it preponderated; or whether, indeed, the subject was one which, as it is stated, fairly admits of comparison. Before authority is allowed to have weight, it must be remembered that a disease, and not the name of a disease, is the subject of judgment. But are the sailors at Deal, under the care of Mr. Hutchinson, similarly circumstanced to the sufferers from an epidemic in London, or are the patients of Dupuytren and Dr. Williams under the same influences? From the opinions alone of these individuals, we cannot set any value upon the comparative fitness or unfitness of this or that mode of treat-

ment. Our own conclusions would be, that the disease was subject to variations requiring corresponding variation in the treatment; that very much of the favour shewn to one mode of treatment or another depended on the natural disposition of the disease to terminate favorably, as well as on the bias of practitioners to adopt generally an antiphlogistic or stimulant system; but that it was safer as a rule to employ cautiously the former. Dr. Williams is entirely opposed to the practice of bleeding. In our Fifth Number we have stated what his practice is; we need not therefore dwell on it here.

We have already extended our notice so far that we must pass over the subject of *Whooping-cough*, which occupies the next chapter. The volume terminates with an Appendix on the advantages of the Bromide of Potassium in enlarged spleen; in illustration of which some striking cases are given.

The refutation of a law (as it was termed) of poisons, originating with Hunter and of which Adams spoke, "as well ascertained as any other in pathology," i. e. "that two actions cannot be carried on at the same time in the same part or in the same constitution," is sufficiently afforded by Dr. Williams. It is a useful lesson to those who are fond of laying down pathological laws, the existence of which often depends on imperfect acquaintance with disease, and is endangered by every addition which is made to our knowledge. Such a fate, we doubt not, awaits many of the laws contained in the present volume; which is, however, a book of very considerable ingenuity, though frequently characterized by loose and inconclusive reasoning: from the many important facts contained in it, whether the result of the author's own experience, or drawn from the experience of others, it is well worthy of a very attentive perusal.

ART. IV.

1. *De Suicidio, Observationibus anatomico-pathologicis illustrato*. Scripsit D. J. A. ARNTZENIUS, Med. et Chir. Doct.—*Trajecti ad Rhennon*, 1835. 8vo. pp. 204.

Of Suicide, illustrated by anatomico-pathological Observations. By D. J. A. ARNTZENIUS, M.D.

2. *Das Heimweh und der Selbstmord*. Von J. H. G. SCHLEGEL, M.D. &c.—*Hildburghausen*, 1835. I. II. Theil. 8vo. pp. 125, 427.

Nostalgia and Suicide. By Dr. SCHLEGEL.

NOTHING but the frequency of the fact could make it credible that a rational animal, short-lived and fond of life, should be distinguished, among other things, from all other animals not considered to be endowed with reason, by inflicting premature death upon itself. The circumstances under which the act is done are not calculated to lessen the surprise it occasions. It is resorted to by the young, who might be supposed to be in the enjoyment of the gift of life, and by the old, who might be expected soon to be delivered from it in a natural way. Very often it is performed with evident deliberation and forethought, with much ingenuity of concealment and elaborateness of preparation; and the last act is as calmly executed as if it were but the dignified end of a

well-spent life. Charity towards human beings disposes juries, in defiance of barbarous remnants of ancient laws, to assume that, in every case of this kind, the guiding reason was first overthrown; and we believe that this merciful view is actually borne out by the uncoloured facts disclosed by dissection, and facts related by various witnesses, many of whom had had extensive opportunities of observation. The question is, at least, one of considerable interest to every man of humanity, as well as to every man of science. It may be useful to bring together some of the scattered facts bearing upon it, and it may not be uninteresting to the reader to add to these others in various ways connected with the subject of suicide; and for which the two volumes before us afford considerable opportunity.

The little work, to which the name of Dr. Arntzenius is attached, is an enlargement of an academical thesis written by him in 1829, and treats, in a few well-arranged chapters, of suicide in general, of the organic lesions found in suicides, and of the seat and nature of the propensity.

The German book is chiefly a collection of histories of suicides, taken by its author from every available source. Many of the cases have fallen under Dr. Schlegel's own notice: the Bible, old histories, medical and legal records, personal communications, have been the other sources whence much of his matter has been derived; and he has not even neglected to examine newspapers, where, no doubt, his credulity found ample indulgence in reading of "forty naked female corpses, which were, within a period of two days, taken out of the basin in Hyde-Park." But, as we do not regard the whole of his book as of equal authority with the sentence above quoted, we shall notice such parts of it as appear to be most worthy.

After devoting some pages to the Bibliography of Suicide, and some to the various definitions of insanity in which it has been included, Dr. Arntzenius classes it under the head of *Delirium sine febre*, in a form in which there are false ideas and erroneous judgment concerning one object or a small series of objects; a monomania, in short, according to the term adopted by Esquirol in preference to the term *melancholia*, which conveys an erroneous impression respecting its origin. Dr. A. then divides suicide into acute and chronic; the first characterized by great physical and moral excitement; the second preceded by sadness, moroseness, fear, and love of solitude; and of the symptoms of the latter, as regards the face, eyes, tongue, respiration, a regular account is given, on which it is impossible to place any reliance. The mode of suicide, it is observed, is that which the unhappy person deems the speediest and the least painful. This, however, is subject to exceptions, many of which the German author has collected. Lord S., in 1834, cast himself into the crater of Vesuvius; a Frenchman, ambitious of leaving the world in a distinguished manner, attached himself to an enormous rocket, to which he then set fire, and thus ended his life; a German, in 1834, cast himself into a smelting furnace. There are, besides, several instances of suicide by abstinence from all nourishment. The American Journal of the Medical Sciences, for August, 1830, relates the case of a young man, aged twenty-seven, who, in a state of religious melancholy, resolved on thus terminating his life. He took nothing but water for seven weeks and four days, at the end of which time he died. An Italian, named Viterbi,

according to his own statement, unjustly condemned as an accomplice in a murder which was committed, adopted a similar mode of suicide, and died after twenty-one days; during which time, although food and drink were constantly placed before him, he took only one draught of water and a spoonful of wine. And Dr. Froriep, in his "Notizen," relates another instance of similar self-murder; the object of the suicide being thus to reconcile himself to God. In Paris, the object of terminating existence with as little pain as possible seems to be most cultivated: hence the very frequent use of burning charcoal. And on this account, also, the number of attempted but unsuccessful suicides is far larger in that city than elsewhere; since the poisonous effect of the carbonic acid gas is not unfrequently limited to stupefaction, during which the individual is discovered. The frequency of suicide in Paris, and the trifling circumstances under which many of them originate, have lately been alluded to in the Quarterly Review. Dr. Schlegel has collected some cases of a similar character: one of these is the case of Victor Escousse, a writer of plays. The second of his performances of this kind was blamed by the Parisian journalists: his vanity was mortally wounded, and he killed himself. The suicide of a French cook, because he was disappointed of some fish which was to have supplied a regal dinner, is generally known. An anecdote is here given of a boy, aged thirteen, who put an end to himself, because, as a punishment for some trifling fault, he was not allowed to wear his Sunday's clothes. A German student of law shot himself, from a melancholy state of mind into which he was brought in consequence of a deformity of the foot. The various and irrational shades of lovers' quarrels terminate frequently in the suicide of one or both of the infatuated. Simple ennui appears to be a sufficient inducement to some to end their days. Such an instance is mentioned of two young men, at Versailles, in 1834; and similar instances are not uncommon. The elder was not twenty-four years of age. They appeared possessed of all which could render life agreeable, and were on the most familiar terms with one another. They gradually collected a quantity of charcoal, and, when it appeared that there was but little chance of their being detected until such time as they were no longer in existence, they ignited it, and lay exposed to its fumes. The written excuse for this French folly was "aversion and weariness of life;" the value of which, probably, as far as it concerned their relations to this world, they had properly estimated, by being anxious for its termination. From the following table, it appears that different means of suicide are adopted at different ages:

				By Pistol.	By Hanging.
Between 10 years and 20 years of age				61	68
20	..	30	283	51
30	..	40	182	94
40	..	50	150	188
50	..	60	161	256
60	..	70	126	235
70	..	80	35	108
80	..	90	2	0
				1000	1000

So that, in the earliest suicidal years, there appears to be a preference

for hanging, which afterwards yields to the use of fire-arms; and this is, in more advanced years, again substituted by hanging. In classing 9000 cases of suicide which happened in Paris, between the years 1796 and 1830, Dr. Schlegel concludes that what he terms the "philosophic suicide," or that which is performed after deliberation, is executed during the night, or shortly before sunrise; whilst that which is not the result of premeditation occurs during the day, when its causes are especially operative; e. g. quarrels, mournful intelligence, loss at gaming, intemperance, &c. Drunkenness, says Dr. Schlegel, is the chief cause of suicide in England, Germany, and Russia; love and gambling, in France; whilst bigotry, or the fear of dying without the sacrament, he supposes to prevent it in Spain. In Italy it is very rare. In confirmation of the general opinion respecting its rarity in Italy is given the remark of a Roman lady, on being told that a young man had designedly shot himself: "*Dev'essere un Forestiere; gli Italiani non sono tanto matti.*" The suicide was a melancholy German tailor.

Suicide, since the revolution in France, has greatly increased. The average number during the last forty-two years has been 409 $\frac{3}{4}$. To these must be added the instances in which the attempt on life fails; a circumstance which has been already mentioned as of frequent occurrence; and then, according to the statement of a commission instituted for the purpose of enquiry into this subject, the number must be stated as 1639 annually in Paris; almost as many as in London, taking into consideration the different population of the two cities. Dr. Schlegel alludes to the abandoned state of some of the inhabitants of Paris, when there existed a brotherhood, termed "*The Society of the Friends of Suicide.*" It consisted of twelve members. A lot was annually cast to decide which of them should commit suicide in the presence of the others. Each member of this insane union (proving to what baseness the absence of religion and morals may conduct mankind) was required, by its statutes, (1) to be a man of honour; (2) to have experienced the injustice of mankind, the ingratitude of a friend, or the falseness of a lover or wife; (3) to have experienced, for years, a certain invincible vacuity of the soul, and a discontent with everything in this lower world: "*Das Herz ist gestorben—die Welt ist leer.*" Dr. Schlegel has but little charity towards the French capital. He calls it "*a suffocating, boiling cauldron, in which, as in the stew of Macbeth's witches, there simmer, with a modicum of virtue, all kinds of passions, vices, and crimes!*"

He has inserted statistical tables of the proportion of suicides to various populations: the first shewing the proportion as it respects the entire country, the second in relation to the inhabitants of cities or large towns. From these we shall make some selections.

FIRST TABLE.

Countries.	Proportion of Suicides to the Population.
1. Kingdom of Sweden	1 in 92,375
2. The Milanese	1 .. 72,570
3. Russia, 1819-1820	1 .. 36,860
Do. 1824-1827	1 .. 34,246
4. Kingdom of Prussia	1 .. 14,224
5. Saxony	1 .. 8,446.

Sweden exhibits a small number of suicides: but, when the simple habits of the Swedes, its few towns, and the scattered inhabitants of the country, are considered, the proportion is still considerable.

The estimate for the Milanese relates to the years from 1817 to 1826 inclusive, and is thus divided:

		Individuals of Population.
1.	Circle of Lodi	yearly, 1 in 19,410
2.	Milan (excluding the town)	1 .. 35,217
3.	Cremona	1 .. 72,747
4.	Mantua	1 .. 79,082
5.	Bergamo	1 .. 82,012
6.	Pavia	1 .. 91,084
7.	Brescia	1 .. 100,256
8.	Como	1 .. 100,749.

The numerical proportion of suicides is remarkably different in Russia, according to the governments. In some places, and indeed where the population is the thickest, the proportion is not more than 1 in 100,000; in others it amounts to 8 in 20,000: in this last is included the Siberian governments.

The proportion of suicides is very different in the different provinces of Prussia. It is greatest in that of Brandenburg, if the city of Berlin is included; then follow Saxony, Pomerania, Silesia, and lastly Westphalia and the country bordering the Rhine. In the above classification, we find the greatest number of suicides in Germany, and especially in the territory of the Elbe and Oder. Russia is in this respect intermediate, as it is also with regard to culture and civilization. The fewest suicides are in the most populous and most fertile, and in the least fertile, countries of Europe,—i. e. in Lombardy and Sweden.

SECOND TABLE.

		Cities.	Proportion of Suicides to the Population.
Russia		St. Petersburg	1 in 416
England	{	London, in the 18th century	1 .. 10,572
	 19th	1 .. 21,491
France		Paris, Department of the Seine,	1 .. 2,215
Switzerland	{	Geneva	1 .. 3,714
		Berlin, 1788-1797	1 .. 23,066
Germany	{ 1798-1807	1 .. 12,917
	 1813-1822	1 .. 3,312
		Hamburg	1 .. 4,800
		Leipzig	1 .. 3,143
Italy	{	Milan	1 .. 18,021
		Naples	1 .. 27,230
United States of North America	{	New York	1 .. 9,474
		Baltimore	1 .. 15,696
		Philadelphia	1 .. 20,000.

Dr. Schlegel appears to think that the estimate of suicides in St. Petersburg, although taken from the *Bulletin Universel*, (sect. 6, tab. 18,) is somewhat too high: in this we cannot but agree with him, or, rather, we think it *much* too high. He thinks also that the diminution in London, during the nineteenth century, is scarcely credible, particularly as the population has so considerably increased. He calls attention

to the remarkable increase of suicides in Berlin during successive periods. The Italian towns, as Italy generally, afford but few instances of self-murder. America, in respect to the subject of suicide, does not appear to differ much from the European states.

Self-murder, and other kinds of death, appear to possess this remarkable similarity, that, in a certain period of years, a certain average proportion of each occur. Should not this fact prove that neither causes of a general character, nor changes in the condition of states, occasion or augment suicides; but, rather, that they take their origin in private life? Causes acting generally are of rare occurrence, and do not recur periodically: if to these were to be attributed suicides, they would be occasionally very abundant, but in quiet times very rare, which is contrary to experience. The effects of human passions, exhibiting themselves in private life, do not occur suddenly, like an epidemic, but gradually and in a certain proportion; and these are as little liable to deviation from a certain course as the character and individuality of mankind from whence these passions arise, and produce their effects. But, as the excitement and unrest produced by general causes continue long after their commencement, and are liable to increase from new political events, the increase of suicide may be a *mediate* effect of extraordinary general circumstances.

Among the predisposing causes is enumerated, by Dr. Arntzenius, hereditary disposition. Fabrat has observed families of suicides. The proportion of suicides in men to women is stated to be as three to one, and the majority of suicides in women are occasioned by love. It is most frequent in men and women between the age of puberty and fifty, but there are infantile and senile exceptions. Thus Dr. Schlegel states, on the authority of Casper, that, in Berlin, between the years 1812 and 1821, no less than thirty-one children, of twelve years of age and under, committed suicide, either because they were tired of existence or had suffered some trifling chastisement. The melancholic temperament is, of course, accused; so also a sedentary life, &c.; but this regular detail of causes is fitter for an academical essay than for a practical work; for the student is expected to shew his reading, but the practical reader looks for guidance.

There is nothing in the German work on the morbid appearances found after suicide which requires notice. For these we must refer to the Latin essay.

Without expecting to find anything very satisfactory on this point, it is interesting to see what those who have at least been diligent seekers of information have supposed themselves to discover. But their contrary evidence takes away all feeling of satisfaction. It would, for instance, be interesting, and perhaps instructive, to learn that great thickness of the skull had often been noticed, if it were not accompanied by a precisely similar remark concerning its thinness. The testimony of Greding and Gall is in favour of its frequent thickness; but even this applies to lunatics in general; out of 216 of whom, thickness of the cranium was found in 167. Out of 100 who died with furious mania, 78 had the cranium very thick, and 20 very thin. Out of 30 fatuous patients, 21 had thick crania, and 6 thin. The thickness of the cranial bones in melancholy and maniacal patients, and in old people, was supposed by

Dr. Gall to be connected with a diminished size of the brain, to which the inner table of the cranial bones accommodated itself; and, together with this thickness of the cranial bones, he considered there was thickening of the membranes and ossification of the blood-vessels: but Esquirol denies the existence of thickened cranial bones in the majority of suicides. Dumas, and Baillie, and Everard Home, (quoted as two persons, "Everard *et* Home,") are mentioned as considering thickening of the cranium productive of mental disorder, by compressing the brain. Osiander regards it as an effect of increased vascular action, or a kind of chronic inflammation; and observes, that the same thing is observed in drunkards. In some cases the brain has been found of a capacity smaller than the cavity of the cranium, and in others the capacity of the cranial bones has been considered too small for the brain, and the cause of dreadful cephalalgia or melancholy. Malformation of the cranium, productive of partial pressure, as of the medulla oblongata, by an oblique position of the occipital foramen, or of any portion of the cerebrum, by a depression of the cranial bones, may be a cause of suicide, or of general insanity. More frequently morbid growths at the base of the cranium have been supposed to have such effects. The case of an old man is related by Osiander, who had been long affected with dreadful headach, and at length, weary of life, hanged himself; in whom small osseous excrescences were found near the carotid foramen. In a case mentioned by Lancisi, of hypochondriasis and suicide, a sharp bony excrescence was found near the apex of the lambdoid suture; the whole cranium was in this instance thinner than usual, and diseased from rickets in early life. Cases, similar as respects bony growths and their supposed effects, are quoted from Isenflamm, Elgenstjerna, and Recamier.

Accretions of the membranes to the cranium, or to one another, or to the brain, are also enumerated as being frequently found in suicides. According to Falret, the dura mater not unfrequently presents ossifications, the arachnoid is thickened, and the pia mater inflamed. Vascularity of the dura mater, and a collection of fluid in the ventricles, are mentioned by Home. Osiander considers congestion in the brain as a powerful cause of suicide, and thinks that this often occurs in botanists, whose heads are frequently directed towards the earth, and who "often commit suicide!" But Dr. Arntzenius very properly observes, that the appearances of congestion after death are not always to be depended upon. He quotes Schallgruber, as asserting that effusions between the membranes, sero-sanguineous, milky, albumino-purulent, or bloody, are found in suicides; effects often shewn by no symptoms, until apoplexy or mental disturbance, and fatal consequences, ensue.

Every variety of morbid appearance which has been found in the brain of lunatics, has, as might be expected, been found in suicides: softening, hardening, toughness, abscess, albuminous deposits; deposits of bony matter, ossification of arteries; hydatids in the ventricles, in the plexus choroides, and even in the brain; alterations of colour; cavities; unusual size of concretions in the pineal gland; diminished size of the ventricles, or their distension by fluid, &c. A case of suicide is referred to, related by Auenbrugger, in which there had been long-continued headach, and a fissure was found in the middle of the pons Varolii. Meckel's experiments on the diminished weight of the brain in lunatics,

and on its comparative weight after various diseases, and in man and animals, and of the brain and cerebellum, are referred to; but so many accidental circumstances may influence the results of such experiments, that it does not seem necessary to dwell upon them. Cabanis was of opinion that there was a greater quantity of phosphorus in the brains of lunatics, and especially of suicides, than in persons of sound intellect.

Bloody serum has sometimes been effused in the canal of the dorsal spine, and softening of the extremity of the lumbar cord has been observed.

The sections on the lesions found in the thorax and abdomen of suicides are as interesting in relation to insanity in general as to suicide; and we might extract from Dr. A.'s book many cases of suicide in which the previous symptoms being chiefly referrible to the brain, the only palpable lesions were found in the abdomen or the chest; cases subversive of the vain doctrine that, wherever there is functional disorder, there is and must be physical lesion of the disordered organ.

Every physiological student is familiar with the sympathies existing between the brain and the organs of circulation and respiration, and with the important relations of the latter to the integrity of the cerebrum. Some authors have gone so far as to consider impeded respiration a frequent cause of melancholia, and of the suicidal kind; and every practitioner has occasionally observed cerebral excitement connected with diseases of the heart. Both melancholia and mental excitement are now and then seen palpably to induce or hasten the progress of phthisis. Lesions of the lungs are, perhaps, among the most common morbid appearances in the bodies of lunatics; and Esquirol states that one-fourth of the melancholic die of phthisis pulmonalis. Of the insane who die at the Salpêtrière, Georget has found that more than one-half die phthisical; the disease being always in a chronic form, and often unsuspected before death. The difficulty in these cases is to distinguish collateral events from causes and effects.

A disordered state of the skin, with suppressed perspiration, has often been remarked in connexion with melancholia; and Dr. Arntzenius speaks of it under the head of Affections of the Circulation. The actual relation of diseases of the heart and long-continued mental disturbance is not more easy to be determined than that of some mental and pulmonary disorders; their frequent coexistence is well known. It appears that, in many suicides, the heart has been found diseased, enlarged, or contracted, of unusual dryness, misplaced, or the pericardium adherent.

Between the abdominal viscera and the brain, the sympathies have long been acknowledged. The figurative language of all countries conveys the assurance of this conviction having long prevailed. If, with reference to the chest, we have the expressions "suffocated," "breathless," "heart-broken," indicative of various degrees of emotion, the stomach, the liver, the spleen, and the bowels have been equally commemorated in the greater number of languages, both by the vulgar and the learned. Daily experience teaches the effects on the functions of the brain of various stimulants and sedatives received into the stomach; food, wine, opium, &c.; and few observers of their own economy are ignorant of the mental depression attendant on overloaded and sluggish intestines. The effects of injuries of the head upon the stomach, and the relief of the brain often ensuing on the administration of emetics, are things long

observed and commonly known; as well as the various shades of cerebral irritation, stupor, sensorial affection, convulsion, and disease arising in intestinal sources.

Loss or excess of appetite, heat of stomach, nausea, vomiting, and obstinate constipation, are circumstances observed in the majority of lunatics; so that the ancients, and even the moderns down to Boerhaave, have been prone, as observed by the author, to consider disturbances of the bile as the general cause of all mental disorders. Dissections of melancholy patients and of suicides have shewn the size of the stomach increased or diminished, inflamed or scirrhus. The small intestines, however, are more frequently the subject of inflammation than the stomach, and especially, it is said, the jejunum. The intestines are not unfrequently found infarcted with fæces; and even the appendix of the cæcum. Displacement and contraction of the larger intestines are not unfrequent. Esquirol found displacement of the colon in several species of insanity, but especially in the melancholic; the transverse portion having become oblique, or even perpendicular; the left portion tending towards or descending behind the pubes, or even the middle portion fallen into the hypogastrium. These displacements were found in 33 bodies out of 168 subjects of melancholia examined. Esquirol considers the epigastric pains and constipation from which lunatics suffer as arising from this cause; for the removal of which he recommends the tartar emetic and various kinds of exercise. Elvert pointed out this circumstance, in his work "*Ueber den Selbstmord*," published in 1794; and Dr. Arntzenius alludes to a case reported by Pruys, (Rott. 1828,) in which similar displacement of the colon was found in a boy who had been fatuous from birth, and died of quotidian intermittent, with violent delirium. Cases of displacement are also referred to, as being mentioned by Desgenettes, Ballin, Henze (*Hufel. Journal*, 1822,) and Falret. Contraction of the colon has been previously noticed by several authors as of frequent occurrence in maniacal subjects; but this appearance is not uncommon in patients dying of other diseases. Dr. Schlegel relates the cases of two females, who, in consequence of the pains which they complained of in their bowels, inflicted extensive wounds with knives in the situation of the pain. In one, during a fit of melancholy, a cut was made through the abdominal walls, six inches in length. The intestines escaped, but were replaced, and the patient recovered. She afterwards excused the act, on account of a "painful sensation in the belly, from which she wished to relieve herself; a consequence of suppressed menstruation without pregnancy." The subject of the other case lost her life, from the wound which she inflicted with the view of relieving a constant abdominal pain.

The liver, continues Dr. A., is far less frequently found diseased in suicides, and even in maniacs in general, than the commonly entertained opinion among the older physicians would render probable. In 168 examinations of patients affected with melancholia, Esquirol only found two instances of organic lesions of this viscus; and Pinel only found five in 259 examinations of the bodies of lunatics. But considerable functional disorder of the liver may exist during life, and leave no organic trace. Worms (*lumbrici*) have sometimes been found in the biliary ducts, as well as in the intestines.

Bichat was of opinion that the pancreas was often affected in hypochondriasis, and it has sometimes been found diseased in suicides. Dr. Arntzenius quotes Heister's relation of two cases, in one of which the pancreas was scirrhus, and in the other enlarged and gorged with dark-coloured blood: in both cases there was vitiated bile and disease of the gall-bladder and excretory duct. A case of a suicide, in which a large *lumbricus teres* was found in the pancreas, is quoted from Mauchart; but in the same case there was induration (*completam concretionem*) of the lungs, liver, and spleen, and other morbid appearances.

Dr. Arntzenius alludes to the "English malady," in proof of the spleen being often implicated as a cause of melancholy and of suicide; but its lesions are not, it would appear, very frequent. Renal calculi are mentioned by Lorry as occurring in melancholy subjects; and a case is quoted from in which the scrotum was full of calculous concretions. In women, the ovaria are sometimes diseased.

Researches into the morbid anatomy of suicides failing to shew more than the intimate connexion existing between such cases and cases of general insanity, and between the moral affection and corporeal disease, and leading to no satisfactory conclusion as to the primitive cause, the author relates several cases of suicide, with dissections, to afford an opportunity of comparing the moral causes and the organic lesions, before giving the opinions of different authors respecting the seat and nature of this unhappy disease. The chapter entitled "*Historiæ et Autopsiæ*" contains an account of twenty-seven cases, original or extracted from various authors. Many of these are very interesting and valuable, both in a psychological and pathological point of view; but our limits will only allow us to extract one as a specimen: it is reported by Dr. A. himself, and is for nothing more remarkable than for shewing the fallacies of diagnosis.

Case 25. A man, forty-six years of age, of robust constitution and very active habits, temperate in diet, and of a happy disposition, experienced a loss of property which, although not very considerable, strongly affected his mind and his feelings of honour. Before that time, after the strongest exercise, his breathing had never been affected; but after this mental emotion he suddenly became troubled with difficult respiration, a sense of pressure in the right hypochondrium, a dry cough, pains in the shoulders, and depression of mind. No measures relieved him. For nine months he continued to get worse; palpitation, loss of sleep, and fear of death supervened; his temper became morose, he drank spirits, he grew fat, and became subject to hæmorrhoids. At length, on returning one evening from a convivial party, he complained of extreme heat and anxiety, walked up and down his room, would not go to bed, and begged the bystanders to take care of him. After some hours he became more tranquil, and as if meditative, and suddenly threw himself out of the window into the street, by which he suffered compound fracture of the thigh and other injuries. Copious bleeding and appropriate treatment restored his sensibility, but he had no recollection of what had taken place. Symptoms of commotion of the brain followed, but the previous symptoms of diseased heart entirely disappeared. In seven weeks he resumed the management of his affairs, but appeared more taciturn, anxious, and timid, than he had formerly been. By degrees, and in the course of several months, his former symptoms returned, and he became subject to violent accessions of

anger. He now also complained of deep-seated, continual, and circumscribed pain in the middle of the occiput, of a feeling of weight in his head, of specks and sparks before his eyes, and of frequent vomiting of bile. Organic disease in the head was supposed to exist, and trepanning recommended but not performed. Nothing afforded him the least relief. He often sate for hours in one place, inattentive to all that passed around him; and often declared that he should go mad. Furious symptoms succeeded, with a desire to murder those near him, and he was removed to a lunatic asylum, from whence in seven months he returned home, looking well, and appearing to be of sound mind, but depressed, silent, averse to society, and restless as if from severe pain. He complained of cephalalgia, vertigo, and anxiety about the præcordia; but he continued for two months to transact business, seemed sane, was careful in his diet, and went about without restraint. About that time his wife became ill, and he removed to a separate bed-room, that out of the window of which he had formerly thrown himself. On the very first night of being there, he fastened the door on the inside, and when it was forced open, he was found to have hanged himself. It appeared that he had been making preparations for this act for some days.

We may suppose the feeling of interest with which an examination of the body would be made in this case. The first result was, that there was no morbid appearance of any kind in the brain or in the spinal cord. The heart was very much enlarged, its base projecting near the sternum, its apex uniform and reflected backward upon the convex surface of the diaphragm; the muscular substance of both ventricles hard and thickened, their cavities very small in proportion, and containing scarcely two drachms of red fluid blood, resembling syrup; the valves and columnæ carneæ thickened and rigid. The external superficies of each auricle, although they contained only a small quantity of red glutinous blood, nearly exceeding by two-thirds the superficies of the ventricles: the cavity of the right auricle thrice the size of that of the right ventricle, and its parietes very thin, and resembling the parietes of the urinary bladder after maceration in water and forcible distension of the muscular coat; so that interstices of from half a line to two lines were observed between the muscular fibres, which were only contained by the investing pericardium, itself in many places as thin as the pia mater, and pellucid. The left ventricle, although larger than natural, in other respects normal. The right lung everywhere adherent to the pleura and pericardium, dense, and gorged with blood; little crepitation on division of its substance, and frothy blood with pus flowing from the divided surfaces. The left lung healthy, but full of blood. The liver of natural size, its upper surface so convex as to carry the diaphragm as high as the middle of the chest; its substance harder than natural; its colour a deep violet. The spleen of enormous magnitude, weighing twenty-one medicinal pounds, and very soft: the vasa brevia and all the abdominal veins much dilated. Varicose vessels on the parietes of the stomach at the great curvature. The ileum exhibited a diverticulum resembling the vermiform process of the cæcum coli, but of three times the thickness, and three inches and a half longer. The omentum and mesentery much loaded with fat; the plexus solaris of a rose-colour and harder than usual.

The reader of all the cases so carefully detailed by Dr. Arntzenius will

not, we fear, find himself much advanced towards a knowledge of the seat and nature of suicide; but he will learn that in every case of suicide dissection would probably reveal actual disease of the brain, or so much disease of other organs as might reasonably be supposed to have violently disturbed the cerebral functions.

It is in consequence of the omission of such examination that the remarks made by Dr. Schlegel in the cases of suicide arising from physical, and those from non-physical causes, are not available in a scientific investigation. He speaks of cases of suicide consequent on disease or organic disorder of the body, and of those arising from fear of punishment, deficiency of food, gambling, unhappiness in wedded life, poverty, &c. The former he terms physical, the latter non-physical. But the question still remains to be answered, how far these latter causes have or have not been productive of certain tangible or intangible physical changes, on which the suicide is consequent, and whether they must not therefore be classed with the former? Dr. Schlegel does not appear to participate in the feelings of compassion with which suicides are commonly regarded. He speaks of the act as being "mostly spontaneous, undertaken after free deliberation, and accomplished during perfect consciousness, and one which must be regarded as among the greatest and most abhorred crimes for which an individual can be responsible, and which the cowardly and vicious perpetrator can commit but once." This is a bold judgment to pass, and one in which we cannot coincide. The impulse under which a human being is compelled to take away that which is so valuable as life, or, on the other hand, the state of mind in which he regards it as so valueless as no longer to be worthy of preservation, are conditions on which we can probably pass no correct judgment, and where silence would be the wiser course.

In cases where a tendency to suicide is declared, (and it is not unfrequently avowed by those who show that they were sincere in the avowal,) the practitioner is not left wholly without indications of cure or mitigation of the mental malady. The important lesson taught by the details communicated by Dr. Arntzenius is one which disposes us still less to coincide with the German author in his condemnation of suicides. We close his book by a conviction that, upon all unhappy beings who have died by their own hands, the charitable sentence should be that they were insane. The appearances after death are identical with those commonly found in cases of mania and melancholy; and the previous history of the patient will probably in every case shew a signal departure from ordinary habits previous to the event. The act of suicide is not the effect of specific morbid lesion; it may ensue on any disturbance of the brain, primary or secondary; and the introduction of the particular idea of self-destruction may often be unaccounted for by the nature of existing disease, and only capable of explanation by a more intimate knowledge of the habitual trains of thought of the individual than we can often in any way obtain possession of. The same difficulty, and to the same extent, exists in other cases of monomania; cases in which a single idea, originating in mystery, takes dominion over the whole mind. This may even be the case when there is not only no visible disease of the brain, but no notable affection of any other organ. The healthy condition of the brain may

have been suspended in consequence of a violent moral impression; its energy may be impaired and its substance apparently unchanged.

Dr. Arntzenius quotes Dr. Gall's opinion, that suicide arises from too great a predominance of the organ of cautiousness; but later phrenologists, and particularly Mr. Combe, with more correctness add that with this predominance a deficient development of Hope, and a large Destructiveness, must be combined. The cause of phrenology gains little, we fear, from these attempts to be at once brief and precise. The diseased ideality, the morbid conscientiousness must often be taken into consideration; and peculiar modes and degrees of action of other organs.

ART. V.

1. *The Philosophy of Health; or, an Exposition of the Physical and Mental Constitution of Man, with a View to the Promotion of Human Longevity and Happiness.* By SOUTHWOOD SMITH, M.D., Physician to the London Fever Hospital, &c. &c. *Volume Second.*—London, 1837. 8vo. pp. 448.
2. *The Philosophy of Living.* By HERBERT MAYO, F.R.S., Senior Surgeon of the Middlesex Hospital.—London, 1837. 8vo. pp. 318.
3. *Management of the Organs of Digestion, in Health and Disease.* By HERBERT MAYO, F.R.S., &c.—London, 1837. 8vo. pp. 198.
4. *The Economy of Health; or, the Stream of Human Life from the Cradle to the Grave: with Reflections, moral, physical, and philosophical, on the successive Phases of Human Existence, the Maladies to which they are subject, and the Dangers that may be averted.* By JAMES JOHNSON, M.D., Physician Extraordinary to the King.—London, 1837. 8vo. pp. 229.
5. *A Popular Treatise on Diet and Regimen; intended as a Text-book for the Invalid and the Dyspeptic.* By W. H. ROBERTSON, M.D.—London, 1835. 8vo. pp. 251.
6. *A Treatise on Diet; with a View to establish, on practical Grounds, a System of Rules for the Prevention and Cure of the Diseases incident to a disordered State of the Digestive Functions.* By J. A. PARIS, M.D. &c. *Fifth Edition, corrected, enlarged, and nearly rewritten.*—London, 1837. 8vo. pp. 414.
7. *Observations on the Preservation of Health, in Infancy, Youth, Manhood, and Age; with the best Means of improving the moral and physical Condition of Man.* By JOHN HARRISON CURTIS, Esq.—London, 1837. 8vo. pp. 128.
8. *Education Physique des Jeunes Filles; ou, Hygiène de la Femme avant le Mariage.* Par A. M. BUREAUD-RIOFREY, Docteur en Médecine de la Faculté de Paris, &c.—Paris, 1835. 8vo. pp. 352.
Physical Education of Young Women, &c. By A. M. BUREAUD-RIOFREY, M.D. of the Faculty of Paris, &c. &c.—Paris, 1835.

9. *The Philosophy of Living; or, the Way to enjoy Life and its Comforts*. By CALEB TICKNOR, A.M. M.D.—New York, 1836. 8vo. pp. 334.

THE above are the titles of but a portion of the numerous publications which have recently issued from the press for the instruction of general readers. However various in method and in merit, we see the announcement of all such with satisfaction, believing that the public are in some degree benefited by each. The world has readers of all sorts: some are attracted by a grave treatise; some are pleased with a popular view; some disregard higher recommendations, if the decencies, the foibles, and prejudices of the world be but respected; and some are infallibly caught by those who angle with a long title-page.

I. We derived so much gratification from Dr. Southwood Smith's first volume, that we waited almost impatiently for the second. Like the first, it is full of information, conveyed in very lucid language, although we think the subjects treated of in it are, many of them, such as will not impress cursory examiners with a true idea of its great interest. The running title-pages include many particulars from which idle readers may perhaps turn away; affrighted by *Calculations, General Results, Experiments, Development of Glands, Chemical Action, Ultimate Processes, Evidence of Absorption, Endosmose*, and many other terms indicative to the physiologist of the completeness of the range taken by the author, but less attractive to the superficial and selfish persons for whom authors, wiser in their generation, are careful to provide allurements. Associating habitually, we should conjecture, with a circle distinguished by a high degree of mental cultivation, and the preservation, by mutual example and influence, of great intellectual activity, Dr. Southwood Smith has written a work which is eminently adapted to the readers which such a circle may be expected to create, even to the youngest of such a circle. To furnish such readers with such information,—information which it is always most difficult for them to procure,—and in terms and in a manner adapted to their taste and acquirements, indicates an ambition of a much higher kind than animates popular writers on medicine in general.

The present volume of the "*Philosophy of Health*" comprehends the subjects of Respiration, Animal (and Vegetable) Heat, Digestion, Secretion, Absorption, Excretion, and Nutrition; completing, according to the author's plan, the description of the processes of organic life, "as far as those processes have for their object the conservation of the individual being." The subject will be continued in future volumes, by a description of the nervous and muscular systems, and an account of the action of air, water, heat, light, and electricity, on the different organized structures; and from this survey Dr. Smith purposes to deduce the proper rules "for the management of food, clothing, sleep, exercise (bodily and mental), and whatever other agent, under human control, influences the development of the physical and intellectual powers, and their due balance." When these subjects have been treated of, the consideration of the mental phenomena will be entered upon. This design, it will be observed, then, extends far beyond that of a mere popular book; being

no less than that of influencing, through the earliest instructors, the minds of the young, with a view to increasing the amount of human happiness, by increasing their possessions in a department of unquestionably useful knowledge.

"Few," says the author, speaking of this extended view of his subject, "at present recognize its importance; but he who has a clear perception of the means by which human beings will advance to a higher and happier state, if they advance at all, can have no doubt that knowledge of this kind, made part and parcel of the mind, in consequence of having been received at a very early period of life, is destined hereafter to exercise an all-pervading influence over the speculations of the philosopher, the discipline devised by the practical educator, the institutions sanctioned by the moralist, and the laws positively commanded by the legislator." (*Pref.*)

The first chapter of this second volume (the eighth of the work,) is on Respiration. Dr. Smith sets out by stating that "no organized being can live without food, and no food can nourish without air;" and that, of the two, the necessity for the supply of air is the more urgent. He then gives a clear and pleasing account of the manner in which the air and the food of a plant react on each other in the leaf; explaining the digestive function performed by the leaf's upper surface, and the respiratory office of the lower. A fact often alluded to, perhaps without a very clear conception of its cause, is familiarly explained in the following passages.

"The physical agent by which this chemical change, which constitutes the digestive process of the plant, is affected, is the solar ray: hence the vesicles which contain the fluid to be decomposed are placed on the upper surface of the leaf, where their contents are fully exposed to the action of the sun; and hence also this process takes place only during the day, and most powerfully under the direct solar ray; but, although the direct influence of the sun be highly conducive to the process, yet it is not indispensable to it; for it goes on in daylight, although there be no sunshine. Light, then, would appear to be the physical agent which effects on the crude food of the plant a change analogous to that produced on the crude food of the animal by the juices of the stomach." (P. 7.)

The subsequent transmission of the sap thus prepared to the under surface of the leaf, where it is subjected to a process analogous to that of respiration in animals, is then described:

"This operation, which is strictly analogous to that of respiration in the animal, in which carbonic acid is always generated and expired, is carried on chiefly in the night. In this manner, under the influence of the solar light, the leaf decomposes carbonic acid; retains the carbon, and returns the greater part of the oxygen to the air, in a gaseous form. At night, in the absence of the solar ray, the leaf absorbs oxygen, combines this oxygen with the materials of the sap to produce carbonic acid, which, as soon as formed, is evolved into the surrounding air. The carbonic acid gas exhaled during the night is reabsorbed during the day, and oxygen is evolved; and this alternate action goes on without ceasing: whence the plant deteriorates the air by night, by the abstraction of its oxygen and the exhalation of carbonic acid; and purifies it by day, by the evolution of oxygen and the abstraction of carbonic acid." (P. 9.)

Dr. Southwood Smith's account of all these processes in the plant is written with his customary clearness and fulness of illustration, and leaves no part of them obscure on which physiology has yet thrown a steady light. He then explains to the reader the necessity of a greater extent and complexity in the respiratory apparatus of animals, arising out of their superior structure and functional activity; and goes on to describe

the varieties of aquatic and ærial respiration; shewing that the simpler structure and limited range of functions of animals in the lowest part of the scale are associated with aquatic respiration, whilst the higher animals directly and more largely respire atmospheric air.

To whatever chapter we turn, we find the same minute and intelligible descriptions, and the same copious and valuable illustrations. In that on Digestion, there are numerous details concerning the process by which food is converted into nourishment in the various classes of animals, from the animalcules up to man. The descriptions of the human mouth, teeth, and instruments of mastication and deglutition generally, are admirably written; and the functions of the stomach and intestines are so perfectly explained that no reader whom we can suppose ever likely to open the book can fail to comprehend all that relates to the subject. At the same time all the higher and abstruser questions connected with all the subjects are so ably introduced that every professional student may refer to Dr. Smith's pages with benefit. This extent of real merit will yet, we fear, limit the popularity of Dr. Smith's volumes, which we should regret less on his account than on that of the public. His reputation will suffer nothing from being limited to very intellectual readers; but we should be glad to see the philosophical truths, interspersed with his anatomical and physiological details, spread abroad more widely; believing them to have an important relation to many social improvements which, in the absence of such knowledge, no benevolence and no patriotism can carry into complete effect.

II. With views more limited, with an ambition more easily satisfied, and with a distincter aim at certain classes of the "pensive public," Mr. Mayo, in a goodly drawing-room volume, inculcates the *Philosophy of Living*. He commences with an apology for inditing a popular book, an apology unnecessary in any case where the intention is to give useful views of subjects important to the populace; and especially unnecessary in Mr. Mayo's case, already distinguished as he is by his scientific writings. His object in this volume is to bring together and explain the rules which should be observed respecting Diet, Exercise, Sleep, Bathing, Clothing, and Choice of Residence; and to these he adds some observations upon Health of Mind; reserving for a separate volume, according to an odd custom of popular writers, some remarks on the Organs of Digestion. Mr. Mayo quotes, in his preface, the whole of Lord Bacon's Essay on the Regimen of Health, which we have ourselves always considered as comprehending more valuable knowledge than all the books on Diet and Regimen that ever were composed; but this wholesale quotation, of which there are several other instances in Mr. Mayo's book, lays him under the suspicion of book-making. Even book-making, however, may be venial, when the book that is made is good, and well adapted to a good purpose; and with a view to the part of the fashionable world who now and then look into well-bound publications, we pronounce Mr. Mayo's to be both sensible and useful.

Aware of the difficulty of defining the diversities of constitution, Mr. Mayo has yet thought it proper to devote some pages to this subject, under the heads of Temperament, Habit, and Diathesis; words, it must be confessed, often very vaguely employed. We cannot say that we think

Mr. Mayo has been much more successful in his attempt than those who preceded him; and we cannot but feel surprised to find him talking of such an imaginary entity as the *bilious* temperament. How odd it is that regular doctors will persist in reminding us of the witticism of the "Bath Guide:—"

"I'm bilious I find, and my sisters are nervous."

What can be less definite than the information contained in the following paragraphs? (the *italics* are our own:)

"A gentleman with whom I am acquainted, *an eminent diplomatist*, is of the *nervous* temperament. He suffered some years ago with palpitation of the heart, which was *nervous*. A celebrated physician, lately deceased, attended him, and had him bled on two or three occasions; the treatment rendered him worse, and a different plan was adopted. His physician afterwards candidly advised him, whatever he did on the recurrence of these symptoms, not to be again bled.

"Another gentleman of my acquaintance, who displays the *nervous form of a mixed* temperament, with *excellent stamina*, a pulse slow and not excitable, about the middle period of life, when otherwise in health, became subject to nervous feelings, the most serious of which gave him the idea of sudden and imminent dissolution. He would occasionally wake in alarm with this impression, the feeling producing which was liable to recur during the day: Dr. Baillie told him not to mind these sensations, which resulted *from temperament*; and which, as he advanced in life, would leave him. They have done so." (P. 13.)

The chapter on Diet is full of good rules, mixed up, it is true, with anecdotes from all imaginable sources. That on Exercise is equally distinguished by good sense and pleasant writing: it begins with a short and admirable explanation of the apparatus of motion, and to this succeeds much good advice concerning the exercise of the young and of the old. We are glad to find Mr. Mayo speaking very cautiously of the effects of what are called gymnastic exercises, in which the body is forced into actions much more suitable to monkeys than to man: "The exercises," he observes, "which pass under that name are inappropriate to boys; to the strongest, indeed, they do no harm, or may not; but children, not the strongest, are liable to be strained by them, and to have the seeds of permanent structural weakness sown by their influence." He speaks with approbation of "systems of exercises, in which the frame is thrown into a succession of attitudes, with gestures of more or less rapidity or force." Any quantity of exercise, he observes, may thus be obtained; "and there is this safety in such exercises, that we may be sure Nature has not overweighted the body; and this advantage, that, by means of an organized system of exercises, every class of muscles, every region of the trunk, each weaker or neglected part, may in its turn be submitted to strengthening discipline." (P. 120.) He does not fail to notice the defect of exercise, and its consequences, in girls: he considers girls as being healthier than boys, and liable only to one disease, "and that is Education." The following is Mr. Mayo's account of the spinal curvature so often induced in young ladies, of its causes and its progress.

"Mr. Shaw attributed the initial curve to the *loins*; Mr. Coulson is disposed to look for it in the *back and shoulder*. The latter writer, however, put forward, with a full general conception of its importance, the principle already quoted,—the general influence of the rightsidedness of our habits upon curvature of the back.

"But neither Mr. Shaw's nor Mr. Coulson's ideas are strictly just. *The two curvatures are not successive, but necessarily simultaneous*; neither has priority; in health and strength both of these curvatures, producing serpentine flexure of the spine,

continually occur, and disappear with the next change of posture. In the weak, they become permanent, and are progressively aggravated.

“The steps by which the spine ordinarily gives way are these. The child kept at its music-stool, or books, or drawing, has a weakened and aching back. The muscles of the spine have not been invigorated by the sportive exertions and the various changes of attitude which nature dictates. Wearied by its task, the next change is to stand listlessly beside its governess, or in a drawing-room. What is the posture which it assumes? It is of course that which gives greatest ease to the languid muscles. The child stands with its weight supported upon one leg, the body swayed to that side, the knee of the other side bent, and the hip lowered. The limb which it uses on this occasion for support is almost always the *right* limb; for this simple reason, that it is the strongest. And the child assumes the position at all times, because it is one of change from its former more rigid position, and because, in addition, the fascial structure of the limb takes off, in that posture, some of the strain from the muscles. Let me, in passing, observe, that what has been already said sufficiently indicates the source of one minor kind of displacement, that is not unfrequent. The right ankle constantly rested on *grows* inwards,—that is to say, the joint *gives* inwards, its ligaments being elongated by the perpetual strain. In like manner, or from the same cause, the knee will give inwards,—one limb becoming in-kneed. The child thus weakened by its habitual in exertion, and tired by the discipline of the morning, is standing supported on its right leg. To judge of what is happening to its back at the same time, place before you a healthy child, and having instructed it to rest its weight alternately on both its legs, and, as in the position supposed, upon one only, observe its back when the alternation to the latter takes place. You may distinctly see that the straight line of its back becomes, in the second case, a serpentine flexure, the *ordinary flexure of curvature*. The mechanical elements of the change are equally obvious. At the time that the weight of the frame is transferred to the right limb, the left side of the pelvis is seen to sink; but the spinal column is attached at right angles to the middle of the pelvis; if the whole length of the column continued vertical to its base, the child would have fallen towards the unsupported side; the column, to avoid this consequence, is instinctively bent at the upper part of the loins to the right, to throw the weight well over to the right side; but the degree of flexion required for this purpose would carry the neck considerably out of the perpendicular; another contrary bend is therefore requisite, which begins in the middle of the back, and terminates at the root of the neck. These are the elements of the simultaneous changes which ensue, the inclination of the pelvis to the left, the flexure of the lower part of the spine to the right, of the upper part towards the left. They may be thus experimentally produced in the flexuous spine of the healthiest child, as quickly redressed, and the spine restored to straightness. *They are thus likewise inseparable, not successive, but simultaneous parts of one action.*

“Let us now apply the preceding observations to children with backs weakened in bone, sinew, muscle. This position of rest, this standing-at-ease, to which they are more prone than other children, and which becomes habitual, brings the spinal column into the following relation to the weight of the body, arms, and head. It is no longer a straight pillar of support; but, so long as the posture is maintained, a flexuous one. That would matter little if all the elements of the column were strong and rigid; but they are weak, debilitated, disposed to yield, and they give accordingly; and the flexures become, not the temporary yieldings of elastic joints, but permanent givings and yieldings of weakened textures. Once begun, the change can but progress, and the greater the obliquity at each part, the greater the mechanical inability of the spine to resist the growing evil. The only difficulty that remained in the theory of spinal curvatures was that which I have attempted to explain. When a curvature of the spine of the back is *convex* towards the right side, it necessarily follows that the right shoulder will be elevated, that the right side of the chest will be fuller, that the left shoulder will drop, the left breast be flat. It is easy, again, to understand how the case may be exactly reversed; how the habitual inclination may be to rest on the left leg, with a parallel train of consequences. Again, it is evident that the serpentine curvature, the *common* origin of which I have explained, may be favoured by or even entirely proceed from other causes; how an invalid in bed may be bent with a con-

vexity towards the side on which she lies; how the spine may be twisted slightly on its axis by one or other habitual motion; how the posture employed in writing or drawing may give rest and pressure to one side and shoulder, expansion and elevation to the other; how the trivial circumstance of the dress of children hanging on their shoulders, and so contrived as to be always ready to fall off, will almost necessitate the practice of perpetually hitching up one shoulder to support the dress, and of letting the other drop within it. The description of the steps by which weakness of the back leads to curvature, explains at the same time the means of preventing the latter, or of remedying its early threatenings." (P. 126.)

In connexion with this most important subject, we request the reader's attention to Professor Stromeyer's views given in our last Number; p. 133.

Some observations occur in the chapter on Sleep which, as their practical bearing is extremely important, should be received, we think, with more caution than they are given. Speaking of the condition of the brain during sleep, Mr. Mayo quotes a case from Blumenbach of a person who had been trepanned, and whose brain was observed to sink when he was asleep and swell out when he was awake; and this is considered to be a proof that when we sleep the cerebral circulation is more languid than when we are awake. Mr. Mayo says that it is no less certain, although it is difficult to prove, that "the nervous power of the brain is lowered in sleep." The bearing of these conclusions is, he observes, of supreme importance. "Brain attacks" commonly occur in the night; and, consequently, at a time when the power of the brain is lowered. To bleed, therefore, in epilepsy, apoplexy, and palsy, especially in persons advanced in life, is not the proper treatment; these attacks result from general or partial cerebral failure: they require repose of mind, gentle exercise, free air, a regulated diet, sometimes medicine, sometimes stimulants. They may, however, Mr. Mayo acknowledges, be primarily induced by "*action in the head*," and may often be traced to full living and excitement, and sometimes the circulation is loaded and labours, and in these latter cases, the diminution of the quantity of blood by cupping is most likely to give relief. There can be no doubt that in aged persons attacks apparently apoplectic occur from causes not requiring or justifying depletion; and that in many cases of epilepsy and palsy, and in the convulsions of children, bleeding is positively injurious, and an opposite treatment beneficial. Any conclusions drawn from the occurrence of such seizures during sleep, however, would, we fear, prove too unsure to be relied upon.

In the chapter on Climate, there is an interesting section on the climate of the factories. Mr. Mayo quotes Dr. Ure's account of the state of the work-people, by which it would appear that under the influence of the steam-engine, the blest genius of the place, crowds of feeble persons are enabled to earn a livelihood, screened alike from summer's heat and winter's cold, and thus procuring abundant food, good clothing, and what Dr. Ure is pleased to call "*domestic accommodation*." These views, as Mr. Mayo justly remarks, rest on partial facts, and do not comprehend the general condition of the manufacturing population. He cites what we conceive to be the juster opinion of the late Mr. Thackrah of Leeds, who observed the children to be "universally ill-looking, small, sickly, barefoot, and ill-clad." The young men were pallid and thin as children: none of the women fresh-looking; altogether, "a degenerated race—

human beings stunted, enfeebled, and depraved." This evidence is strikingly confirmed by the recent testimony of Mr. Wing, part of which we shall here quote:—

"Having provided myself with a thermometer," says Mr. Wing, "I visited —, a coarse mill, and found the operatives, both adults and children of thirteen, worked the twelve hours, exclusive of meal-times,—that is to say, from six o'clock in the morning to half-past seven at night, with half an hour for breakfast at eight, and one hour for dinner at twelve; about four another meal is taken, without stopping the machine. A copper of hot water on the outside supplied the power of having tea and coffee, which the operatives thus obtain: the different parties, as they come, unfold a paper containing tea or coffee, with a little sugar; the contents are placed in their pitchers, the water is poured upon the ingredients, they quickly depart to their different operations, and can scarcely be said to rest at this period of refreshment. In the card-room, a boy, who was in-kneed, sickly-looking, short, and of spare muscular development, and apparently about twelve, I found, to my astonishment, was seventeen years old. 'Had worked five or six years in a mill. On a Monday morning was so stiff could hardly move, and at night was sadly tired; his limbs were straight when he entered the mill.' This unfortunate boy was doomed to work the full time. The temperature of this room was sixty-seven. A boy belonging to this mill having told me he was thirteen, upon being closely interrogated, said that he should be of that age the next month; he worked the full time: my belief is, he was barely twelve. In the dressing-room of this mill there were nine adults, who looked of a factory cast. The temperature was ninety-eight degrees, and the overseer said that when the gas was up, the increase of heat would be to 103 or to 105 degrees. The work necessarily required a high temperature." . . . "In one mill which I visited, there were about forty operatives in the card-room, which was very dusty, and oppressive to respiration. I found the effluvia particularly offensive. To describe the effluvia in some of the rooms is difficult, partaking, as it does, of the combined qualities of the friction of the machinery and of oil; the effect is of a faint and sickly nature. I had an opportunity of seeing spinal deformity, and, upon making the relative enquiry, I found that it was induced by the system, and I saw wounds caused by the machinery." (P. 265.)

It is evident, from these and other passages which might be quoted from this work, that, notwithstanding the increase of popular knowledge respecting health and disease, the principles of justice, a compliance with which would secure larger portions of the community from the penalties of bad air, bad food, and all the evils of poverty, are not yet generally complied with or even commonly adverted to. It is not our duty to dwell, as Mr. Mayo has very properly done, on the moral evils incidental to those who live and labour in the factories. We trust that some among his fair and fashionable readers may be led to reflect on the lot of women especially, who go to their toil from home at very early hours, return only to hurried meals, see little or nothing of their children, preserve no domestic habits, and bring up a young family amidst the disadvantages of an enfeebled frame and almost continual absence from the little creatures who demand a mother's care. Perhaps, too, although on this point we are more doubtful, their sympathies may be roused for the poor infants consigned to mercenary nurses, and early taught to drink laudanum and gin. An occasional word of compassion, addressed to legislators by those who seldom speak in vain, would do no discredit to the female character, and be in no degree opposed to the principles of Christianity. But how seldom are such words spoken! In this portion, and in many portions of his book, Mr. Mayo's sentiments do him great honour; and we feel both respect and gratitude for his endeavours to awaken good feelings as well as to establish good habits among the readers of his elegant and sensible volume.

III. Mr. Mayo's supplementary volume, entitled "Management of the Organs of Digestion," is remarkably illustrative of the little necessity that existed for divorcing it from the former. Seventy pages of it are devoted to diet and indigestion, including several subjects which shew the difficulty of filling even these with appropriate matter, such as sickness of pregnancy, sea-sickness, and vomiting following surgical operations. About forty pages more are devoted to the management of the bowels; and here the book should have ended; or rather, all this, or part of it condensed, should have been added to a condensation of the former volume. The consequence of the contrary plan has been, that when Mr. Mayo arrived at the 109th page of this promised book he found he had nothing more to say on the proper subject of it, and was compelled, against his better judgment we doubt not, to treat the public with an account of all sorts of diseases of the rectum. Of the value of this part of the work we do not speak, but at the best it can only be considered an addition to a popular treatise—made in very indifferent taste.

Mr. Mayo gives some interesting cases in which symptoms, occasionally perhaps mistaken by practitioners for indications of fulness of blood, arose from want of proper nourishment, including one in which epileptic fits were apparently kept off by timely stimulants. Other examples, less liable to be mistaken, are given, in which chilliness on going to bed and a thready pulse, pointed out the propriety of taking some moderate stimulant at night; and it is remarked, judiciously we think, that as those who have mental exertion to go through after dinner, are indisposed to do so by a hearty meal, and yet not sufficiently supported by a very light one, a light supper may be indulged in by them without any impropriety. Some examples are also given of the bad effects of too rigid a diet, with abstinence from animal food, and also of too vigorous antiphlogistic measures, in inducing not only fainting fits, but fits of epilepsy. These are cases to which the young physician will sometimes find himself called in by his elders among the general practitioners, and great firmness is required, as well as discrimination, to prevent a perseverance in the bold measures which have done all the mischief. Vague notions of congestion, or of what is still more vaguely called "general inflammation," beset the mind of the lover of the lancet; and as the symptoms are generally serious, and the result of a wrong decision may be death, the physician has need of all his observation. We have known symptoms relieved in half an hour by a moderate stimulant, for which it had been strongly contended that a large bleeding was essential. The colour of the face, the state of the pulse, and all the circumstances indicative of excitement or exhaustion must be carefully observed and compared in these cases; and the theories of the unreading men, who have their theories no less than the scholar, put aside. The number and variety of symptoms arising sympathetically, and depending on disorder of the stomach, must never be forgotten. Cases resembling paralysis, and even apoplexy, are sometimes of this kind; but then the observer is not to set down every case as sympathetic of stomach-disease. No disquisition on these subjects in a popular work, we may observe, is very likely to be auxiliary to the practitioner, who may vainly contend with the prejudices of the lady of the house, or a clergyman "who knows something of medicine," and has even read Mr. Mayo's book. The following case, degraded to a foot-note, was

worthy of a place in the text. It is illustrative of the effects of a cause merely mechanical.

"Dr. Sweatman attended a lady who had been compelled to remain in the horizontal posture for ten years. If she sat up, in a short time she fell into, or began to fall into deliquium, and fainted; but she looked in perfect health, and had borne several children during this period. The opinions of several eminent London physicians had been taken, who, finding nothing wrong in the circulation, had all recommended tonics. Dr. Sweatman begged to see the patient's legs, which he found covered with immense varicose veins. Into these, when she stood up, all the blood in her body gravitated; and hence she became faint. The legs being bandaged, she was cured at once." (P. 15.)

We cannot imagine a case more forcibly illustrating the necessity of a careful examination before prescribing; or one more calculated to put all those who had been previously consulted very much out of countenance.

There is much sound sense, we think, in Mr. Mayo's remarks on the effects of a lowering plan of diet and antiphlogistic treatment too long pursued after surgical operations. The ill consequences are spoken of as more common in the French hospitals than in our own; which we can readily believe, having witnessed, we fear, the absolute starving to death of some men, some women, and many children, in the Parisian hospitals; but the fault and its consequences are not unknown at home. We see, or used to see, patients sinking after operations from what the older surgeons called "an indescribable state of irritation," which supervened; and we have witnessed even livelier examples in the country. A sportsman, in the prime of life, and the vigour of health, hunting four days a week, and taking a bottle of claret after an excellent dinner daily, and not living very poorly at breakfast, has a fall with the hounds, and breaks his collar-bone, or dislocates his shoulder. Pale and faint, he is hurried home, and surrounded by surgeons; the shoulder after some trouble is replaced, or the collar-bone attended to. Comfort and warmth return: blood is taken; calomel is given, a black draught follows; gruel and tea are allowed, but nothing better; no dinner, no wine. All this is well, or even quite necessary; but in a few days, more or less, it is neither well nor necessary. The patient begins to feel the exhaustion produced by his sudden low diet, and his doses, repeated too often, of calomel and draughts. The dinner comes, no dinner hour to him; no wine revives, no evening company consoles him. Both mind and body suffer. He becomes irritable, restless, yawning, very sensible to noises, sleeps ill, and thinks the day as well as the night will never end. He becomes alarmed, and sends for his surgeon. Even in these cases bleeding is sometimes proposed; and we have seen it proposed to an extent which we think would have been fatal, and in which, at least, the stimulus of strong coffee, or a moderate dinner, and a little wine, would have entirely restored the patient to feelings of comfort.

Critics more captious than ourselves might take exception at the manner in which the information contained in Mr. Mayo's book is frequently brought before the reader. "A very learned and distinguished person told me." "An artist of eminence I had occasion to prescribe for." "A gentleman now in his eightieth year, who is under my care for a local complaint." "A few nights ago a gentleman sent for me." "In a case, in which I performed amputation at the hip-joint (the patient happily did well,) for a few hours the utmost prostration existed," &c. &c.

These we look upon as inadvertencies, which to be disapproved of need but to be seen; but seen too oft, they are endured, considered allowable, and imitated. The gentleman who sent for Mr. Mayo "a few nights ago" had a violent fit of ordinary indigestion early in the morning, and was cured by an emetic of salt and water, of which Mr. Mayo speaks very favorably. Half a teaspoonful of common salt is given in a tumbler of tepid water, and a second in a few minutes. This is said not only to cause vomiting of the offending matter, but, if it has already been in a great measure got rid of, to calm the stomach, and to act as an aperient. These slight attacks of indigestion, it is observed, if neglected, are succeeded by diarrhœa, or by an indisposition of several days, with loss of appetite, heartburn, a furred tongue, and general uneasiness. Mr. Mayo gives ample and judicious directions for the treatment of what are generally called, with doubtful propriety, bilious attacks: but it might have been well to warn his readers, and particularly his London readers, and more particularly still, his lady readers, against the habitual resorting to calomel and black draughts; which, producing immediate relief, are often had recourse too so frequently as to convert an occasional into an habitual invalid. The use of a simple aperient pill, followed by a domestic enema, or even an occasional enema without any pill, is well adapted to cases in which torpor of the bowels characterizes the intervals between one of these favorite doses and the next; and will generally render calomel and strong medicine unnecessary, to the great comfort of the intestinal canal.

The plan of Mr. Mayo's book seems to be to teach by the introduction of cases; which are given to exemplify the state of habitual indigestion, and the effects of particular kinds of diet in relieving very aggravated suffering, although no general rules can be deduced from the relief afforded in particular instances. He thinks it superfluous to enlarge on the good effects of bodily exercise, friction, change of occupation, mental relaxation, and amusement, with change of air and scene, as means of cure; but makes more particular observations on the use of aperients, including the dinner pill, that ingenious invention of luxury, unwilling to forego enjoyment yet anxious to escape the penalty of excess. He recommends it in the shape of four grains of rhubarb and one of carbonate of soda; and a night pill of equal parts of Castile soap and compound extract of colocynth; "or the same," he says, although we do not know whether he means the dinner or night pill, with two grains of scammony, or, instead, a drachm of the lenitive electuary; "or, in the morning, a drachm of Epsom salts in peppermint water, alone, or with two drachms of tincture of rhubarb; or two drachms of Cheltenham salts, in a third of a pint of water." (P. 43.) These directions remind us of an acute saying of Mr. Landor, that, in conversation, people do not expect to be instructed, but to have their own ideas reflected. We know many habitual dyspeptics who take all these things, dinner-pill, night-pill, morning draught, and occasional electuary, but few who are the better for them. The public require to be told that medicines of this kind are but the substitutes for the common sense which would dictate exercise and good regimen; and that when the auxiliaries of rhubarb, and soap, and Epsom salts become habitually necessary, the case of the dyspeptic is almost hopeless. The purgative school has, we are convinced, much to answer for: every day we meet with its victims even among the young, parti-

cularly among young ladies, who are sent to school with a box of family pills, just as regularly as with a spoon, silver fork, and the other requisites. To induce them to abandon the practice of pill-taking, and to enable them to do so by the substitution of exercise before breakfast, and at other intervals during the day, is to save many of them from being invalids all their lives, and almost useless to their families, if not plagues to every body about them.

It is well observed by Mr. Mayo, that a combination of tonic remedies with purgatives is often advisable; and he quotes a striking case of gastrodynia, from Dr. Abercrombie, in which nothing proved, after the trial of many medicines, of so much use as a combination of two grains of sulphate of iron, with four grains of aromatic powder, and one grain of aloes, taken three times a day. These are the cases, so often met with by the physician, in which the unfortunate patients have taken all kinds of mercurial preparations, not only without advantage, but with infinite detriment.

Mr. Mayo considers the disorders of the stomach with reference to its secretion, its mode of sensibility, and its action. Female readers, afflicted with spasms of the stomach arising from any of the little contrarieties of life, will be glad to find themselves advised to have recourse to sal volatile, brandy, until "wind breaks off the stomach," and, if these fail, to take a few drops of laudanum. There can be no question that all patients of this kind will entertain a very high respect for Mr. Mayo's opinions. He warns them, however, against indulging in increasing doses of the latter remedy; and does not forget to observe, that pain of the stomach, although often a trivial malady, may depend on serious disease. But, still inclined to make the readers their own physicians, he tells them, if there is tenderness on pressure, which he admits may indicate ulceration of the lining membrane of the stomach, to apply leeches and blisters, and to take the milder preparations of mercury; using, at the same time, abstinence. We have nothing to object as regards the remedies; but what can be said of placing them in popular hands, when fatal disease may be impending? So also, with respect to the subject of vomiting, the directions are generally good, but it can be of small use to the popular reader to peruse cases in which this symptom yielded to "sugar of lead and opium."

The observations on the different forms of diarrhoea, and their management, are plain and practical: but we are surprised to find Mr. Mayo asserting (p. 86) that "the indigenous cholera of this country is characterized by purging of bile;" and the account of Asiatic cholera is surely somewhat uncalled for.

Among the effects of habitual constipation, Mr. Mayo enumerates indigestion in all its forms,—every disease of the rectum, headach, hypochondriasis, fulness of the cerebral vessels, palsy, and apoplexy. Mr. Mayo is a greater advocate of the habitual use of purgatives than we profess to be. The importance, on "many occasions," of using "the principal means that are calculated to correct this complaint (constipation) when it does not exist;" and the "value" of aperient medicine "in numerous cases where the bowels are not costive," are, if truths, such as we are scarcely prepared to receive. There are, no doubt, some cases that might be cited in support of such a doctrine; as that of an

existing tendency to plethora; but we fear that this opinion, if even medically true, is not devoid of danger applied to popular practice; whilst the rule which Mr. Mayo deduces from the supposed principle, that "when the system is out of order, and there is no failure of stamina, in three cases out of four it is only necessary to purge; that is to say, to excite great increase of action in this one secretory organ,—and the disorders of the rest subside," (p. 100,) is, we are perfectly convinced, pernicious in a high degree. The theory, often as it is acted upon, is seldom so openly avowed; and the avowal, in the work of so distinguished a physiologist, will be very welcome to many a student and many an idle practitioner; leading to a compendious and ready practice, without the fatigue of thought or the anxiety of doubt; but we question the innocuousness of the practice, nevertheless. It may often relieve present troubles, but it lays the foundation of others, which will rise up in after years; and, as we suspect, among them some of those general and local affections of the bowels which occupy so considerable a portion of Mr. Mayo's volume. Indeed, for the relief of occasional constipation, Mr. Mayo does not himself advise medicinal interference. Where it arises from neglect of bodily exercise, the cure, he says, is easily suggested; and walking or riding before breakfast is most salutary. Thus far we agree with him; but not when he recommends afternoon exercise as preferable for women, and approves of seven o'clock as a dinner hour, because it allows "from ten to five for business, and leisure for exercise after it." (P. 101.) Such recommendations are very agreeable to fine ladies, and to their busy, excited, ambitious husbands; but they are not, we conceive, quite so useful as agreeable.

The following observations are of much practical interest, in relation to mechanical means now very frequently resorted to in this country, and long almost universal in some parts of the continent.

"Those in whom fæces are formed in adequate quantity, but are not voided through sluggishness of the muscular structure of the bowel, or because there is not liquid enough, often obtain perfect relief by the use of injections of water. The water should be used cold, to be more stimulating; to render it still more so, half a teaspoonful of salt may be added. The quantity ordinarily requisite is a pint. It is sometimes, of course, proper to use warm water. Many persons, who are temperate livers, and of sedentary habits, find the daily use of such an injection, half an hour before the customary time of relieving the bowels, sufficient to obviate troublesome costiveness, and to cure the local complaints it has produced. But this remedy does not answer with all: in some it produces uneasiness and a sense of weight and dragging from the loins, with general lassitude; in others, the bowel becomes relaxed, and a disposition to protrusion is formed through it. Nor is the practice of injections wholly free from more serious accidents. I have known the intestine torn, and the gravest consequences ensue, through inexperience in the use of the instrument. The tube of the apparatus is generally made so long and sharp, that it is easily capable of tearing and lacerating: properly, it should not exceed an inch and a half in length, and should end in a smooth sphere, half an inch in diameter, tapering to a neck a third of an inch thick." (P. 102.)

The remarks made on the administration of different purgatives require no comment from us, except that we fancy we discern the same accommodating conformity to popular notions which we have already taken the liberty of noticing, when Mr. Mayo says there are some persons whom "mercury always suits;" and that, with these happily constituted

individuals, "it serves as the best alterative, the best aperient, and, indirectly, the best tonic;" all which must be considered very questionable.

The general subject of the present article excludes us from any particular notice of the remainder of Mr. Mayo's book, which constitutes nearly one-half of it, and comprises Fulness of the Hemorrhoidal Vessels and Bleeding, Piles, Prolapsus, Fissure of the Rectum, Contraction of the Anus, Stricture of the Rectum, Substances in the Rectum obstructing the Passage, Defects of the Lower Bowel existing at Birth, Laceration of the Intestine, Abscess and Fistula, and Cancer of the Rectum. On all these matters, whatever may be thought of laying them before general readers, we do not doubt that every reader will find very useful practical remarks in Mr. Mayo's pages; and his general observations on the causes and prevention of affections so peculiarly distressing to many who would otherwise employ the later years of life in pursuits the most worthy of rational beings, but are condemned to daily suffering and wretchedness in consequence of some of them, are valuable to all readers. He lays the greatest stress on abstemiousness; on avoiding the habit of costiveness, and on a scrupulous observance of cleanliness. All the rest, we think, would have been better omitted; for, if these means fail to prevent the occurrence of the lightest among the disorders of the lower bowels, the only security, as regards continued comfort, is to be sought in the advice of a physician or surgeon. As some of these disorders are also often imagined to exist by the nervous, we fear Mr. Mayo's book will do some harm to a few of his readers, unless it leads them to have recourse to professional opinion.

IV. In our notice of the two volumes from which we have just parted, we have entered so fully into the means of preserving and restoring health recommended by their author, that we may be excused for not taking up the subject again on turning to the work of Dr. Johnson, which is consecrated to the same benevolent objects: and we the more willingly adopt this course, because what we shall have to say of the *manner* of Dr. Johnson's volume will leave us but little space to animadvert at any length on its matter; which, however, must be allowed to be, on the whole, both entertaining and valuable. It is not very often that we dwell minutely on the mere style of an author. The advancement of medical knowledge is so great a benefit to the public that it may well excuse many literary defects in any writer; although, generally speaking, we ought to expect that a medical author should express himself like a man of education. Every man has his own style, formed by his course of reading and his modes of thinking; and his own style is best suited to his own thoughts. Even in the intricate sentences of John Hunter, we seem to perceive a great genius making his way through the fogs and mists which hang about other men's minds. But we consider that we have a right to look for a correct manner of writing and a style befitting a learned profession, when a physician takes upon himself to speak for his brethren to the general public, and to explain to them what rules of health the studies and experience of his colleagues have succeeded in establishing. We do not like the excellent wisdom of medicine to be disfigured with gaudy ornaments, and truths of great importance to the happiness of society to be delivered in the slang of drawing-rooms, or the gossiping dialect of nurses, or the wretched drivel of feeble-minded and

pampered hypochondriacs. Medical science is then degraded from its true rank.

The truthfulness of criticism compels us to say that we have fallen into this train of reflection on looking over the pages of Dr. James Johnson's "Economy of Health, or the Stream of Human Life from the Cradle to the Grave." He disappoints our reasonable expectations. The general aspect, the very title-page of his book, has that suspicious, or, we should say, that meretricious character which a man, anxious for the true respectability of his profession, cannot remark without regret. We recognize the great model, of which, in the course of our critical duty, not a few imitations have come before us. The imitation of such things is indeed easy, requiring only a negation or absence of all estimable qualities of manner: but how such a model can be exhibited by a physician possessing so many excellent qualities, largely experienced, not to be suspected of a want of information, and accustomed to the daily collision of a world in which good sense is not quite trampled under foot, we are really at a loss to explain. Not in this work alone, but in *Essays on Morbid Sensibility*, and on *Change of Air and the Pursuit of Health*, and on *Autumnal Relaxation*, &c. &c. we discern, too plainly to be denied, the same catching at the folly and weakness of the day, and the same appeal to those whose approbation confers small honour. The results are curious: in each we turn away, wearied, and more than wearied, by such a vertiginous commixture of subjects as no brain can bear. In every one, and in almost every page, we see the same violent quotations of the poets,—the same unnatural intertwining of Latin and English,—the same literary impromptus made at leisure,—the same humorous headings of chapters,—the same dashes, hyphens, notes of admiration, and other wonders of the printer's art,—the same lavishness of capital letters,—the same comprehensive type, and the same somewhat indifferent paper. But the strangest thing of all is, that we find much evidence of strong good sense, a tone of experience not to be mistaken, and, assuredly, a bonhomie which disarms the severity of criticism.

There are of writers not a few, to whose reputation it would do great service if they were occasionally debarred for a time from pen, ink, and paper. It is undeniable that, with the author whose peculiarities now occupy our attention, writing has become a habit; and there are probably few intervals of his busy life in which he does not write out every thought that passes through his active brain. By a fatal facility, every thought thus written finds hasty way to the printing-office, and is fixed in types for ever. As must happen, and always does happen in such cases, the first works are the best; for authors of this kind always contrive to let themselves gently down, by beautiful degrees, until they perish wholly by their own recording hand. There is much that is worthless in their first compositions,—more that is worthless in the second,—and the adjective is subjected to its superlative degree of comparison in the closing portion of the series. The subjects of the later works, being anxiously sought for, necessarily become more forced; the quotations grow more distressing, the dashes are more desperate, the headings of chapters more wilful and barefaced, and the capital letters more outrageous. Even the good sense can scarcely hold out for ever, or, as Dr. James Johnson would say, "to the crack of doom;" and the bonhomie gives

place to a bristling, challenging air, defying critics who delight to find fault with original writers. Such was once the career, and such the fate, of Dr. Kitchener, as may yet be instructively seen in his *Art of Invigorating Health*, his *Peptic Precepts*, his *Principles of Cookery*, his *Treatise on Spectacles*, and on *Reading and Accent*, and a score of other works which had their glittering day. Such, and so oblivious, will be the fate of Dr. Johnson, unless his good sense comes to the rescue. Without that help, his literary life will never, to quote one of his felicitous creations, reach its TENTH SEPTENNAD.

The *Economy of Health* presents more than its share of the faults we have tenderly alluded to. In a few pages of introduction, staring capitals meet the eye, blazing forth the words HEALTH—TIME—POVERTY—AFFLUENCE—FAME—POWER—SCEPTRE—PRINCE—BEAUTY—LITERATURE—SCIENCE—RELIGION—CHOLERA—HYGIÈNE—COMMUNITY. These words, starting out from undistinguished type, are well-adapted appeals to idle people, who open new books in publishers' shops; and no critic, however lenient, can fail to perceive that, being interpreted, their signification is, TRY WARREN. Then, health being happiness, Pope of course is quoted,—“Oh happiness!” *et cetera*; and, fame being alluded to, we have “the pillow of Napoleon.” The Hindoos, the Egyptians, the Hebrews, the Greeks, and the Jews, are not forgotten. Lord Byron, Ajax, Ulysses, Lycurgus, Xenophon, Pythagoras, and Sir John Sinclair, shine together; and the eleven pages into which this galaxy is crowded end with the particularly unnecessary and novel assurance, in any work of the author's, that he shall embrace

“Quicquid agunt homines,” &c. &c. &c.

It must be, that, after Dr. Johnson has written the *sense* part of his books, he entrusts his pages to some engrafter of poetry. No sooner is Pope done with, than unhappy Goldsmith is seized upon: “The soul's dark cottage,” and so forth; then comes Horace; then Goldsmith again; then Horace again; then a French monarch; the old story of “*toujours perdrix*,” in capitals too; then Shakspeare; all in twenty more pages. This is the very wantonness of literature.

Turn where we will, the same sportiveness confronts us. Is the fashionable world mentioned? straight we have “the gay, licentious proud.” If love is spoken of as a disturber of life, he comes as Cupid, with Nox and Erebus, with the Upas, and with Juvenal and Persius; and then we have Shakspeare, with “She never told her love;” and, worse still, with this abominable addition, “*but* Shakspeare knew not a tithe of the numerous links in that extensive chain,” &c. &c.; which shews how much more easy it is to quote Shakspeare from Dodd's “*Beauties*” than to read, mark, learn, and inwardly digest him. Who but Dr. Johnson's assistant quoter could have said of Shakspeare, that “he knew not a tithe”—of anything?

Are young men exhorted to be prudent as to marriage, it is “*Parce puer stimulis et fortius utere loris*.” Do we speak of drink, we have Circe and the turning of the friends of Ulysses into swine. Is the choice of a wife alluded to, we have “the lunatic, the lover, and the poet,” and two quotations more. Is ambition touched upon, we have “Cromwell,

I charge thee fling away ambition." Does Dr. Johnson faintly surmise that it has pleased God to give a tolerable brain to rustics, as well as to ladies and gentlemen, we have two stanzas from Gray: "perhaps in this neglected spot;" and, at p. 176, we are a second time assured that the work embraces "*Quicquid agunt homines, votum, timor, ira, voluptas;*" which would appear from this to be a very favorite quotation. Speaking of the mingled pain and pleasure of revisiting scenes known in early years, when we have reached the "seventh Septenniad," it is recommended that we confine our sympathies with such spots to letter-writing; and we should feel surprise here if Dr. J. did not tell us that "Heaven first taught letters:" but, turn over the leaf, and there we have the very quotation, and with the illustration of capitals: "Heaven first taught LETTERS," as if the poet meant the documents concerned in the projected Post-office reform. Speaking of memory, he passes, by a rapid transition, to the havoc which BEAUTY (in capitals, of course,) makes in the memory of other qualities, and then we have

"If to her lot some female errors fall," &c. &c.

Half a page further on we have four lines more from Pope, most of whose works will be found quoted from beginning to end, we imagine, in some or other of Dr. J.'s volumes.

The number and nature of these quotations is not excused by the manner of their introduction. Few of them arise naturally out of the subject. They do not so much appear to be the recollections of a mind filled with classical images, as to be dragged in perforce with "it is said by the poet,"—"our poet has remarked,"—"the words of the poet," &c.; a phraseology as plainly indicative of the effort to enrich common matters with extrinsic embroidery as can well be conceived. The broken lines amaze the ignorant and vulgar, but give no satisfaction to readers whose approbation is far more to be desired.

But enough of this. Perhaps we take up the subject of these blemishes too seriously: they may only appear so to us. They may be caused by Dr. Johnson's more correct estimate of the mass of the reading public; and of the hypochondriacs, valetudinarians, and wretched sensualists who walk the Steyne or the beach, the pump-room or the promenade, in search of that happiness which they hope to build without any foundations, or who knock at the doors of fashionable doctors to seek deliverance from their ill-regulated minds and pampered bodies. But to such, and such alone, Dr. Johnson's pages seem adapted; and we hope that these unhappy readers, at least, may be able to gather the real sense so ignominiously overlaid. We are tempted to be ourselves figurative, in the author's line. The fine lady congratulates herself that she takes pills unknown to vulgar palates; not smelling of base orris-root, or sticky with starch, but enveloped in leaf of gold. The faithful aloes and steadfast rhubarb, nevertheless, perform their duty, and, as Dr. J. would remark, "cleanse the full," &c. &c. &c., as "our poet says," without respect to rank or station. Dr. Johnson's style is the gold-leaf, glittering but valueless; and, if his practical experience acts a salutary part, despite the tinsel, on the sickly minds of Tonbridge, Hastings, Cheltenham, and Leamington, well and good. Upon such minds the sober reflection, the profound acquirement, the dignified simplicity, which we

have been accustomed to regard as the proper characteristics of a physician's writings, would be quite thrown away.

With these drawbacks, which some may consider trifling, Dr. Johnson has composed an amusing, desultory book, upon every possible subject. Ten chapters embrace nine septenniads, the fifth and sixth being comprehended in one, and each septenniad has its characteristic objects for remark. The fifth and sixth septenniads form one chapter, and this chapter is headed *The Golden Æra*. Out of forty-two subjects, distinctly referred to in this chapter alone, we select, from the table of contents, a few, in order to shew the discursiveness of the author's manner. This extract gives also a few illustrations of the compound terms and types.

“Development of a grand Principle in Hygiène.—ACTIVITY OF BODY AS AN ANTIDOTE TO DEPRESSION OF MIND. Illustrations.—Retreat of the Ten Thousand Greeks, under Xenophon.—Siege of Mantua.—Shipwreck of Captain Byron.—Retreat of Sir John Moore.—Narratives of Bligh and Wilson.—Retreat of the French from Moscow.—Application of this Principle of Hygiène to Private Life.—Græco-Byronian Precept, ‘KEEP THE BODY ACTIVE, AND THE STOMACH EMPTY.’—Misfortunes of the Female Sex.—Ingratitude to Mothers.—Maternal Affection.—Filial Affection.—Punishments in this World.—Suicide.—Hope of Rewards.—Zenith of the Journey of Life.—Retrospection.—Tree of Knowledge.—Probable Effects of KNOWLEDGE.—On Intellect.—On Learning.—On Wealth.—On Rank.—On Happiness.—On Equalization.”

V. Dr. Robertson, of Chesterfield, has compiled a sensible and useful book on Diet, Bodily Exercise, Ventilation, Climate and Change of Air, Dress, the External and Internal Effects of Water, Mineral Waters, and Sleep. This may probably be regarded as one of the numerous works for which the world is indebted to Dr. Combe's admirable publication; but Dr. Robertson has contrived to give a novel air to this subject by his arrangement, and by variety of topics. It is written in a plain unaffected manner, and contains much valuable information. The appended tables of mineral waters give it additional utility to many readers; and Dr. Robertson's observations on the subject of these highly fashionable remedies are instructive and judicious. Seeing so much as we do of the capricious manner in which mineral waters are recommended and taken, we value every fragment of evidence concerning any of them which seems really authentic, as the following, relative to Buxton, appears to be.

“It was my fortune to see Buxton, for the first time, after riding twenty-four miles, the greater part of the journey having been performed in the heat of a summer's day. I dismounted, and in less than five minutes was in the act of undressing for a plunge. The baths are fitted up with great neatness, and are lined with white porcelain. Water is constantly running through them; no small advantage to the man of nice and refined ideas. Perhaps the best thing that I can do is to state briefly the effect which the water had upon myself. On plunging into it, the shock was unusually severe, greater than I ever experienced in ordinary cold bathing; of course greater than I ever felt from bathing in the sea: and this is not a thing peculiar to myself, but, I believe, the usual effect that these waters produce. Reaction, however, came on much quicker than is usual with me in cold bathing; and this was followed, in about ten minutes, by all the luxurious languor which the warm bath produces. This increased until I left the water, (in twenty minutes,) when, expecting to find my skin easily chilled, as it usually is after warm bathing, I was agreeably surprised to find it all in a glow, my strength recruited, my spirits even buoyant, and my appetite voracious. I trust this little bit of egotism will be pardoned; for really so few, except those who go to a watering-place, know anything of its effects, that it may be useful.” (P. 191.)

Dr. Robertson omits to mention the temperature of the water, which is stated in the table at the end to be 82°. He considers the baths of Buxton favorable to all whose cases require a course of temperate bathing, combined with the advantage of a somewhat bleak but bracing air. We very much approve of his observations on the insufficient knowledge yet possessed of the actual qualities of mineral waters, or of the mode in which the ingredients exist in them, by mere chemical analysis. The common idea is, that the water which produces, by analysis, the greatest quantity of sulphate of magnesia or sulphate of soda must be the best, because the most purgative; and patients resorting to Cheltenham and Leamington cannot be persuaded that any other benefit is to be expected than that of acting briskly on the bowels. The impatient desire of the sick for immediate results presents a great obstacle in the way of investigating the effects of various doses of many medicinal substances. Without being followers of Hahnemann, we know that several drugs are given in doses much larger than necessary, and that smaller quantities are more serviceable. Many a man is deterred from giving small doses by the fear of being esteemed a timid practitioner; but, if a few grains of medicine cure a disordered condition of body, we see no glory in the administration of the same medicine in ounces and pounds.

It is indeed strange, as Dr. Robertson remarks, that so much of the real benefit of watering places being produced by the altered habits, improved regimen, and greater activity and cleanliness attendant on the change, it is yet so impossible a thing to induce the same dietetical and regiminal reforms at home, without trouble and without sacrifices; but it is still to be remarked, that the mind is refreshed by change of place, relieved from habitual cares, and rendered capable of more exertion than it can put forth at home, whatever the doctor may advise, or whatever the growing symptoms of debility may threaten. There is no man, whatever his resources or his strength of mind, who does not derive some benefit from change of place; and the facilities now created of obtaining this kind of change will probably be very beneficial, among other things, to the public health.

VI. Dr. Paris's Treatise on Diet is already too well known, both to the profession and to the public, to require any lengthened review at the present day: its merits, which are many, and its defects, which are by no means inconsiderable, are as familiar to our readers as to ourselves; and therefore we shall "bestow none of our tediousness" in recapitulating either, but proceed at once to notice the peculiarities by which the present is distinguished from the former editions, and to enquire whether the work is so entirely changed as the announcement in the title-page would lead us to expect.

The fourth edition of Dr. Paris's treatise was published in 1830, and "it has remained out of print for a considerable period: so far, however, from this being a subject of regret, I rejoice," says the author, "at the opportunity it now affords me of collecting from the stores of a wider experience such facts as may tend to confirm and extend the doctrines, or to illustrate more clearly those general views which were submitted to the profession in the first edition." (P. 1.)

From these remarks, and from the numerous contributions which have

been made to the physiological history of digestion since the publication of the preceding edition, seven years ago, we naturally took up the present volume with the expectation of finding it replete with those improvements which the progress of science demanded, and which the imposing assurance of being "nearly re-written" implied a consciousness on the part of the author of being due to his readers.

On comparing the present with the preceding edition, which we have taken some pains to do, we find it to contain rather less matter, and under a somewhat different arrangement. Instead of being divided into three parts as formerly, it is now divided into four, of which the second, "of Dietetic Observances founded on Physiological Principles," seems to have been suggested by the title of the second edition of Dr. Combe's work: but the novelty lies only in the title; for almost all the contents of that section appeared verbatim in the fourth edition, under different heads. Several pages of old matter have been entirely suppressed, and fifteen or twenty new paragraphs, amounting in all to about as many pages, have been introduced; but very few new facts or views have been contributed from the stores of experience, to which the author refers in the above quotation; and even those which are new have been either simply thrown into the heap or turned to very little account in any subsequent application to practice. In all other respects, then, the fifth is truly a transcript of the fourth edition, with all its excellencies, defects, and omissions.

It is with pain that we feel ourselves compelled to make such remarks on the writings of one who has been neither a slothful nor an useless labourer in the field of science. Had the volume been announced with less pretensions, and simply as a new and improved edition of a work already known, it would never have elicited from us a single disparaging line; but, when it is put forth in the guise of an almost new work, we are bound to put our readers on their guard against purchasing it as such; and, if the truth be disagreeable to any one, the fault lies not with us, but with the author himself, from whom alone the announcement could emanate.

Even where Dr. Paris has made any alteration on the text of his work, we are not aware that he has taken the opportunity of correcting any of his former errors, or of supplying any considerable omissions. Under the head of the anatomy of the digestive organs, for example, two meager paragraphs, about the functions of the nerves of the stomach, are added, in the first of which the author merely remarks, that, "*notwithstanding* the important discoveries of Sir C. Bell, there still remains some obscurity with respect to the influence of each particular nerve on the function of the stomach;" and forthwith dismisses the subject with a few vague generalities. Here, however, a twofold injustice is committed. Not only are the researches of Tiedemann, Gmelin, Philip, Magendie, and, above all, Brächet, passed over without an allusion, although they are the most interesting which have yet appeared on the special functions of the stomachic nerves; but, from the words quoted, the reader is led to consider Sir Charles Bell as the chief, if not the only, enquirer who has thrown any light on that branch of the subject; whereas, he has, in reality, done nothing to elucidate it, and has never entered very minutely into its investigation.

Another remarkable omission occurs when discussing the venous origin of the biliary secretion. Dr. Paris re-produces, *literatim*, the old, and comparatively unsatisfactory, evidence of Saunders, Simon, Abernethy, and Magendie; but never even hints at the existence of the far more conclusive experiments and researches of Kiernan, although the latter have now been some years before the public; and, again, when enquiring into the absorbing or non-absorbing power of the skin, as connected with nutrition, he justly declares the question to be one of great importance, but, nevertheless, does not add one word to his former imperfect account of it; although Edwards and others have since established the affirmative by direct experiment. Nay, even in discussing one of the principal topics of his own book,—Digestion, and its agent, the gastric juice,—he reiterates his former descriptions to the very letter, even to the quoting of Montegre's mistaken conclusions on artificial digestion; and altogether disregards the experiments and contributions of Beaumont, Brâchet, Schwann, Müller, and Tiedemann, which are not only the most recent but by far the most important and instructive which we possess.

The author's neglect of recent information, and literal adherence to his former text, are further exemplified in his very slender notice of the curious experiments performed by Dr. Beaumont, on the Canadian voyageur, Alexis St. Martin; of which we gave an account in a former Number, and which are now familiar to the profession. We consider this omission as blameable in an author writing *ex professo* on the subject to which they relate, because, without by any means adopting all Dr. Beaumont's results, we cannot be blind to the fact that the zeal and ability with which he availed himself of his singularly favorable opportunities, constitute a strong claim on the respect and attention, and even on the justice, of every succeeding enquirer. Dr. Paris cannot plead ignorance as an excuse; for, in one of his earliest pages, he refers to the confirmation of former results by the case of St. Martin as "remarkably striking;" and after quoting, from Dr. Combe's work, "the most interesting facts that were thus ascertained," he promises again "to advert to the observations of Dr. Beaumont;" but, except in the form of a mere allusion, he never does so.

It is not because Dr. Beaumont has made any important *discoveries* regarding digestion that we find fault with Dr. Paris's neglect of his labours. The chief value of his experiments, as Dr. Paris himself justly remarks, consists in the confirmation which they afford of known but hitherto uncertain facts, and in their substituting certainty for doubt where certainty is of great practical value. From the nature of the case, Dr. Beaumont was able to free his researches from many sources of error inseparable from experiments made on animals, or during bad health; and hence he has succeeded in adducing evidence on some points of a much more conclusive kind than any quoted by Dr. Paris; and in so far, consequently, the latter has done less justice to his subject than his readers had a right to require from him.

An amusing inconsistency appears in reference to the very first "fact" which Dr. Paris adopts as ascertained by Dr. Beaumont's experiments. After explicitly concurring with Dr. B. in stating that "gastric juice is never found free in the stomach, but is always excited to discharge itself

by the introduction of food or some other irritant," Dr. Paris very quietly repeats, from his old edition, that one of the reasons why appetite is *not always felt* on rising in the morning is, "*because gastric juice may not be secreted in any quantity during the night*, while the muscular energies of the stomach, although invigorated by repose, are not immediately called into action," (p. 139;) just as if the accumulation of gastric juice in the stomach, in the absence of food, was one of the efficient causes of appetite, and had not been previously declared by himself never to occur!

Another contradiction, which existed in the former edition, and which recurs uncorrected in this, is where he first tells us, at page 35, that "*six or seven grains of the inner coat of the stomach, infused in water, gave a liquor which coagulated more than a hundred ounces of milk;*" and then, farther down on the same page, very gravely adds, "*the coagulating and efficient principle, whatever it may be, is evidently not diffusible in that liquid.*" If the latter be true, what, in the name of wonder, is the liquor which is stated to coagulate the milk? According to our view, the experiment in the first sentence proves the coagulating principle to be *extremely diffusible* in water; and we cannot imagine what Dr. Paris can mean by affirming the contrary. We are induced to notice this contradiction more seriously than would otherwise have been necessary, because he has linked his erroneous inference with an important practical conclusion, shewing "how unfounded that opinion is which attributes to the potation of water the mischief of diluting the gastric juice, and thus of weakening the digestive process." Now if, as we have seen it to be, the gastric juice is really diffusible in water, the fact of its being diluted by such diffusion is surely the reverse of "unfounded." If, indeed, the author means merely that drinking water some time *before* a meal does not dilute the gastric juice, and thereby injure digestion, we entirely agree with him; but then the non-dilution in such circumstances proceeds, not from any repugnance in the one to mix with or be diffused in the other, but simply from the fact that there is then no gastric juice in the stomach for the water to mix with, as its secretion begins only with the ingestion of food: and, accordingly, experience shews that the copious potation of water soon *after* eating does injure digestion; and we think it probable that it so acts by then diluting the gastric juice secreted in consequence of the contact of food.

It has often struck us that writers on diet attend too little to the condition of the system at the time of eating, as an element in determining the kind and quantity of food which ought to be consumed; as the latter ought manifestly to vary according to the expenditure attending the age, sex, mode of life, and other circumstances. In Dr. Combe's work this principle was strongly enforced, as lying at the very foundation of all dietetic regulations; and, as we consider it to be of great practical importance, we are glad that it has been adopted by Dr. Paris.

"If there be one law," he says, "in the animal economy which, above every other, is irresistibly forced upon our attention, and which must command our unqualified dissent, it is this, that THE ADJUSTMENT OF SUPPLIES SHOULD ALWAYS BEAR A DIRECT RELATION TO THE WASTE AND GROWTH OF THE INDIVIDUAL; and yet it would be difficult to point out a principle so little regarded and so generally abused. Do we not daily see the adult indulging in an excess adapted only to the demands of

a rapid growth? the sedentary and indolent feeding like the active and laborious?" &c. (P. 69.)

We confess, however, that, after seeing the principle thus prominently brought forward at the outset of the new edition, we felt considerable disappointment on finding its applications to practice overlooked by few so much as by Dr. Paris himself.

One of the few additions of any interest in the volume before us occurs at page 91, where Dr. Paris gives a succinct account of the ingenious views of Dr. Schultz, of Berlin, regarding the *cæcum* being the seat of a second digestion, especially of vegetable food. Most of our readers are aware that, in some cases of artificial anus occurring near the termination of the small intestines, Dupuytren often observed vegetables pass out almost unchanged, while no trace of the animal food eaten along with them could be perceived. He thence inferred that the digestion of vegetable substances was completed in the large intestines; and the accuracy of his conclusions was to a great extent confirmed by the subsequent experiments of Dr. Beaumont, and by the fact of the unusual development of the colon in vegetable feeders, and its comparative smallness in carnivorous animals. Dr. Schultz, however, goes a step further, and maintains that the digestion of vegetable matter is completed in the *cæcum*; that it is there once more acidified, and that the office of the *cæcal valve* is to prevent its premature passage into the colon. Viridet affirms, in support of this view, that, in rabbits, he has found the food become again distinctly acid in the *cæcum*, after having been neutralized by the bile in the small intestines. We think this not unlikely to be correct; but must wait for further confirmation, before receiving Dr. Schultz's views as established.

Most of the other alterations in the present edition occur under the head of Indigestion, but they are neither numerous nor important. The most deserving of attention on the part of the practitioner is, perhaps, the description which Dr. Paris gives of a peculiar appearance of the tongue, consisting of "a dirty brown fur, principally occupying one-half of the tongue, longitudinally, from its root to its tip," which he believes "may be very generally received as evidence of cerebral disturbance. In paralysis it is frequently very striking; but, in morbid states of the nervous system, short of such mischief, I have noticed its presence in a less defined form, and have found the conclusion deduced from it subsequently confirmed by other symptoms." (P. 342.) We strongly recommend this to the verification of our readers, and are glad to have the means of aiding Dr. Paris in calling the attention of the profession to its occurrence. The remarks on the examination of the intestinal discharges are also very good.

While we have thus freely criticised the imperfections and omissions of the present edition of Dr. Paris's work, we must not be considered as on that account condemning it as a whole: on the contrary, we recommend it to the student as containing a great deal of useful information. From the announcement of its being nearly re-written, we were certainly entitled to expect a higher degree of improvement in its pages than they really present; but, notwithstanding this defect, it contains much that is both interesting and practically useful.

VII. The announcement of Mr. Curtis's book, and our knowledge of some of his previous writings, did not, we confess, prepare us to see a work of the character of that now before us proceeding from his hand. Knowing it to be impossible that the author of several of the volumes that have appeared under his name could compose an original work worth reading on the subject of the present, we were not even disposed to believe that he could compile such a work from the writings of others. We must admit, however, (if he is the author of the volume before us,) that he has really succeeded in doing so. How Dr. Combe may be pleased to find such wholesale use made of his admirable treatise we know not; but we think it probable that his benevolent feelings may be gratified to see many of the valuable truths contained in it diffused through a new class of readers in the pages of Mr. Curtis. For our own parts, we pass over without comment the incongruity of a frontispiece representing Mr. West and his family, copied from West's picture; and allusions to the author's self and family; and a Greek motto; and the mention of ear-inventions and eye-inventions; and advertisements of countless publications, and declarations of loyalty to George III., and other peculiarities, in consideration of the cheapness of the work, its small size, and, generally speaking, its sound sense. It is but justice to the author to admit that there are some few observations in it that are either new or put in a new light. Whether a good or a bad aurist and oculist, Mr. Curtis must be allowed to have had no small experience of mankind.

In the observations on the skin, and on respiration, (chiefly taken as usual from Dr. Combe,) the impropriety of closing bed-curtains is enforced by stating that "it is a remarkable fact that if a canary-bird be hung up in a cage at night at the head of a bed with close-drawn curtains, it will be found dead in the morning." (P. 22.)

We trust that some of Mr. Curtis's readers may be impressed with the undeniable truth which he advocates, and which parents too seldom remember, that "the first years of life should be directed to laying the foundations of health, which are the foundations of happiness." Throughout all plans of education it is, indeed, deeply to be regretted, that views of worldly prosperity, (to a certain extent no doubt commendable, and even necessary to many kinds of virtue), predominate over considerations which affect the character of the person to be educated.

We are always glad to meet with proofs that the health of the poorer classes is thought worthy of especial attention. Mr. Curtis mentions the baths in the City Road, at Lambeth, and Brighton, which are thrown open to the public at such low rates as to exclude few from the comfort of bathing. The subject is well worthy of the attention of medical practitioners in all towns of sufficient size to support such establishments.

Among several miscellaneous remarks, we find one on the subject of sending young persons to the continent to be educated; which Mr. Curtis thinks is done at the expense, in many instances, of health and happiness. There are, we hope, many exceptions to this observation; but that much evil is often incurred may readily be supposed; for several of the young persons sent abroad for education are the children of indifferently educated people, whose chief desire is that their sons and daughters should talk French. We entirely agree with Mr. Curtis in 'condensing' the toil of Sunday observances so common in schools. Early morning prayers,

repetitions of collects; morning sermon; afternoon lecture; serious readings; evening lecture again, and evening prayers, concur in many establishments to make Sunday no day of rest. On habits of early rising we need make no remark. The example of the royal family, adduced by Mr. Curtis, is not a very happy one. The poor king, (William IV.) whose early training Mr. Curtis thought bid fair to make him "exceed in length of life any of the previous kings of England," died a month after the publication of this loyal prophecy!

The remarks on Diet are almost entirely taken from Dr. Combe. When making reference to Temperance Societies, Mr. Curtis quotes from Grund's work on America the statement that, in 1833, the American Temperance Society consisted of two millions of members; and that two years before, when the number was much less, 1500 distilleries had been stopped, 1000 vessels sailed from American ports without ardent spirits as a part of their provision, and 4,500 drunkards had been reclaimed. He mentions, also, that Mr. Livesey, of Preston, informs him that upwards of thirty thousand persons, chiefly mechanics and artisans, have joined the temperance societies in Lancashire. We have always considered these societies as temporary expedients, not to be despised; but to be superseded by a general up-raising of the moral and intellectual condition of the working classes.

It is truly observed that there are many young men in the city who are engaged in sedentary occupations all the day, and yet, by riding to and from their offices in omnibuses, deprive themselves of the only opportunity they possess of taking exercise. In this respect Mr. Curtis thinks their elder brethren are wiser; and he recommends them to live two or three miles out of town, and to consider a plot of garden ground as an indispensable appendage to their dwellings; so that, becoming early risers, they may cultivate their gardens and improve their complexions. Some useful remarks are made on moderating the passions which actuate the busy, the ambitious, and the sentimental; and at page 76, Mr. Curtis favours us with a description of the symptoms of love, or of the disease of love in excess; and bad enough they seem to be:—

"The progress of the disease of which excessive love is productive, may be thus described: as the force of love prevails, sighs grow deeper, a tremor affects the heart and pulse, the countenance is alternately pale or red, the voice is suppressed in the fauces, the eyes grow dim, cold sweats break out, sleep absents itself, at least until the morning, the secretions become disturbed, and a loss of appetite, a hectic fever, melancholy, or perhaps madness, if not death, constitute the sad catastrophe." (P. 76.)

Mr. Curtis disputes an opinion of Mr. Mayo, given in the *Philosophy of Living*, that "the best instrument for conversation with a single person is the ear-trumpet with an elastic tube;" and he strongly recommends his own "acoustic chair," the person sitting in which "hears at the opposite side from that at which he is addressed." (P. 105.) He pronounces Mr. Mayo equally unfortunate in eulogising his (Mr. M.'s) invention of grey glass spectacles "of a pure black diluted;" and decidedly prefers his own "wire-gauze" ones, made convex; which have "received the approbation of his majesty, who has ordered two pairs; and of the Emperor of Russia, who has purchased three dozen pairs, and constantly uses them in his journeys over the plains of snow." (P. 107.) The only serious objection, he says, that he has heard against them is

that little profit attends the sale of them. Reflecting on these contrary opinions, we cannot consider that the public should be perplexed by the differences of doctors.

VIII. The work of M. Bureaud-Riofrey is on a less comprehensive subject than several of those already noticed. The subject, however, is of the very first importance. In an eloquent introduction, the author observes, that since the publication of Rousseau's *Emile* the physical education of women has been a subject treated with disdain, whereas the perfecting of the human race, he goes on to say, must commence by the physical perfection of women. He particularly regrets that of the treatises already written on the subject of woman, some are too learned to be read by the mothers of families, and others treat of subjects which can only be properly brought before medical readers. The public, thus left to themselves concerning the management of young women, have, he remarks, fallen into the hands of rhetoricians and fencing masters, who have applied the robust gymnastics of Sparta to the delicate women of modern cities. Their practices were indiscriminate, and they paid little attention, or none, to the influence of physical agents on the body, to the laws of life, the phenomena of growth, the distinction of constitutions, age, and customs. A new science has arisen, the orthopædic, of which the object is to prevent or correct the faults of conformation, chiefly of the muscles and bones, and mostly by mechanical means, resolvable into extension and forced repose. M. Bureaud-Riofrey, whose work is dated London, and who describes his practice as principally confined to the diseases of women, has very laudably, therefore, employed himself in preparing a work adapted to female readers, enforcing attention to those elements of physical education the study of which may not only prevent defects of osseous and muscular organization, but control with salutary effects the development of every organ which is subjected to the influence of education, or of the will. Like most works of this kind addressed to popular readers, it has not a few pages of extraneous matter, introductory and collateral; but the practical remarks which it contains are indicative of observation and judgment.

In proceeding to treat of the influence of external agents he first notices Air, Caloric, and Light. This last forms the subject of a distinct chapter. It is difficult to convince mothers of the importance of this agent, although perhaps not more difficult than to impress them with a sense of the importance of good air, to the health of their children. Dr. Milne Edwards's experiments on the development of tadpoles in the dark and in light are alluded to. An animal which in the first stage of its existence has the characters of a fish,—a tail, branchi, and no limbs; and in the second stage no character of the fish, no tail, no branchi, and four limbs,—afforded an excellent subject for these experiments; and it is well known that the results proved that these changes, if not absolutely prevented, were singularly retarded by the privation of light. The infrequency of deformity among the Chaymas, the Caribs, the Mexicans, and Peruvians, is ascribed by M. Humboldt (we think on very insufficient grounds,) to the free action of light over their bodies, rather than to any peculiarities by which their life is distinguished from that of the inhabitants of more civilized countries; and it is certainly probable that the use of what the French

call *insolation*, or free exposure of the body to the sun and the air, might, as suggested by Dr. Edwards, be advantageously had recourse to in scrofulous affections and other maladies of degeneration. It is presumed by some physiologists that the frequent and free exposure of the body to the atmospheric air, in the fine climate of Greece, tended to the development of the exquisite forms which yet live in ancient works of art. The traveller in Ireland, if acquainted with the condition of the poor in our large manufacturing towns, may usefully compare appearances resulting on the one hand from habitual residence in dark habitations, and on the other from almost constant exposure to sunshine and air. The manufacturer's younger children leave the cellar or the garret, but only for the narrow court or damp neglected back street; but the Irish cottager's child, although born in even greater poverty, runs about, as soon as it can run at all, in the open air of a temperate climate, not at all overburthened with clothing. The pale, unhealthy, scrofulous character of the manufacturer's child is too well known; whereas it is impossible to imagine finer examples of childish beauty and grace than are beheld at the doors of the Irish cabins.

M. Bureaud-Riofrey attaches importance to the chapters on hereditary and acquired constitutions, in which, however, there is little that is new. He refers to M. Pariset, who says, in his energetic manner, "Give me the roughest peasant, accustomed to the severity of the seasons, and I will make him delicate, nervous, susceptible, one whose corpulence, whose fresh-looking yet pasty countenance, (*visage fleuri, empaté*,) shall announce how his fibre is weak and distended, and the cellular tissue largely drenched with fluids. Give me, on the contrary, a slender citizen, pale-faced, of weak organization, soft contexture, whose body wants energy, whose mind is timid, and I will make him a soldier, a hunter, a sturdy sailor, whose tenacious fibre, compact bones, dense muscles, and iron arm, shall constitute a man intrepid in danger!" M. Pariset quotes the example of a certain Abbé Rucelleri, whose delicacy in all matters was excessive. His only drink was water, fetched from a distance. The sun, a calm, the least excess of heat or cold, affected his constitution. The very fear of being ill obliged him to take to his bed. The very idea of vapours, (we still quote from M. Pariset,) originated with this abbé, a malady without malady, which finds occupation for the indolent and profits those who treat it: oppressed by trifles, he shrunk from every form of fatigue and trouble: at length, spurred on by ambition, or by revenge, he entered the service of Catherine de Medicis; and overcoming his horror of labour, he became so robust and active, that his friends, seeing him at work all day long, and scorning repose at night, riding post on indifferent horses, and eating and drinking whatever came before him, asked him if he knew anything of the Abbé Rucelleri, saying they could not imagine what had become of him, or who had taken his place, or into what other body his soul had migrated.—The chief use of such examples is the encouragement they give, even in cases of strong hereditary predisposition, to persevere in that form of physical education which thus promises to overcome it.

In the chapters on Growth and Alimentation we observe many excellent observations; and these are followed by several chapters devoted to subjects connected with the exercise of the body, the importance of which

fully justifies the author in devoting so much space to it. It would, indeed, be difficult to exaggerate the value of that which appears to determine blood, nutrition, nervous energy, vitality, into any organ, or into all, according to its application; and which must consequently when neglected or attended to, materially influence the development of the body and of the mind in the years of growth. Erroneous notions on the subject of female beauty still lead to customs subversive alike of natural beauty and of health. Even the calisthenic exercises, which now may be said to form a regular part of female education, cannot avert the evils arising from the stiff and imprisoning corset: and, managed as they are in many schools, these very exercises are productive of as much evil as good, if not more. It can scarcely be doubted that the more violent exercises, the forced positions, and unnatural and ungraceful attitudes, are in every way unfitted to the female organization. A lover does not admire the hard muscles of a pugilist, nor masculine carriage and demeanour, but the soft and rounded forms natural to the female figure. A rational man does not desire that his wife should be a rope-dancer, a balancer, a climber of rigging, or a performer of summersets over a bar, but a mother. Neither can the finishing school do better for him: for its aims are uniformity and insipidity, and an apathetic mind; and he requires a friend and a companion.

M. Bureaud-Riofrey does not carry his admiration of gymnastic measures to the excess of considering them in every case sufficient for the cure of muscular imperfections, which, he justly observes, require also the use of tonics, good diet, salubrious air, baths, frictions, shampooing, &c., which the physician must prescribe. His general principle is, that the weakest muscles ought, in cases of inequality of strength in the two sides, to be the most exercised. Several methods of doing this are mentioned. We must confess a degree of apprehension concerning some of these, such as balancing vases or boards on the head, at least in the way practised in some schools, in which young ladies while occupied in drawing or music are required to balance a board upon their heads. The object in these cases is to produce that uprightness and even stiffness of carriage which is after all unsightly; whilst the torment of the means employed is in some irritable constitutions very serious, and the inconvenience must always much impede the progress of the pupil in what it is professed to teach her. If we do not go so far as to say that we have seldom seen unequal developments of muscles, once formed, entirely removed or manifestly checked by any means employed; we have at least often witnessed evil consequences from forcible efforts to depress shoulders, to flatten prominent chests, and to keep heads erect. But the prevention of such deformities is another matter, and may reasonably be hoped from the employment of a judicious hygiène.

If it can be justly said that the perfect development of any one part of the body is more important than that of the rest, it must be said of that of the chest. The best organized brain must even possess a limited range of exercise if there is not an ample chest in which the blood may receive the full benefit of the atmospheric air, and the nutritious fluid commingled with it undergo perfect assimilation. Unfortunately, as is too well known, this is the part of the body which is subjected to the most pernicious constraint, or if the same constraint is extended, it is to the epigastrium and

hypochondria, where the most important organs of digestion are situated. We suspect M. Bureaud-Riofrey of a wish to appear singular when we find him defending the use of stays, and denying their bad effects. In a previous chapter he counsels their being disused during gymnastic exercises; yet he considers that they are useful to weak persons on the principle of a laced stocking. The same argument might be employed to recommend the universal use of laced stockings. It is surely inconsistent to recommend them as strengthening the muscles, and yet to discard them during exercises which call for more than usual exertions of muscular strength.

That which gives peculiar interest to the hygiène of young women is, that the observance or neglect of its rules may have a serious influence upon them at the age of puberty, when certain organs peculiar to the system of the female, and on the healthy development of which all her future health and comfort may be said to depend, are called, as it were, into existence. The general effort observable at this period in the female constitution, the uncertain direction of this effort, and the importance of its right direction, render everything in the management of a young person material. In great cities, M. Bureaud-Riofrey observes, the young woman arrives at puberty by the mind, by the imagination and the desires of the heart, long before actual puberty has taken place, and hence arise the disorder and struggle between the sensibility and the organization, and all those peculiarities which make this period of life, in the higher ranks, a period of disorder and suffering. Nothing, he adds, making use of the expressions of M. Georget, nothing is spared to obtain for them the fatal gift of sensibility; inactivity of the muscular system, the cultivation of music, society, balls, plays, mental indolence, books which excite the feelings, and give visionary pictures of life. We must remark, however, that whatever evils this management may produce, sensibility has little to do with producing them, although selfishness, which is the only feeling really nursed by such a system, may have a great deal to do with them. Such sensitive young ladies perform the finest compositions without understanding them, mix in society without powers of conversation, dance without enjoyment, see plays without feeling the charms of poetry or of acting, and read almost without amusement. And thus it results, that notwithstanding the spread of education, the diffusion of knowledge, and all that excites the enthusiasm of some and the fears of others, nothing is more rare than an intellectual young woman.

M. Bureaud-Riofrey has described the dangers, physical, moral, and mental, of the period of puberty in glowing language, but with strict truth, making some allowance for the nation in which his observation has probably been the most exercised. The pages he has devoted to this part of his subject, and the short chapter on beauty, in which he claims for medicine, with Descartes, the prospective honour of contributing largely to perfecting the human race, will, we hope, recommend his hygiénic precepts to many who would be insensible to higher arguments. And although the medical reader will find his book verbose, and often too diffuse for professional reading, he will not fail to meet in the course of the author's remarks on the phenomena of growth and puberty, and on the care required to conduct these phenomena successfully, many acute and valuable observations, indicative of a reflecting and philosophical mind.

IX. Dr. Caleb Ticknor's work, (whose title, by the way, Mr. Mayo has laid violent hands on,) has greatly interested us. Its general subject is, to be sure, only that of the works we have already had to notice; but its application to the citizens of the United States, and the glimpses it affords of the manners and mind of a people considered to enjoy a fuller share of free agency than is allowed or avowed in the older countries of the earth, impart to it a variety which makes us forget the sameness of the details to which we are accustomed in writings of this kind. Its very commencement is exciting.

"The author can assign no other cause for preparing this volume than the presence and universal prevalence in this country of a malady, an epidemic, the like of which was never before witnessed, sparing neither age, sex, nor condition, and being followed by the most unhappy consequences. If the reader ask the name of this disease, he may be told that it is a sort of mania, fanaticism, or ultraism; if he ask where, and in what it may be seen, he may be answered, in all places and in all things. It is seen in most of the charitable and benevolent operations of the day; in religious zeal, political warfare, morality and immorality; in most of the domestic concerns of life, and, in fact, in all the particulars and minutiae of living, moving, and being. There seems a remarkable propensity in us Americans to run into unwarrantable extravagances: whatever scheme is adopted, or whatever plan devised, whether for good or evil, is carried to an extreme. To one who contemplates the present condition of our country with calmness and deliberation, everything would seem to be upside down, or in a state of the most perfect confusion. He would see men running into opposite extremes on all subjects, and man warring to the death with his brother or neighbour on some trivial question, while they are no better agreed on matters of the greatest moment. To judge of men by their actions, one would suppose that a great proportion were mad, and that the world was one immense madhouse. Retrenchment and self-mortification seem to be the order of the day in relation to food and drink; there being no virtue, on the principle of radicalism, which does not consist in going counter to the appetites and instincts of nature. 'Let us be temperate in our meats and drinks,' says one, 'and use the world as not abusing it.' 'No,' says another; 'but let us rather cut no meat while the world stands; and, as to drink, let that be cold water.' Such sentiments have been put forward on the subject of diet, and such ultra measures urged, that the very injury is caused which is attempted to be averted, — to wit, ill health and consequent unhappiness.

"The mind cannot thus continue in a feverish state of excitement; but, agreeably to the operations of nature's laws, it must pass into a diametrically opposite state, — into a state of depression proportioned to the previous excitement. We may, therefore, confidently anticipate a reaction; and, without some signal interposition, I verily believe that, within ten years, infidelity and apathy will bear a more triumphant sway in this country than has ever yet been known; and far less will be done in the benevolent operations which characterize this day than if our zeal had been tempered with more discretion." (*Pref.*)

Although the whole of Dr. Ticknor's volume is written with great liveliness, and contains much matter for observation, we must confine our attention almost entirely to points presenting some peculiarity arising out of the author's national position. Thus, we should leave the chapter on Diet untouched, but for the necessity under which Dr. Ticknor finds himself of combating one of the prevalent fanaticisms of America, in the shape of an "anti-animal" system of living. One Solyman Browne did, it appears, in 1833, publish a poem in New York, whereunto certain notes were appended by "E. Parmely, dentist;" and these notes were of a "truly vegetable, antiphlogistic character," setting forth how he, the said Parmely, having suffered much from a gross and improper diet, reformed

his living, excluded all animal food and strong drinks, and was restored to health. This, Dr. Ticknor remarks, was all very well. Mr. Parmely led a sedentary life, and the abstemious plan, although suitable to him, was not on that account fit for every man in New York. Nor should he vainly quote Dr. Lambe, and assert that "an adherence to the use of animal food is no more than a persistence in the gross customs of savage life, and evinces an insensibility to the progress of reason, and to the operation of intellectual improvement." Dr. Ticknor very ably refutes these fancies, by an appeal to Combe and various other writers, and, above all, to experience and observation; pointing out, by reference to numerous vegetable-feeding nations, how little the absence of beef benefits the "progress of reason." Wishing to look upon this question without prejudice, he observes that the Irishman living on potatoes, and the Scotchman living on oaten cake, are not far behind their English neighbours, who, even at the worst, use so much more animal food; and we do not doubt Dr. Ticknor's assertion that "the slaves in the Southern States, with their usual allowance of a peck of corn each week, enjoy better health than their masters, with all the luxury that wealth can procure." The fact seems to be, that life and strength *may* be supported, to a certain extent, in any climate, upon any kind of food; but that the structure of man, his instinctive tastes, and the necessities induced by various climates, are all opposed to a fanatical abstemiousness wherever mixed food can be easily obtained. We admire the stern good sense with which Dr. Ticknor puts aside the dreams of dyspeptic writers, who would make all the world live by rule; of the folly of which he gives some pleasant examples:

"A young man commences study: he enters college, studies hard; his professor lectures him into sixteen ounces of food each day; to walk half a mile, fifteen rods, and three paces, just forty-three minutes after each meal; and to sleep six hours, thirty-three minutes, and forty-two seconds, every night. The consequence is, that the student, for a short time, makes rapid progress in his studies, and gives promise of future eminence; but soon he becomes pale, thin, and debilitated: he has dyspepsy, and he is dieted yet more; he is now deprived of all solid food; slops and gruel are his only allowance; and he finally adds another to the catalogue of singular dispensations. This is no overwrought picture, but a common, and quite too common, occurrence.

"It is strange to see a man tormenting his friends, and making himself miserable, by his unceasing and unavailing complaints, and his overweening anxiety about what he puts into his dear stomach. To-day he must have a little bran-bread, to-morrow a soda biscuit with a little milk, next a little rice and sugar, then rice and molasses; one day he is filled with wind, another with acid, and very often with whims and nonsense." (P. 48.)

We might accumulate proofs such as these of our American brethren having exhibited a great precocity of refinement. They pamper or starve themselves as fashion dictates; reject the regular doctors for quacks, who administer steam, red pepper, lobelia, and other articles of incendiary practice; and, in short, promise fair to imitate all the fashionable follies of older communities. In what relates to the happiness of life, physical, moral, and intellectual, we shall see, as we proceed, that republican institutions have yet done little for them. The causes of this it is not for us to consider, but we do not think that they lie very deep.

The probity of bakers is not greater in transatlantic regions than in

London itself. "The New York bakers will not use the Western flour, because, although it contains more starch, the chief aliment of bread, and is consequently more nutritious, yet it contains less of gluten, and, consequently, the baker is not able to raise so large a loaf from a given quantity of flour." So they use one eighth of sour flour, to make the dough rise, and the people of New York eat bad bread. The bad loaf swells into saleable size, and suits the baker better; but it swells after it is eaten, and distends the citizens with gaseous dyspepsia. But a righteous man, one Graham, makes what he calls "the staff of life," which is only bran bread, or bread made of unbolted flour, of which the said Graham asserts himself the inventor; and Graham's staff of life is all the fashion. Let us see whether the chapter on Drinks, affords equal proofs of advancing civilization.

Combating the abstemious fanatics of his country so effectively on the ground of animal diet, we were surprised to find Dr. Ticknor maintaining that because all animals except man can preserve health by drinking water, therefore man should drink nothing better, at any time, in any climate, at any age, in any circumstances or mood of mind. So used to teach our venerated Gregory; stoutly maintaining that wine was no more necessary to a Christian than to a Mahometan, nor to a man than to a horse; but we demurred to this, even at the time, being previously imbued with the milder doctrines of the ancients.

Philosophically considered, there may be some necessary connexion between stimulant liquors and the other stimuli peculiar to a creature of the highest intelligence and sensibility. The degradation of reason by their abuse is no argument against their use. The stimulus of many of the tender and nobler affections is equally unknown to the lower animals, but man is not on this account to discard them as unnatural to him. Man is often their slave; but his reason was given to him to rescue him from such slavery, and also from the practice of intoxicating himself with ardent spirits. The abuse of spirits has, however, been carried to such a frightful extent in America, that we cannot be surprised to find several well-wishers of their country desirous of seeing their use utterly abolished. Dr. Ticknor subsequently acknowledges that a glass of wine may now and then be taken without doing any irreparable mischief: he evidently disapproves of the enthusiastic extremes of some of the temperance people; and, although he wishes "that every man were a *cold water* man," thinks that a moderate use of artificial stimuli may be permitted to old people. His good sense has an evident struggle with a favorite theory.

To those acquainted with the nauseous habits arising out of the custom of smoking, and of which almost all travellers in America have recorded their disgust, it will give pleasure, but no surprise, to learn that Dr. Ticknor devotes a chapter to The Great Pleasure and Benefit of Using Tobacco; in which after reciting its history, and relating instances of its poisonous effects, he censures with effective irony the customs of smoking, snuff-taking, and chewing, pointing out the insensibility to smell which these induce, the ruin of the voice, the deterioration of the breath, and of the manners. From these habits, and these consequences, even the ladies do not appear to be free.

"Could any one," he says, "entirely unacquainted with the unaccountable habits and propensities of man, after knowing the properties of tobacco, be made to believe that half the adult inhabitants of America are passionately addicted to its use? And were he told that America's fairest daughters use it too, would he not be perfectly incredulous? And were he told, further, that ladies of the greatest respectability, the most genteel and accomplished in one of our largest cities, carry their little jars or boxes of snuff into the social circle, and, with a delicate ivory spoon, feed their *sweet* mouths with this most delicious and agreeable poison, would he not be petrified with astonishment! How delightful it must be to have an amiable spouse rendered doubly *sweet*, and bewitchingly interesting, and most charmingly stupid and idiotic, by the constant practice of *chewing snuff*; and what a fine example to children, of the pure, cultivated taste and self-control of the mother!" (P. 112.)

On the subject of Dress we do not find that the fair Americans are in any degree behind or before Europeans. They lace with equal severity, and even with more system and perseverance than our countrywomen. By way of opposing this hurtful fashion, Dr. Ticknor is at much pains to teach the uses of respiration, and the impediment of that important function created by tight-lacing. He appeals to the Venus de Medicis, introduces the well-known outlines of her figure and that of a modern belle, both dressed and undressed, and tries to make an impression on the American ladies, whom, in Transatlantic fashion he calls *females*, by telling them that if they would only be quiet and lace not, a great proportion of them "would closely approximate, if not quite equal, the divine standard." He takes scarcely less pains to convince them that scanty or insufficient clothing is full of danger to the health. The case is evidently desperate; for, in obedience to fashion, the ladies "defy physical laws, breathe freely in armour, and are warm in gossamer."

"But a short time has elapsed since it was considered unfashionable, or ungenteel, to wear any other than cloth shoes, with a very thin single sole, even in the most severe winter weather. And what is very singular, no lady was ever known to complain of cold feet—they were *always warm, quite warm, not the least uncomfortable*: were we to give full credence to the devotees of fashion, we should never find health injured, or comfort abridged, by any custom, however rigid, or however absurd. The miss in her teens, her mother of forty, or her grandmother of sixty, though so tightly laced as to perform but half a respiration, will tell you, and insist upon it, they are so loosely dressed that their clothes will hardly stay on, and that they breathe with the most perfect freedom. A lady in midwinter, with open-worked stockings, and only a pair of thin cloth shoes, will most unhesitatingly say, that her feet are never cold: and if her neck is at the same time bare, will say that she feels no inconvenience, that she is indeed very warm." (P. 147.)

The chapter on the Management of Young Children teaches us that the Americans put a tight bandage round the bodies of infants, leave the arms of boys and girls bare, dress children of all ages very lightly, and dose them with paregoric. The practice of giving them stimulants is nearly obsolete; but Dr. Ticknor says, "not long since I met with an old fashioned mother, who wondered that her baby grew so very slowly and looked so puny, for she fed him with *gin-sling* two or three times every day." It seems, indeed, that until very recently, many very pious and good women thought it impossible to rear children without giving them milk-punch. Both mothers and nurses continue to ply children with food, with the admirable assiduity so observable in the nurseries of the mother country. He very justly observes that infants often cry from

thirst, which neither food nor the breast will relieve, but which a little cold water will entirely allay.

We learn from the chapter on Amusements, that the theatres of America present a combination of the fashionable follies of the opera of London with the disgusting scenes exhibited at our third and fourth-rate theatres; and that the public taste seems rather to become more degraded than to improve. Indecent exhibitions, immoral sentiments, incorrect company, drinking, smoking, rude conversation, and late hours, are no less common to American theatrical amusements than to those of England, which in these respects are far more offensive than those of the continent. Nor have the Americans the good sense to begin dancing at their balls before the hour of sleep; whilst the ladies, on such occasions more tightly laced than ever, sometimes expire in the midst of a dance.

These follies are fully accounted for in the chapter on Education; by which we learn that the young are confined for many hours to all the old monotonies of unreformed schools, now almost unknown in England; sit on hard and backless benches for six hours out of twenty-four; and whilst the boys are urged and stimulated to all acquisitions which can subserve the purposes of ambition or of mammon, the girls conclude what is called their education at seventeen, and sit down, frivolous, and fine, and uncompanionable for life. The consequences may be plainly seen, as depicted in the works of Dr. Brigham and of Dr. Ticknor. An ill-regulated, because excessive, pursuit of worldly objects, associated with a fanatical and exciting religion, exhibits its effects on the minds of the American population to a very fearful extent. We trust the writings of the several able physicians of that great nation, who have directed their attention to the subject of hygiene, will awaken the energetic communities in which they dwell to their truest interest and highest wisdom; and that among the proud distinctions of a country, to which no friend of mankind can look without the deepest interest, will soon be that of having carried into practical effect the *Philosophy of Living*.

It was not our intention in the foregoing pages to give a regular account of any of the works we have noticed; which would have required more space than could with propriety have been allotted to a subject not new to medical men, and chiefly concerning unprofessional persons: but we did not think it proper to pass by without notice publications of which the general object was excellent, (for such may be said of all,) and which we believe calculated to do essential public service. The medical observer, viewing the errors of mankind without passion, and knowing how many are inherited, how many inherent in organization, how many the result of unhappy circumstances, becomes a lenient judge of his fellow-creatures, and above all things characterized by his humanity. Well knowing that one of the first and most constant conditions for remedying or preventing these social and moral evils is to preserve the integrity of the physical frame, he can never view with indifference any attempts to instruct mankind concerning the means of preserving that perfect health without which, it may be said without hyperbole, they can neither be fully useful or morally good, or transmit good dispositions to those who are to come after them.

ART. VI.

Surgical Observations on Tumours; with Cases and Operations. By JOHN C. WARREN, M.D., Professor of Anatomy and Surgery in Harvard University, and Surgeon of the Massachusetts General Hospital.—*Boston*, 1837. 8vo. pp. 607.

NOTHING is more apt to produce a favorable impression of any literary production than to find that the author has justly appreciated his own labours, and has offered them to the public under an appropriate title, and without immoderate pretensions. A prepossession is thus created in favour of his good sense, and the reader is prepared to listen with attention to one who is able to decide fairly in a matter in which the natural bias of self-approbation is frequently too strong for sober judgment. In the work before us, these first favorable impressions do not lead to disappointment. "It is not," we are told, "to be considered as a treatise comprehending all that is known on the subject, but rather as a collection of cases intended to illustrate the distinctions between different tumours." (*Pref.*, p. vi.) Now, although a collection of this kind falls short, both in the labour of compilation and in the information conveyed, of those completer works which trace out and explain in a systematic narrative the beginning, the progress, and termination of a class of disorders, it nevertheless possesses great utility, is an easy and natural mode of delivering the results of observation, and is sanctioned by the authority and example of the best practical writers of our country.

The cases which we find reported in modern medical writings are chiefly of two kinds: the one, formed on the model of the French school, and carried to the highest degree of perfection by Professor Louis, consists of a collection of the most minute particulars, whether connected or unconnected with the principal disorder; the other, which seems especially to flourish in English soil, characterized by a selection of the more prominent features which distinguish the disease, and which appear to afford some direct clue to the means of palliation or of cure. The former method, from which all hypotheses and inferences are professedly excluded, is no doubt highly philosophical, and capable, when carried out with the zeal and talents which have been bestowed upon it, of eliciting valuable truths; the latter, by its more direct tendency to practical conclusions, and the fixing of the attention on those points on which questions of interest appear to turn, is more attractive to minds accustomed to reason while they observe, and make utility and applicable knowledge the prime objects of attainment. To this last class belong the cases contained in the present volume: clear, simple, and graphic, they bear the unaffected impress of truth, and report with manifest candour and honesty, as medical writings ought, the opinions that influenced the treatment and the motives that guided the surgeon in performing or abstaining from operations. In fact, throughout his work the author proves himself to be a worthy disciple of the school in which he received his early instructions, and which, connected as it is with the names and celebrity of Cooper, Bright, and others of hardly less eminence, he seems proud to acknowledge as his alma mater: and surely that school need be no less proud to claim him as a pupil; as we believe there are few members of

our profession who are more entitled to the consideration of his brethren than Dr. Warren. Distinguished as a zealous and successful author and teacher for the last thirty years, foremost in every undertaking calculated to improve medical literature and science, and never weary in the cause of benevolence, he has long earned for himself, in his native country, a reputation of the most enviable kind, and to which mere literary distinction, more especially in foreign countries, can add but little of value. It gives us, however, sincere pleasure to have this opportunity of publicly expressing our sense of Dr. Warren's great merits both as a public and private character, and as a member of our profession.

The frequent occurrence of tumours in every texture of the body renders an acquaintance with them of great importance. The wide difference which exists among them,—the small size, the stationary condition, the harmless and inoffensive nature of some, the magnitude, the rapid progress and the destructive tendency of others, demand an exact discrimination; and a correct diagnosis becomes the more essential as the life of a patient may in some instances depend upon a timely interference; and as the disease—in most cases open to the view,—remains, both before and after death, a permanent token of the sound or incorrect opinion of the surgeon. But, however desirable it may be to distinguish different tumours from each other, to appreciate the degree of danger attendant upon each, and to know whether an operation will be permanently beneficial; the present state of our knowledge, notwithstanding the pains already bestowed upon the subject, does not enable us in all cases to attain this degree of certainty. Ever since the term *malignant* was introduced into surgical nomenclature, as expressing a specific character, it has been a most interesting subject of speculation whether a tumour possess this quality or not; and, indeed, if we take as the grand feature of this class an invariable and irremediable tendency to the destruction of life, the question becomes one of the most serious that a surgeon can be called upon to decide: and yet how greatly have we to regret the uncertainty and confusion that prevail on this point, and how frequently do we find that the most able practical men are altogether at variance, not, indeed, respecting names and definitions merely, or minute and speculative distinctions, but on points of the utmost importance,—the propriety or impropriety of performing severe operations! If the same tumour, even after its removal, be presented to different pathologists, it is not uncommon for one to declare that it is malignant; a second, that it is non-malignant; a third, that its malignity is excessive; a fourth, that it possesses a low degree of malignity; a fifth, that, although it is not yet malignant, at some future time, sooner or later, it may, or it certainly will become so. In cases of this kind, the question which the patient asks is this: “Do you advise me to submit to an operation?” To this it is a rational and no unphilosophical answer to say, “If you do not, this tumour may perchance cause your death.” Such, however, is not the reply which a surgeon or pathologist would desire to give: it conveys the acknowledgment of an imperfect acquaintance with the disease. Therefore, while we laud the candour which does not profess greater knowledge than is really possessed, we think it would only be just, at the same time, to admit an equal ignorance as to whether the performance of the operation, malignancy being supposed, will at all retard the fatal period;

whether, in fact, the disease be so far local as to receive a material check by the eradication of what has been already developed; or whether it be so far constitutional as to make its progressive advances, notwithstanding its apparent removal, as rapid as if remedial measures had not been adopted. This is a question to settle which requires an improved knowledge of the essential nature of such tumours, and also extensive experience of the effects of operations and of the uninterrupted course of these complaints. We cordially join Dr. Warren in the hope that the investigations of Sir Astley Cooper, whose aptitude and opportunities for the task exceed that of any other man, will, ere long, throw light upon a subject at once so important and so obscure. The two following extracts from the work before us bear on this difficult point: the first relates to scirrhus of the breast; the second to fungus of the testicle.

"Experience has satisfied me that many cases may be cured by operation; according to the best of my information, I should say that one case in three had been cured without relapse. When, therefore, the tumour is not large, has not been of long standing, is not ulcerated, and there are no signs of constitutional affection, it is best to try an experiment, which, notwithstanding its painfulness, is generally desirable and satisfactory to the patient. I wish I could say anything more definite on so important a point, but I am not at present able to do so." (P. 278.) . . . "The statement of these cases will give you an idea of the disposition there is to constitutional disease in the fungoid testis. Whether this constitutional derangement precedes or is consequent on the local malignity, we cannot certainly determine, because we cannot examine the internal organs in an early stage: the common opinion is, that the evil begins in the local affection. Until this point shall be decided in a way contrary to this opinion, it must be considered a matter of the greatest consequence to extirpate the testicle at an early period. But how are we to know whether the disease is of a malignant character before malignant symptoms arise? Simple chronic inflammation, scrofulous and venereal inflammation, may produce appearances similar to the early stage of the fungoid disease. How, then, are we to discriminate the fatal disorganization in season to arrest its transmission to the great organs? It must be admitted that this discrimination is often impossible in the early stage. We must be careful to study the history of the case, and satisfy ourselves that there is not a scrofulous, syphilitic, nor sympathetic action going on in the part. We must enquire if the patient has suffered much pain, whether the functions of the digestive and pulmonary organs are disturbed; we must notice whether the tumour is irregular and comparatively hard. In the case where these symptoms shew themselves, we are not to wait. Better is it to remove a number of testicles without absolute necessity than to allow one to proceed beyond the reach of art. The scrofulous, and even the venereal inflammation, when they have continued long, produce a disorganization from which it is impossible to recover. The organ, in such instances, is of no use; it is but a name; and though men are naturally tenacious of preserving it even in this unsubstantial state, they would not desire to extend the experiment long enough to endanger life. When they are thus determined, it is better that the patient should take this responsibility on himself; and therefore, without exciting unnecessary fears, I should fairly state to him the dangers he has to encounter and let him decide the question.

"Even the scrofulous testis in an ulcerated condition is better removed; for, although the healing of such ulceration is often accomplished, the texture of the part is broken down, as already stated, and its functions lost in many cases. The cure is long, and causes much pain and embarrassment. I have known, and have at this time in my care, a number of patients who have had chronic enlargement of the testicle for many years. I do not propose to remove such tumours, for the following reasons: because the patient suffers no pain, because his health is unimpaired, because the disease does not advance, and because I can trace the origin of these tumours to some satisfactory cause not connected with the character of malignity." (P. 335.)

Whilst, in the main, we consider this to be the statement of a judicious

and discriminating practitioner, we cannot allow it to pass without deprecating a mode of expression calculated to lead to the reckless performance of operations. We cannot for a moment suppose that a surgeon of Dr. Warren's experience would undertake the removal of a testicle that was not affected with a disease which rendered it either dangerous or useless; but we look with much apprehension on the establishment of such a maxim as that "*it is better to remove a number of testicles without absolute necessity than to allow one to proceed beyond the reach of art,*" lest it should afford a cloak to the ignorance of inattentive or superficial observers. This view of the eligibility of an operation depends on the probability of the disease being malignant; and though it be true that, in the early stages of the disease in question, our present knowledge affords no clearer indication than can be derived from what may happen, or is likely to happen, still the want of certain information should inspire us with extreme caution, and render us unwilling to perform an operation which may subject us to a charge of useless or unnecessary interference. At the same time, it is hardly to be admitted that the total inutility of the organ is quite so probable a consequence of the chronic, scrofulous, or syphilitic inflammation as the preceding account requires us to suppose. The following extracts from Sir Astley Cooper's work on the testis will set this point in a clearer light.

"When I make a section of a chronic enlargement of the testis, throw it into water and agitate it, a whitish yellow fluid proceeds from the seminiferous tubes, which are extremely dilated and which then appear emptied. But still the same bulk of testicle remains, owing to the cellular membrane of the part being loaded with a yellowish fibrine or coagulable lymph; the rete is filled with the same secretion as the tubuli; the epididymis is similarly diseased, and sometimes the vesiculæ seminales and vasa deferentia are distended with a similar morbid secretion. But the effusion whether placed in one situation or in the other, when it becomes absorbed by proper treatment, may and apparently does leave the testicle capable of performing its functions, and allows therefore of its complete recovery." (*On the Testis*, p. 37.)

"At length [after suppuration] the testes diminish, form but a small quantity of semen, and they continue to waste until but a small portion of them remains and their secretion almost entirely ceases. Mr. S. had a scrofulous disease in each epididymis and both testes wasted. Only two or three drops of their fluid issued under an unnatural excited emission. He had erections and occasionally a desire for sexual intercourse but had no nocturnal emissions. He had the disease four years; and a sinus from each epididymis still discharges a quantity of fluid which stiffens the linen." (*Ibid.* p. 98.)

"When the venereal poison affects the testicle, it probably attacks the tendinous structure, for example the tunica albuginea; and from thence extends into its interior fibrous and not its tubular part; but this I allow to be hypothetical, and am led to that opinion from the structure of that part most resembling the periosteum in its tendinous composition and from the very ready and complete recovery of the organ." (*Ibid.* p. 104.)

When, therefore, we regard the specific function of the testicle, and consider it as an organ giving rise to numerous important sympathies, the tenacity with which men commonly cling to the preservation of it is neither unnatural nor excessive; and we cannot allow that, before the loss of its power or the danger of the complaint be placed beyond doubt, the surgeon is justified in performing extirpation.

There has always been the greatest difficulty in accounting for the origin of tumours: we cannot say that, in the following observations,

Dr. Warren has removed much of the obscurity in which the subject is involved.

“What is the cause of tumours? An individual making a violent effort of some muscle has a tumour formed afterwards in the muscle, and we say the tumour is caused by the strain; that is, the strain changes the healthy action of the capillary exhalants of the part, so that, instead of their usual deposit, they form a morbid substance: or he may receive a blow on the testicle, and a disease succeeding, we say the blow is the cause. A female has a mammary abscess which leaves a chronic induration, and, when she reaches a particular period of life, this assuming the character of a scirrhus, we say the induration is the cause. But is it not notorious that similar accidents occur constantly, and are not usually followed by such results? Of course, it is impossible that those injuries are the simple causes of morbid actions generated in such cases. We must look for accessory causes, such as some peculiarity in the accident or in the individual. The former is inconceivable; and we are therefore compelled to suppose a peculiarity in the constitution of the individual. Such peculiarity may sometimes be fairly suspected, as in the case of a female one of whose parents had been cancerous; or in a man who had abused his health by the use of ardent spirits: but how many instances remain where no such cause can be seen; and how many cases are there where no cause, either predisposing or exciting, comes within the cognizance of our observation! Mr. Hunter suggested the notion that a drop of blood, being accidentally effused, became organized, and assumed a growth independent of the system, and continued to grow till it was limited by some obstacle opposed to it. The objection already instanced has been offered to this opinion,—namely, that effusions of blood take place continually without any such consequence.” (P. 4.)

“Effusion of lymph, or coagulable lymph, from the capillary vessels, has been considered as a possible cause of the origin of tumours. The lymphatic effusion must of course become organized, and thus constitute a new-formed substance: such effusions are common after accidents and during diseases. We notice them in the cellular texture, in the muscles, in the serous cavities. In the first two of these textures they more rarely become organized, but are soon taken up by the absorbents and disappear; in the last, organization occurs in a great number of instances, but does not produce a tumour. The effusion of lymph, therefore, is not necessarily followed by tumour, even when it becomes organized; yet that some tumours are generated in this way I have no doubt. A third supposable mode of production of tumours is by chronic inflammation of a natural texture. This chronic inflammation happens most frequently in parts disposed to inflame, as the cellular texture. Steatomatous tumours are caused in females, in the cellular texture, on the upper part of the deltoid muscle, by the pressure of their dress, with a frequency that is notable. The greater number of tumours being formed without any such accidents, so far as we know, this chronic inflammation, if admitted to be a cause, is only a partial or occasional one. On the whole, therefore, it must be understood that we have but a very imperfect knowledge of the predisposing and exciting causes of these derangements of organization.” (P. 6.)

The list of causes here enumerated cannot be considered as complete; for (not to mention hernial tumours, which, as not being morbid growths, do not fall under notice in this volume,) we find no allusion to parasitical animals forming hydatid tumours, nor to the development of cysts supposed by some to include a numerous and important division, nor to the obstruction of ducts, which as a cause of tumours is far from hypothetical; nor, finally, to the alteration and distension of the coats of vessels, giving rise to the interesting class of aneurismal tumours.

In the present work an exact or scientific arrangement has not been attempted. The plan adopted is that of presenting the different tumours under the head of the different textures of the body, as far as could be done. The author treats first of those which are the most superficial, most easy of inspection, and the affections of which are most simple.

Beginning with the textures of the skin, he next takes those of the cellular membrane; then the muscular, the fibrous, and the osseous. After these most obvious and extensive textures, he places the numerous tumours of the glandular texture; next those of the vascular; and then those of the membranous textures, mucous, serous, fibrous, and synovial. Finally, he notices tumours composed of different textures and not found exclusively in any,—the encysted tumours.

Our space will not permit us to follow the author through the details of this extensive plan: nor do we find a sufficient amount of novelty to tempt us to do so. The cases, however, are all exceedingly illustrative; and we proceed to extract one or two examples which bear upon particular objects of interest. The first we select is one of scirrhus of the breast, and is principally remarkable on account of its termination.

“The recent specimen we have now before us is one you saw amputated, about a month since, by Dr. Hayward. It has some peculiarities: the scirrhus texture involves every part of the breast; the glandular structure and the intervening cellular texture is lost in the new-formed granulated white scirrhus effusion; it is remarkably hard: yet this disease had not been long in coming to the state you see. The patient was a well-formed woman, of thirty-five, rather delicate in appearance, and had never been robust. About a year since she discovered a lump in the right breast, which at first was not painful, but had become so lately, and she had sometimes required opiates at night. The monthly evacuation had been regular to the time she entered the hospital. Two of her father’s sisters had been affected with cancer of the breast, and had undergone operations. The disease, so far as she knew, did not return in them; as one died of consumption twenty years after operation, and the other is still living.—This patient, Ann Elkins, went through the operation well, and the following week passed without any unfavorable symptoms, excepting some degree of difficult breathing. In eight or ten days this difficulty increased; the pulse became very quick, the abdomen tumid and painful; and, after three or four days’ suffering, she died unexpectedly. A termination so sudden and unlooked for led to a careful examination of the body after death, and the following appearances were noticed:—External appearance of the body natural, excepting some tumefaction of the abdomen. On opening the cavity of the thorax, the lungs were adherent to the parietes very closely on the right side, and slightly on the left. The non-adherent surfaces were reddened, and uniformly covered with a thin fresh layer of lymph. The surface of the pleura exhibited some irregularities, as if there were tubercles within: this appearance was found to be caused by a thickening of the cellular texture under the pleura. The interior of the right lung was in a state of hyperemia, the upper lobe to a great extent; the two others less so, but not natural. The left lung was in a slight degree affected in the same way. The mucous membrane of both lungs, and of the trachea, was very much reddened, and somewhat thickened. The longitudinal fibres were strongly marked in all the large bronchial tubes, as in persons affected with chronic bronchial inflammation. Emphysematous appearances were visible in the lower part of the lungs. The cavities of the heart were quite empty. The foramen ovale was so far open as readily to allow an instrument to pass from the right to the left auricle, through an aperture a quarter of an inch long. In the cavity of the abdomen, the intestines were excessively distended with flatus. The whole peritoneal surface, both lining and investing, was covered with a layer of coagulated lymph, and the capillary vessels injected with red blood. The liver, of a lighter colour than common, was of a crisp consistence, and its coat opaque. The uterus was enlarged, thickened, and very hard: its fundus was covered with a tumour as large as a walnut, excessively hard, so as to be cut open with difficulty. The cut surface presented an arborescent appearance of white striae, branching from the pedicle which connected it with the body of the organ, exhibiting the pedunculated growth described by Dr. Hodgkin. The right ovarium was increased in size, hard, and contained a number of enlarged ova, some of the size of a pea, with thickened, hard, white coats. The left ovarium was double its common

size, soft, and presenting a number of flaccid sacs. Near the ovaria were seen two or three membranous strings, from one to three inches long, with a cyst at the extremity of each lying unattached in the cavity of the abdomen. An incision into the substance of the uterus exhibited two or three hard white nodules, like small pebbles of quartz. The membranes of the brain were not inflamed, and nothing morbid was seen in its substance. The lymphatic glands in the axilla were rather large: one of them had three times the usual size: this gland being divided was found much reddened within, and contained two kinds of structure; an external, of a deep red, and an internal, composed of white rays. Two or three absorbent vessels, running into this gland, were considerably enlarged from their natural size. The cellular texture surrounding and enveloping this gland was intermixed with striæ of white fibres.

"This patient was unmarried, of excellent character, of a constitution not vigorous, but rather inclining to scrofula. No cause was known for the disease. The immediate consequences of the operation were in every respect favorable; the wound was in a healing state when she was seized with fever. The existence of erythema erysipelatousum in the hospital at the time gives us reason to believe that this patient was affected and died with an erysipelalous inflammation of the internal organs. No cause we are acquainted with can so well explain this sudden and unexpected termination. Nor is such an occurrence new to me: I have seen numbers of patients perish a few days after operations, at the time that erysipelas prevailed in the hospital, without the slightest external erythema. In such instances I have been in the habit of stating to you that these patients died of erysipelas, as truly as if they had been covered with an erythematous eruption. The disease is constitutional: it may affect the skin, and generally does so; it may affect the internal organs without affecting the skin, and in such cases is most dangerous." (P. 251.)

The next case we give in an abbreviated form: it is chosen on account of the illustration it affords of the introduction of air into the veins during the performance of an operation; a phenomenon which seems now established by too many instances and too high authority to admit of doubt. No argument, *a priori*, derived from the anatomical study of the heart's cavities, could have led to the supposition that a capability of active expansion resided in its parietes; nor would such an explanation as the author has afforded of the mode in which air gains admission have induced any one to expect such an occurrence, in the absence of direct experience: but it is vain to oppose theory and speculation to repeated and well-attested facts; and, though we still urge a rigorous scrutiny in all such cases as may occur, we feel bound to give credence to observations so carefully made, however extraordinary the relation may appear.

"Nancy Barker, of Trenton, in Maine; married, æt. 33. Three years since she noticed an induration in the right breast, which increased till it involved the whole gland in a tumour, very hard, moveable, yet connected with the pectoral muscle by a morbid adhesion. The nipple drawn in. The axilla is occupied by a considerable tumour, of a globular form, and quite hard. An operation was performed on the 24th of December, 1831. The patient sat in a chair. The right arm was extended and raised above a horizontal line, in order to give tension to the skin and permit access to the armpit. The skin on the surface of the breast, with the diseased nipple, was included in an oval incision. The breast was dissected from the pectoral muscle, and left connected with the axillary glands, while the extirpation of these glands was effected. As they adhered to the great axillary vessels, they were cautiously detached by dissection; and, by insinuating the finger where the cellular substance was loose, between the tumour and the great vein, this separation was nearly effected, only a slight connexion still existing at either extremity of the tumour. Proceeding to separate it, at the outer part of the axilla, a vein was divided, and a small quantity of venous blood discharged. Scarcely was this done when the patient struggled, her complexion changed to a livid colour, and at the same instant a bubbling or gurgling noise, which had not been noticed before, was heard, though indistinctly; but the place

from which it issued was not visible, the surrounding skin and fat lying over it. On this, the axilla was immediately compressed. The patient became insensible, breathing as in apoplexy. The tumour was at once separated. The posture of the patient was changed, and stimulants were diligently administered; but the pulse became less distinct every instant, the lividity of the cheeks increased, the body grew cold, and the respiration more and more feeble until it entirely ceased. The lungs were inflated as a last effort, but without avail.

“In order to understand the manner in which air gains admission into the veins two things are to be considered: the state of the heart, and that of the affected vein. First, the heart: This organ has a dilating as well as a contracting power. The auricles, after contracting, dilate by an active motion, and suck the blood from the neighbouring veins: by this suction of the right auricle it is that air may be drawn in at the opening in the wounded vein. Second, the condition of the vein is to be regarded: The coats of the veins are flaccid, and, in their ordinary state, an attempt to suck in air at any aperture would be followed by a collapse of the walls of the vein; and the introduction of air in this way would be impossible: but, if the coats of the vein are prevented from collapsing by an adhesion to an unyielding substance, air might be sucked into and through it without any difficulty. If a suction-hose were composed of a thin skin, water could not be drawn through it; but if it were covered with metal, to which it adhered, there could be no obstacle to the suction process. In order that air may be sucked into a vein in the living body, its coats must be prevented from collapsing; and this may be done by different causes. First, by position: If the arm, for example, be extended to the utmost degree, the axillary vein will at the time be in a state of tension; and, should an aperture be made in it in that situation, the vein could not collapse, and air might be drawn in. Second, if the vein passed through and adhered on the outside to a firm tumour, the vessel could not collapse. Third, the same result would be produced by an attachment to surrounding fascia, which is again attached to bone. Other causes might produce the same effect; as the situation of a short vein between two others, with each of which it is connected: the situation, for example, of the transverse jugular veins, which pass across from the anterior to the middle jugular. When such conditions of the veins near the heart are taken into view, together with the suction-power of the right auricle, there seems to be no difficulty in explaining the entrance of the air into the vein and heart. The mode in which air introduced into the circulatory system causes a derangement of the vital functions is not so satisfactorily explained. The organs whose functions have been supposed to be especially deranged are the lungs, the brain, and the heart. The lungs have been found to contain air in their sanguiferous vessels; and, as the phenomena bear a resemblance to those of asphyxia, this organ has been supposed to be the primary seat of derangement. The livid appearance of the skin, also, and the gurgling noise in the chest, seem to indicate an affection of the lungs; but the latter symptom arises merely from the passage of air through the blood of the heart and of the lungs; the former from the imperfect state of the circulation. The brain, by others, is believed to be affected by the introduction of air into its blood-vessels. Bichât produced appearances in animals similar to those from this accident, by injecting air into the carotid artery; and, in our own cases, the privation of sensibility, the slowness of the pulse, and the heavy respiration, appear to indicate pressure on the brain. It has been said that the time is not sufficient for the imbibed air to reach the brain; but there is a sensible interval between the entrance of the air and occurrence of the phenomena,—an interval quite sufficient for the air to pass through the right side of the heart, the lungs, the left side of the heart, and the carotid arteries, to the brain. In the examinations of the bodies of some of the victims of this accident, air has been found in the right auricle of the heart. This fact, taken in connexion with the slowness or total failure of the pulse, has been thought to prove that the great organ of the circulation is the principal seat of the disorder. The experiments of Nysten favour this explanation, and seem to shew that the distension of the heart by air is sufficient to account for the fatal effects of the introduction of that fluid into the veins.” (P. 259.)

In a work exclusively treating of tumours, we are not to be surprised at meeting with the description of some operations which seem to have been undertaken with somewhat too bold a hand, and where the risk which the patient ran of perishing under the knife should rather serve as a warning than as an encouragement to imitation. In this light we are disposed to view the operations performed upon the parotid and thyroid glands; though we willingly concede that Dr. Warren undertook these and other operations aware of the difficulties and dangers, and well prepared with skill and anatomical knowledge to encounter and overcome them. He thus describes the extirpation of the parotid gland:

"An incision, three inches long, was carried from behind the ear to below the angle of the jaw, in a slightly oblique direction from behind forwards. This not affording room, a transverse incision, an inch and a half long, was carried from the anterior lip of the wound in the direction of the lower edge of the jaw, and just below this bone. The tumour was then dissected from the face, ear, and mastoid muscle. The facial or external maxillary artery was cut, and immediately tied. The tumour being disengaged as much as possible from the parts above mentioned, was seized with a double hook at the inferior part, and drawn outwards. The dissection under its lower edge was carried on cautiously until it became deep, and the pulsations of the carotid were quite visible, though the vessel itself could not be discovered so as to secure it. Now being sensible of the probability of involving the external carotid artery, I requested an assistant to compress the common carotid, and then began to pursue the deep dissection. In an instant the assistants, the operator, and the ceiling of the room were covered with a torrent of blood. The common carotid was at once compressed effectually. The blood being sponged off, I sought for the divided artery, but soon found it had retreated below and within the digastric and stylo-hyoid muscles. These muscles were dissected, the artery brought into view and tied, the operation continued and concluded." (P. 288.)

The patient recovered.

Dr. Warren is not deterred by the fears that restrain the operators of this country from dividing and placing ligatures upon veins. For a varicose state of the leg he recommends the division of the vein below the knee, in the following manner: "A bandage being placed above the knee, to prevent the entrance of air, the skin and vein are to be pinched up together, and divided at one cut;" and he remarks, "there is no danger of reunion of the vein; nor have I ever seen any other bad consequences in the many cases in which I have operated by division of the vein." (P. 436.)

Of varicocele a single case is related, in which a ligature was applied with success upon the spermatic veins, (p. 439;) but the operation which is principally recommended for this complaint is the excision of the spermatic veins. This we are told has been done many times with success, and never with any unpleasant consequences, excepting in a single instance: then, however, the consequences were unpleasant in no very slight degree, inasmuch as hemorrhage occurred, and produced an extensive slough of the scrotum, in the separation of which the testicle itself was removed.

"The proper course," says Dr. Warren, "would be, to let the patient know that the testicle is probably useless before the operation, and that after it the organ may wither and shrink away. I mean to say that it may wither up, not that it necessarily will do so. We must then leave to the patient the decision of the question whether he will continue to support the disease or take the consequences of the operation.

On the whole, I consider excision of the veins as the only sure mode; and that it is generally free from any bad consequences which may not be obviated, excepting only the wasting of the testicle." (P. 444.)

The frequent occurrence of varicocele, in a more or less severe form, makes it much to be regretted that the advantages of the operation here proposed should admit of so important an exception; one, indeed, which entirely excludes it from all cases in which the organ is not already entirely useless, or in which the sufferings of the patient are not excessive. When this condition has become such that castration would be justifiable, the operation of tying the veins would also be allowable, and perhaps preferable; but, until the last stage of the disorder has been reached, there will not be found many who would not, on a fair representation of the circumstances, rather content themselves with the palliative treatment, however troublesome and ineffectual.

The histories of Abdominal Tumours, which close the volume, are introduced by some judicious remarks respecting the mode of making an examination, and afford a very clear and simple description of the diagnostic signs which distinguish the varieties of these complaints; but, as we shall have occasion to return to this subject in noticing the cases of Hydatid Tumours of the Abdomen, published by Dr. Bright in the last Number of the Guy's Hospital Reports, we shall here close our remarks on Dr. Warren's book. Of the engravings by which the text is illustrated we may briefly remark, that, although they are not to be compared in point of elegance with those in the papers just referred to, and still less with those in Dr. Bright's former works, yet the praise of general accuracy and usefulness must be awarded them. By augmenting the impression produced by the narrative, they serve to fix on the mind the objects to which they refer, and supply the deficiencies of language where this is inadequate to convey distinctly the appearances of disease.

ART. VII.

1. *A Treatise on the Diagnosis and Treatment of Diseases of the Chest.* Part I. *Diseases of the Lung and Windpipe.* By WILLIAM STOKES, M.D. M.R.I.A., Physician to the Meath Hospital, &c.—*Dublin*, 1837. 8vo. pp. 557.
2. *Notes et Additions au Traité de l'Auscultation Médiate de LAENNEC.* Par M. MERIADEC LAENNEC, D.M.P. &c., et M. ANDRAL, Professeur à la Faculté de Médecine de Paris, Médecin de l'Hôpital de la Charité, &c. *Notes and Additions to the Treatise on Mediate Auscultation of LAENNEC.* By M. MERIADEC LAENNEC, M.D. Paris, &c.; and M. ANDRAL, Professor of the Faculty of Medicine of Paris, Physician to the Hospital of La Charité, &c.—8vo. pp. 510; with two coloured Plates. *Paris*, 1836.
3. *Observations on the Surgical Pathology of the Larynx and Trachea, &c.* By WM. HENRY PORTER, A.M., Vice-President and Professor of the Theory and Practice of Surgery in the Royal College of Surgeons in Ireland; Surgeon to the Meath Hospital, &c.—*Dublin*, 1837. 8vo. pp. 280.

4. *A Treatise on the Diseases and Injuries of the Larynx and Trachea, &c.* By FREDERICK RYLAND, Surgeon to the Town Infirmary, Birmingham.—London, 1837. 8vo. pp. 338; with Plates.

5. *Traité pratique de la Phthisie laryngée de la Laryngite chronique, et des Maladies de la Voix.* Par MM. A. TROUSSEAU et H. BELLOC. *Ouvrage couronné par l'Académie Royale de Médecine.*—8vo. pp. 488. Paris, 1837.

Practical Treatise on Laryngeal Phthisis, Chronic Laryngitis, and Affections of the Voice. By MESSRS. A. TROUSSEAU and BELLOC, &c. —With Engravings.

In a former Number, (No. VIII., Art. I.) we were occupied so fully by the valuable work of Dr. Stokes and the interesting Notes of M. Andral, that we had space only to mention the names of the other works which stood at the head of our article. To these, the chief subjects of which are the diseases of the larynx and trachea, we now add another volume, from the other side of the Channel; but, before proceeding to notice them, we must say a few words on the concluding section of Dr. Stokes's work on Diseases of the Pleura, and also on the Notes of M. Andral on Diseases of the Heart.

We believe that Dr. Stokes is the first writer who has recognized a fibrous layer in the pulmonary pleura; although an account of it was published by M. Bazin, (see our last Number, p. 220,) previously to the appearance of Dr. Stokes's treatise. This membrane may be shewn by making a very slight incision, in the shape of the letter U, on the surface of a distended lung, and gently drawing back the serous membrane: the fibrous, which is much stronger, is left smooth and shining under, and may also be separated from the irregular surface of the pulmonary tissue. This fibrous tunic, although increasing the strength of the pleura, does not prevent it from yielding to a certain degree of distension, as is seen in the displacement of the mediastinum by pleuritic effusions and dilatation of the pericardium in the analogous affection of this membrane.

But the most important part of Dr. Stokes's observations is on paralysis of the intercostal muscles and diaphragm, as an effect of the inflammation of the pleura, and as a cause of the yielding of these muscles to the pressure of the liquid effusion. As in regard to the dilatation of the bronchi as a result of bronchitis, he views the enlargement of the side and protrusion of the intercostal spaces as more than the result of mere mechanical pressure, and the extent to which it takes place distinguishes this affection from simple hydrothorax and pulmonary emphysema, where the dilating causes are simply mechanical, and there is no elevation of the intercostal depressions. We are disposed to agree with Dr. Stokes that in many cases there is a paralysis of the intercostal muscles, even not of the diaphragm, on one side; but we doubt that it is constant, even in chronic cases; and, where it does exist, something should be attributed to the disuse of the muscles from the fixed state of the side, as well as to the paralyzing influence of inflammation. We can, however, bear testimony to the truth of the remark, that irritation of the pleura causes spasmodic action of the intercostal muscles, followed by paralysis. In a case of perforation of the lung, with consequent pleurisy, which we had the opportunity of watching closely from its onset, the knotty spastic state

of the intercostal muscles was at first most remarkable, and was in a few days followed by relaxation and tympanitic distension; so that the chest on that side became shining and almost without irregularities.

For measuring the chest, with the view to discover dilatation or contraction of the side, and to determine how it changes from time to time, Dr. Stokes has contrived a pair of spring callipers, with an arc graduated in inches and tenths, and with a wooden ball on each extremity. Above the graduated arc there is a spiral spring in a box, by which the balls can be kept applied to the chest during the movements of respiration, which may also be measured on the arc. This instrument has several advantages over the usual method of measurement: it enables one to read off at once the diameter of any part of the chest from front to back, as well as from the spine to the side; and, by fixing one ball on the ensiform cartilage or on the spine of the lowest dorsal vertebra, by moving the other ball to the lower edge of the clavicle or to the upper ridge of the scapula, the depth of the chest may also be accurately compared on the two sides. Still this is an additional piece of apparatus, and, although not a complicated one, it is not likely to be brought into use, except by the more zealous physicians in their hospital practice. Improved methods of gaining knowledge are daily multiplying upon us; but our time is not lengthened in proportion, and we are often constrained to use the simplest method, and that which takes least time, rather than the most complete and accurate.

Dr. Stokes notices a new mode of displacement of the heart to the right side, in consequence of the rapid absorption of a pleuritic effusion on that side. It is supposed that the lungs, being bound down by adhesions, and the chest unable to contract as soon as the fluid was absorbed, the mediastinum, with the heart and left lung, were drawn over to the right side, to fill the vacuity. We doubt that the rapid absorption of the effusion is necessary to produce this result, which may be rather expected when the effusion has remained long, and the lung so bound down in its compressed state that no collapse of the chest can efface the vacuity. We have at present a patient under our care, a boy thirteen years of age, who had a latent pleurisy of the right side, ten months ago: the chest has been contracting during the last four or five months; the respiratory murmur, which is distinct in the upper two-thirds of the right side, does not reach to the lower third, which is quite dull on percussion, and the heart can be seen and felt to pulsate two inches to the right of the sternum.

We will conclude the subject of diseases of the chest with a notice (short, because we have so recently reviewed the subject in connexion with M. Bouillaud's work,) of some of M. Andral's remarks on the auscultatory phenomena of the heart. They are expressed in his peculiar clear style, and evince his usual discretion and liberality of sentiment, although the somewhat exclusive patriotism of his countrymen is, perhaps, also apparent. It is difficult to say whether it is from ignorance of the English language, or from a national partiality to the works of his own countrymen, that M. Andral scarcely notices the researches or opinions of English writers on this subject, although they were published years before those of the French authors to whom he ascribes them. We would hope, for the sake of the fair name of Andral, which stands so high

for liberality and caudour, that the former reason is the cause of his silence; for he makes an exception in favour of the experiments of the Dublin Committee of the British Association, which had been translated in the *Encyclographie*. The experiments of Dr. Williams, although nearly the same, and published before those of the Dublin Committee were performed, are not noticed, because, we presume, they had not been translated.

The following are supposed by M. Andral to be the causes of the bellows' and other murmurs heard in the region of the heart:—1. Difficulty in the passage of the blood through the various orifices of the heart. 2. Unnatural reflux of blood through the orifices by which it has already passed. 3. Modification in the play or movements of the valves. 4. Abnormal contraction of the muscular tissue of the heart. 5. Increased force of impulse. 6. The compression of a tumour. 7. Friction between the layers of the pericardium, altered by disease. 8. Chlorotic and anæmic states of the system, acting in some unknown way, producing murmurs in the arteries more frequently than in the heart.

Among the first of these, M. Andral reckons not only morbid constrictions of the orifices, which are undisputed causes in many cases, but also too large a quantity of blood, a change in the size of the cavities of the heart, and a loss of smoothness in its internal surface. The three last appear to us questionable. M. Andral admits that he has seen very few cases in which plethora alone could be regarded as a cause of bellows' murmur. It is not easy to conceive either how enlargement of the cavities alone can produce a murmur. When it occurs with thinning of the walls, it undoubtedly causes a louder and shorter first sound; but this is the reverse of a murmur; and we suspect that, in the cases where a bellows' murmur does accompany this condition, there is either a dilated state of the arterial trunk, or an imperfect closure of an auriculo-ventricular valve. The latter is, according to our experience, a very common cause of abnormal murmurs, and is liable to be overlooked by those who do not duly consider the structure and action of these valves. The lengthening or shortening of any of the tendinous cords, change of structure, or loss of power, or irregular action in any of the columnæ carneæ; rigidity, puckering, or wasting of the laminæ of the valves, may afford a chink through which the blood will be forced, in a sonorous jet, back into the auricle. The cause of sound is intelligible here; but not so with a simply dilated ventricle. We would fully recognize the existence of the second class of causes,—insufficiency of the arterial valves, permitting regurgitation. These were first distinctly described by Dr. Corrigan; but, as we have just remarked, the auriculo-ventricular valves may be the seat of a similar lesion. So also change in the structure of the arterial valves may modify the character of the natural second sound of the heart, which has been fully proved to have its cause in the reaction of the blood on them.

M. Andral has, like some other writers, classed together, in a manner that we cannot think correct, the various murmurs which are obviously produced in the current of blood with the friction sound of pericarditis, the seat of which is external to the heart, and the natural sounds, which are most probably dependent on the solid structure of the heart. Yet these three kinds of sound,—a bellows' or sawing murmur, a friction sound, and the

natural double sound,—not unfrequently coexist at the same time; and the manner in which the two former arise and cease, the natural sound remaining the same, or being modified only in its own way, strongly indicate the separate seat of these several sounds, and that the former are rather additions to, than modifications of, the latter. The character of the double sound of the heart may be varied by disease: dilatation renders the first clear and short, hypertrophy long and dull; but such varieties can be always distinguished from complications with murmurs, which, in their smallest degrees, can, by the practised ear, be distinguished to be additions to the natural sounds.

With regard to the eighth class of causes of murmurs, chlorotic or anæmic states of the system, M. Andral expresses a strong bias to the opinion that they produce the sounds in question in consequence of an altered condition of the blood. The remarkable sound heard in the neck, called, from its resemblance to the noise of a toy of that name, "*bruit de diable*;" and one of an analogous kind, which, from its likeness to the buzzing of a fly, is termed "*bruit de mouche*," are considered by M. Andral to be of this kind in the carotid arteries; and so convinced is he that they indicate the existence of a chlorotic state of the system, that he is guided by their presence to prescribe steel medicines for the patient. On the subject of the *bruit de diable*, we stated, in a former Number, the opinion of Dr. Ogier Ward, that it is produced in the jugular veins. This opinion has been confirmed by the experiments of the London Committee of the British Association, appointed to investigate the sounds of the heart. The experimenters, Drs. Williams and Todd, state, moreover, in their Report, (*see, in our present Number, Part IV.*) that, so far from being essentially connected with a chlorotic or any other morbid state of the blood, they were able to produce it at pleasure, in the healthiest subject, by a certain degree of pressure with the stethoscope on the jugular veins; and, in this and every other case of its production, it could be stopped or increased at pleasure by various circumstances which would stop or accelerate the current of blood in these veins. It is probable, however, that in chlorotic patients and others, in whom the circulation is rapid, the veins not full, and the blood thin, these sounds may be more readily produced; and that, under such circumstances, the pressure of the strained muscles of the neck, independently of that of the stethoscope, may be sufficient to excite it. Since the Report of the Committee has been published, we have met with several cases in which a continuous buzzing or humming sound was heard in the neck and below the sternal ends of the clavicles; sometimes obviously produced by the pressure of the instrument, but in others without it: in two of these there was an enlarged thyroid gland, in two others aneurism of the aorta pressing on the veins; in all cases, considerable pressure on the jugular veins in the neck stopped the sounds. We think that attention to these sounds may assist in the diagnosis of tumours in the upper part of the chest.

We now turn to the subject of Diseases of the Larynx and Trachea.

On looking through the different works the titles of which are prefixed, we find them all to possess considerable merit, although differing somewhat in their excellencies. The treatise of Mr. Porter is a second and improved edition of a work with which his name has long been favorably

associated, and in which many interesting observations and judicious reflections are to be found. Its principal object was to determine, by collected cases and pathological reasoning, what are the affections and circumstances in which the important operation of *bronchotomy* may properly be performed. Ten years of additional experience, improved latterly by the assistance afforded by auscultation, have enabled the author to enlarge and enforce his "Observations," which ought to be carefully studied by every young medical man, who may be called at any moment to the responsibility of using or withholding this momentous resource of surgery. It is much to be regretted that observations valuable as these, should not have the advantage of clearer language and arrangement. There is such a looseness in the style of composition, and such a want of system in the whole work, that many a hasty reader will, we expect, rather derive from it a few vague and unstable ideas than the solid instruction which it ought to afford. There is not even a table of contents.

The treatise of Mr. Ryland is chiefly a compilation, (and a very good one it is,) comprising a great part of the best and most recent information in this and other languages, fairly written, well arranged, and commented on by one who is practically conversant with his subject, and who adds some interesting results of his own experience. This is the most complete treatise of the whole, and we shall take it as the basis of our brief review: it is an enlargement of an essay to which was adjudged the Jacksonian prize of 1835.

The joint work of MM. Trousseau and Belloc is also founded on a history of Laryngeal Phthisis which successfully competed for a prize offered by the Royal Academy of Medicine at Paris. The authors had been engaged in the work before its subject was proposed as a prize-question. They have collected, from various sources, upwards of sixty cases of different affections of the larynx and trachea. They have not failed to extract cases from English writers; but, in their historical notice of those who have contributed to the knowledge of the subject, they too often seem to forget that the English have done anything.

Mr. Ryland's first three chapters contain a brief but clear account of the anatomy of the larynx and trachea, their development and malformations,—the physiology of the larynx and the voice. The fourth chapter is a condensed transcript from Mr. Porter's "Observations" on the general pathology of the mucous membrane. Inflammation changes the natural pale-rose tint to a red, or even purple or deep violet: in the chronic state this redness is lighter, and merely in patches; which are shaded into the natural tint. We have seen, however, in chronic cases occurring in scorbutic subjects, the membrane assuming the deepest red tint. The mucous membrane, and the submucous cellular tissue under it, also become swelled and pulpy in proportion to their laxity: thus the lips of the glottis, when inflamed, often become so much swelled as to impede the passage of air in respiration; while the membranes covering the *cricoid cartilage* and rings of the trachea are scarcely thickened. The same thing may be observed of *œdema* and deposition of lymph. Ulceration frequently follows inflammation, and it may attack any part of the membrane: after the acute form, the lips of the glottis and upper parts of the larynx, in the irritation accompanying tubercular consumption,

the covering of the cricoid cartilage, vocal cords, and under surface of the epiglottis and posterior part of the trachea, are the most usual seats of ulceration; while aphthous and venereal ulcers more generally occupy the upper parts of the larynx, and extend from the fauces or pharynx. We do not quite understand why Mr. Porter and Mr. Ryland believe these ulcers never to heal by granulations, but only by a contraction of the edges of the sore. The wrinkled stellated appearance of their cicatrices is surely no proof of this; for the same puckering is seen in the scars from ulcerated wounds of the integuments, which never could have healed but by granulation. There seems to be a general tendency in new tissues, when slowly formed, to contract and become condensed for some time after their formation. Mr. Porter describes as the most important form a broad flat ulcer with uneven edges, covered with a bright yellow slough, and disposed to spread rapidly from the arch of the palate or the pharynx down to the larynx. They rarely heal spontaneously; but the patient generally dies from the depraved and cachectic state to which they owe their origin, and which is often connected with a scrofulous or venereal taint. Besides ulcers, MM. Trousseau and Belloc describe erosions of the mucous chorion or epithelium, which may be mistaken for ulcers. If these be examined under water, they will be found to present a villous surface, unusual in the natural state of this mucous membrane. They do not appear to be connected in any way with ulceration; but, as they are only found in cases of pulmonary phthisis, Louis concluded that they are caused by the action of the expectorated matter on the membrane.

The changes of the secretion of the mucous membrane of the windpipe from inflammation are much the same as those of the bronchi, but perhaps more marked. Thus, in the first stage, it is quite thin and acrid, subsequently becomes viscid, and at the acme of inflammation entirely ceases; the secreting crypts and follicles being, we conceive, blocked up by the interstitial effusion. In this particular the mucous membrane contrasts with the serous, and the more so as it is more mucous and complicated with follicular structures. The albuminous exudations and formation of false membranes depend on the character rather than on the intensity of the inflammation: their greater prevalence before than after puberty is well known. Dr. Stokes offers, by way of explanation, the predominance of the white or reproductive tissue at an early age. We see a simpler explanation in the general fact of the greater activity of the nutritive function in the child, the greater freedom with which the plastic material is supplied from the blood, and perhaps also the simpler, finer condition of the mucous membrane at that age; when, not having been long subjected to the various irritations which it has to endure, its follicular apparatus has not yet attained its full activity or development. Hence the effect of inflammation on it will be more like that on serous membranes. In all these cases, however, we must regard the condition of the blood as an element essential to the production of any particular result of inflammation; and this alone, existing in excess, may, although rarely, be sufficient to cause a similar result in after-life.

The inflammatory diseases of the larynx and trachea are severally taken up by Mr. Ryland, in as many chapters. On Acute Laryngitis, Œdema of the Glottis, Erysipelatous Laryngitis, Chronic Laryngitis,

(subdivided into that affecting the soft parts and that affecting the cartilages,) Croup, and Diphtheritis. The other authors place croup in the foremost rank; and certainly its greater frequency and decided inflammatory character would assign to it that place, although the peculiarity of its product marks it apart.

The acute laryngitis of adults frequently commences with symptoms of ordinary sore-throat, accompanied with fever; the pain, constriction, and soreness in the larynx coming on after, with a harshness of voice and frequent husky cough. By pressing the tongue forwards and downwards, the epiglottis may often be seen, erect, thickened, and of a deep red colour; which state of the part often causes a difficulty of swallowing. Then rapidly follow signs of swelling of the membranes of the larynx, and consequent obstruction to the breathing, bringing with it those frightful symptoms of that strangulation in which too often the disease terminates with awful rapidity, sometimes within twenty-four hours of the first attack. Mr. Ryland quotes four cases, illustrative of the character of the disease, and gives a table of twenty-eight, with their results. Of these, ten recovered; but Mr. Ryland justly considers this above the average. The unfavorable cases are not so freely acknowledged by writers as those of recovery. Death probably depends in part on a spasm superadded to the constriction from thickening; for there are generally exacerbations of the dyspnœa which are too sudden to be anything but spasmodic. Sometimes, however, the patient dies in the interval, but not without a duration of the dyspnœa sufficient to produce the lividity, congestions, and effusions of impending asphyxia.

All writers seem to agree with respect to the treatment of acute laryngitis, that it must be of the most prompt and vigorous nature; and, unless it gives very speedy relief, bronchotomy should be performed, to keep the patient alive until the remedies shall have time to act. Free bleeding, general and local, but not to syncope (Dr. Cheyne thinks), mercury and antimony in full doses, may be given a chance for a few hours; but, as soon as ever the dyspnœa becomes distressing, the operation should not be delayed. "As long as bronchotomy is considered an extreme measure, a *dernier ressort*, it will always be performed too late in this disease:" so said Louis. Mr. Lawrence says that it should be done "as soon as the symptoms enable us to determine the nature of the disease." It has generally failed from having been delayed too long; it does, however, sometimes, although rarely, succeed in extreme cases.

"In a case quoted by Dr. Cheyne, the operation was performed by Mr. Goodeve after the pulse had ceased at the wrist, the face suffused, and the lips livid; yet recovery took place. I shall never forget the case of a gentleman, aged upwards of sixty years, who had recently recovered from a violent pneumonia, and was attacked with the most violent form of acute laryngitis, which, baffling all efforts to control it, brought the patient to the jaws of death in little more than twelve hours. It was determined that Mr. Porter should be sent for to operate; but, before he arrived, the patient had become nearly insensible. The operation was proceeded with, but respiration ceased before the trachea was opened. The operator paused,—it was a fearful moment,—and then rapidly opened the trachea; yet no sound of inspiration followed. Applying his mouth to the wound, he inflated the lungs and produced artificial respiration at least seven times, when a loud and rattling inspiration, followed by full and free bleeding, proclaimed the triumph of art." (Stokes, p. 234.)

The triumph was but short, however; for we learn from Mr. Porter

that, on the night of the second day, bronchitis set in, and the patient died the following morning. When the laryngeal constriction has once begun the process of asphyxiation, the lungs sustain an injury which, with the operation and the remains of the laryngeal disease, constitute a fatal combination against recovery.

All our authors regard œdema of the glottis as inflammatory. Mr. Porter identifies it entirely with acute laryngitis, under the name *L. œdematosa*. MM. Trousseau and Belloc, who are obviously of the Broussaian school, strenuously maintain that all acute affections of the larynx tending to constriction (we might say that they include chronic also,) are inflammatory, and that spasm is an imaginary cause; for, although the parts be found, as Bayle, Laennec, Cayol, and Dupuytren have observed, perfectly pale after death, the epiglottis, during life, has been seen to be in these cases of a cherry-red colour. But it might be questioned how far this redness (which might be a mere congestion, or the result of the straining and exertion of the parts consequent on spasm,) is always to be regarded as a proof of actual inflammation, where it is followed by no further change. In a case of œdematous laryngitis, given by Mr. Ryland, the epiglottis was not erect, as it is in acute laryngitis; and the absence of pain and fever also implies a distinction from this affection. It approaches to the erysipelatous form of inflammation, acute laryngitis being more phlegmonous. Mr. Ryland gives a good coloured drawing of the larynx of a patient who died from the effects of skinning a glandered horse with a punctured hand. The lips of the glottis are seen almost completely closed by their œdematous distension. In cases of œdema of the glottis that have terminated speedily, the liquid effused has been found to be a limpid serum; in those which have been slower, a semi-purulent matter: so similar differences are observed in the œdema of loose external textures produced by erysipelas. Generally, œdematous laryngitis is excited by chronic disease of the larynx or adjoining parts; but it may be developed independently, as Bayle remarked, especially during convalescence from typhoid fevers; and it may, by producing small abscesses and ulcerations which reach the cartilages, induce laryngeal phthisis. Medicine has even less control over this affection than over the more sthenic form of laryngitis. Local bleeding may relieve the soreness, but it rarely influences the œdema; and, if mercury might do more, there is not time for its action. This is eminently a case for bronchotomy, which should not be delayed when once the disease has declared itself.

Mr. Ryland gives six original cases of laryngitis, which he calls erysipelatous, because erysipelas either preceded or followed it, or was prevailing when the cases occurred. Two coloured delineations are given to shew the inflamed state of the larynx, and effusion of matter and sloughy condition of the submucous cellular tissue: the erysipelas here appears to have been of the phlegmonous kind. Two cases recovered: in one, improvement being accompanied by the appearance of erysipelas on the face; in the other, erysipelas of the face and scalp existed during the whole complaint, and the parts were dressed with mercurial ointment, while calomel was given internally, and leeches applied to the throat. Mr. Ryland suggests that, as the fauces are usually attacked first, the occurrence of pain, heat, and dryness in the throat of an erysipela-

tous patient, with impeded deglutition, should occasion prompt efforts to stop the progress of the inflammation to the larynx, by leeches behind the angles of the jaw, free blistering of the back of the neck or upper part of the sternum, the inhalation of steam, and internally by mercurial and antimonial medicines, or tonics, according to the state of the general strength and fever. If the voice and breathing, with a sense of constriction in the larynx, announce that this organ is affected, speedy counter-irritation of the external parts, by *acetum lyttæ* or boiling water, must be resorted to. Bronchotomy is spoken of more doubtfully, by both Mr. Porter and Mr. Ryland, in this form of laryngitis; "because the erysipelas, having existed for some days previous to its attacking the larynx, will have considerably lowered the powers of the system, and perhaps have impaired the condition of the brain." There is, however, the case of a patient of St. George's Hospital, narrated in the Medical Gazette for April, 1837, in which this operation was successful in laryngeal constriction suddenly supervening during erysipelas of the face.

Chronic laryngitis is generally a disease very insidious in its origin, often causing little pain but generally a dry hacking cough, and still more constantly some changes of the voice. The inflammation may be in the mucous and submucous tissues only, or it may extend to the cartilages. The latter form, Mr. Ryland thinks, best deserves the name of Phthisis laryngea, from its incurable nature, and the hectic and emaciation which invariably accompany its latter stages. MM. Trousseau and Belloc, however, consider that both forms lead to atrophy, and merit the name of Laryngeal phthisis, which they refuse to admit to be a disease *sui generis*. The treatise of these authors, being expressly devoted to this subject, contains the fullest and most accurate history of chronic laryngitis that has yet appeared, and we wish that our limits would permit us to extract more freely from it. They describe, as the products or complications of this disease, erosions and ulcerations of the mucous membrane, ossification, necrosis and caries of the cartilages, ossification of their peri-chondrium, morbid alterations arising from the preceding lesions, polypi, vegetations, cancerous and tuberculous productions, hydatids, false membranes, and calculi. The description of these lesions occupies fifty pages. We can only notice one or two of the more remarkable. Ossification of the cartilages of the larynx takes place naturally, in advanced life, as in those of the ribs; but chronic laryngitis, of two years' duration, causes the same change in young persons. This is in conformity with a law well developed by M. Andral, that chronic inflammation, or a certain degree of irritation, accelerates those changes in tissues to which time would naturally bring them. The osseous matter is deposited in irregular plates on the surface of the cartilage, and sometimes quite encases it.

Necrosis of the cartilages has scarcely been mentioned by authors. Mr. Porter describes as unusual two cases, and speaks with hesitation regarding the lesion. Mr. Ryland gives a case of necrosis of the cricoid cartilage, and quotes another from Mr. Lawrence. MM. T. and B. state that they have found necrosis in more than half of the fatal cases of laryngeal phthisis which they have examined. The necrosed portion is always denuded, and whenever its death has been caused by disease of some standing, it is ossified also. They consider the ossification to have

been caused by ulceration in the vicinity of the cartilage; and when this ulceration reaches the cartilage so degraded by ossification, it soon destroys its little remaining vitality. In support of this view they remark, that in very young subjects, where the cartilages are more *living*, they suffer a less common lesion, caries, rather than necrosis; and the same thing takes place in the arytenoid cartilages and epiglottis, which are never, according to the experience of these authors, ossified. In a case of Mr. Ryland, however, all that was found of the arytenoid cartilages was an ossified remnant of the left cartilage; and Mr. Porter relates another in which an ossified arytenoid cartilage was expectorated. The sequestrum of dead cartilage is not readily thrown off by the living; and as a cause of local and constitutional irritation, or as a foreign body in the larynx, when at length detached, it may be the cause of fatal disorder. Ulceration of the mucous and submucous membrane, and caries, and necrosis of the cartilages, frequently cause considerable engorgement and swelling of the adjacent cellular tissue, which constitutes a common cause of fatal obstruction in the larynx, improperly termed œdema of the glottis. Of nine cases of œdema of the glottis examined after death by these authors, two were acute: one supervened on a violent catarrh, the other after the operation of tracheotomy: seven were chronic, five occurred with necrosis, caries, or ulcers of the larynx, two with ulcerated tumours.

MM. Trousseau and Belloc have found no reason for separating phthisis laryngea from phthisis trachealis, as M. Cayol had done. The causes of laryngeal phthisis are various; a tuberculous or scrofulous constitution more particularly predisposes to it. The excessive use of mercury is considered by Mr. Lawrence, Mr. Porter, and Mr. Wood as a common predisponent cause; typhoid and debilitating diseases generally seem to have the same tendency. The excessive and constant use of ardent spirits may act as both a predisposing and exciting cause. Acute affections of the larynx, excessive exertion of the voice, catarrhs often recurring, repressed eruptions, blows, falls, wounds, chills, extraneous bodies introduced, may act as exciting causes. Four species of the disease are enumerated, simple, syphilitic, cancerous, and tuberculous. These are illustrated by numerous examples.

Of the symptoms, the earliest and most constant is a change of the voice. The degrees and kinds of hoarseness are very various. The stridulous variety generally implies a worse state than a mere loose or mucous hoarseness. In a great many patients the voice is very unequal, but patients by habit instinctively moderate these inequalities except on any unusual exertion of the voice when its cracked tones are very manifest. The hoarseness is liable to increase from various circumstances, particularly weakening causes and vocal exertions, and may sooner or later terminate in aphonia. When the loss of voice is gradual, the prognosis is most unfavorable; for this is generally the result of a progressive destruction of the vocal apparatus. The cough is short, frequent, and generally dry in the early stages. Later there is sometimes a very peculiar kind of loose *belching* cough, which is supposed to depend on imperfect closure of the glottis. In more than half the cases of ordinary laryngeal phthisis, there was no pain throughout the duration of the disease. Painful deglutition is often complained of; which MM. T. and B. ascribe to the circumstance that the aryteno-epiglottic ligaments, and

the arytenoid cartilages, which form the framework of the front of the pharynx, are covered by membranes which are very commonly the seat of inflammatory engorgement. At the same time the larynx may suffer outward pressure without pain, because the same parts are protected by the hyoid bone and thyroid cartilage. There is little within reach of inspection, but the epiglottis, and even this cannot often be seen: our authors have twice succeeded on making the patient utter cries during the examination. A speculum for the larynx is much needed. The crepitation felt on pressing the larynx, said by M. Laignelet and others to be a sign of laryngeal phthisis, MM. T. and B. find may be generally produced in the healthy larynx. Feeling the glottis, which has been recommended by some authors, is a thing impossible. To feel even the epiglottis is a matter of considerable difficulty; so violent are the convulsive efforts excited by the touch: so that except very palpable changes, little is to be learnt by feeling. The signs of respiration are next in value to those of the voice. As the disease advances, the patients are attacked in the night with paroxysms of dyspnœa, which leave them in the intervals only with their breathing short on exertion. These paroxysms become more severe, amounting to orthopnœa, and then the respiration in the interval is noisy. The disease advances; the frightfully aggravated paroxysms threaten imminent suffocation; and then the orthopnœa does not subside in the intervals. Death generally ensues in fifteen or twenty days after the commencement of the orthopnœa. Bayle and Thuillier have described the inspiration being much more difficult than the expiration, as an essential character of an œdematous state of the ligaments of the glottis. This MM. T. and B. have not found to be the fact. The gradual attacks of laryngeal paroxysms of dyspnœa and the manner in which the voice is affected by them, will serve to distinguish them from pulmonary asthma. MM. Trousseau and Belloc do not appear to have attempted to find any signs of laryngitis by auscultation. Dr. Stokes describes, as the stethoscopic signs of chronic laryngitis, a harshness in the laryngeal sounds of respiration, giving the idea of a rough surface; this is perceptible even when the breathing is not stridulous. In some cases, above the alæ of the thyroid cartilage there may be heard a rhonchus, like the sound of a small valve in action combined with a deep humming sound. It is sometimes perceptible only on one side; and always disappears as we descend to the bronchial tubes. When the obstruction is considerable and the respiration prolonged, a remarkable feature, mentioned by Dr. Stokes, is the feebleness of the vesicular murmur as compared with the violence of the inspiratory efforts. The feebleness is observed throughout the chest, and not, as in the case of the pressure of a tumour, on one side chiefly.

MM. Trousseau and Belloc fairly establish that laryngeal phthisis does occasionally arise, run a long course, and terminate fatally, without being complicated with any other organic lesion. They admit that these cases are rare, and that generally chronic laryngeal disease is secondary, and most commonly an addition to pulmonary phthisis. But they bring several well marked cases to prove that, not very rarely, the laryngeal affection is primitive, and pulmonary or other organic tubercular affection is ingrafted on it; these cases they think may also properly be called laryngeal phthisis; although they view them as the result of a tuberculous diathesis, now manifesting itself in one set of organs, now in another;

now long affecting the mucous membrane and Peyer's glands in the intestines, or the membranes of the larynx; now developing itself in the lungs and more rapidly destroying life. There is sometimes great difficulty in cases of laryngeal disease, in determining whether pulmonary disease also exist or not. The noisy laryngeal respiration, the absence of voice, the occasional presence of bronchitis, obscure the physical signs by which lesions are to be detected in the lungs; and these lesions may be of that kind which, even without these disguising circumstances, can be discovered only by the most experienced auscultators. When, however, the pulmonary disease is more advanced, the copious purulent expectoration, emaciation, and night sweats, which rarely attend simple laryngeal lesions, with dulness on percussion in some part of the chest, generally make the case pretty clear.

Simple laryngeal phthisis is in its milder forms by no means incurable. MM. Trousseau and Belloc give instances in which a temporary cure was effected even where subsequent tubercular disease in the lungs proved the existence of a scrofulous taint. Rest to the organ of the voice is quite indispensable, and these authors have found that the rest ensured by speaking only in a low whisper is sufficient. This is a far more practicable rule than that of absolute silence and it is quite reasonable, for the larynx is no more concerned in whispering than it is in breathing: it is the voice that constitutes its labour. And here we would remark that the large and extended vibrations of the male voice may constitute one cause of the much greater frequency of laryngeal disease in men than in women. Of depletive measures suited to the early stages of chronic laryngitis, MM. T. and B. give a preference to venesection; but other modes of bloodletting may be better suited to particular cases. Blisters, setons, and especially counter-irritation by tartar-emetic or caustic potass, in the neighbourhood of the larynx, or at the back of the neck, are of more permanent efficacy than bleeding; and are especially suited to the disease when established. When pain or cough is troublesome, it is proper to quiet them by narcotic applications; and frictions of extracts of stramonium and belladonna are recommended by our authors.

The most effective remedies, however, are those applied to the diseased part itself. A solution of nitrate of silver, in the proportion of from one to two parts dissolved in four parts of distilled water is the application which MM. T. and B. have employed with the greatest success. The solution may be applied to the epiglottis and upper part of the larynx by a small roll of paper bent towards its end and well moistened with the solution. A more effectual mode is with a small round piece of sponge firmly attached to a long piece of whalebone, bent at an inch from the sponge to an angle of 80°. The patient's mouth being opened wide, and the tongue pressed down with a spoon, the sponge is passed to the top of the pharynx; as soon as it reaches the fauces, a movement of deglutition takes place, which carries the larynx upwards, at which moment the sponge is brought forward and squeezed under the epiglottis, and the solution freely enters the larynx. Convulsions of cough, and sometimes vomiting ensue, but the application causes no pain. A less disagreeable mode of applying the solution, is by a small silver syringe with a tube five inches long bent at the end; this is filled to a fourth of its capacity with the solution, and the remaining three-fourths with air. The extre-

mity of the tube is carried beyond the epiglottis and the syringe, being discharged forcibly, throws the solution into the larynx in a finely divided rain, in consequence of the presence of air in the syringe. The patient is then made to swallow and rinse his mouth with a solution of salt or a muriatic acid lemonade to decompose the remains of the nitrate; and this precaution should be adopted in those cases also where this remedy is applied in substance. In six cases of aphonia with various degrees of "chronic laryngitis" this treatment proved successful; in the slighter cases, in the course of a few days: in more inveterate cases, where the disease had lasted for three or four years, the cauterization (as they term this application) had to be applied repeatedly during a month or more. It produced considerable improvement in three other cases, two of which proved afterwards to be tuberculous. It may be a matter of some doubt whether the aphonia in some of these cases at least be not the result of relaxation rather than inflammation, and the irregular menstruation of some of the patients also countenances the idea that it may be in some measure hysterical. However, this treatment succeeded where all kinds of other means had failed; and we are indebted to these authors for making it known and for the assurance that it may be safely employed in so ticklish a neighbourhood.

Another method of applying remedies to the diseased parts is by the insufflation of powders, as recommended by Aretæus for angina maligna, and by M. Bretonneau for diphtheritis. The method recommended by MM. Trousseau and Belloc is simple. The powder is put into one end of a reed or glass tube, two lines in diameter and eight or ten inches long, the other end being carried as far back as possible into the mouth, after a complete expiration the patient closes his mouth and makes a sudden forcible inspiration, in which the air carries some of the powder into the larynx and trachea. It excites cough which should be restrained as much as possible to prevent the too speedy expulsion of the medicament. This insufflation is to be repeated several times during the day according to the nature of the case. The powders employed in the various cases are—sugar; sub-nitrate of bismuth (which may be used pure;) calomel with twelve times its weight of sugar; red precipitate and the sulphates of zinc and copper with thirty-six times their weight of sugar; alum with twice its weight of sugar; acetate of lead with seven times, and nitrate of silver with twenty-four, thirty-six, or seventy-two times its weight of sugar. The powders ought to be made quite impalpable. The least roughness or perceptible fragment of a crystal excites such efforts of cough as ensure the expulsion of the powder. Of the powders the sub-nitrate of bismuth is the mildest; and may be used with safety and often with advantage in every form of chronic laryngitis, even in that accompanying pulmonary phthisis. Nitrate of silver is the most effectual in erythematous laryngitis, with erosions or even ulcerations. Calomel and red precipitate have proved beneficial in ulcerations whether syphilitic or not, but they may not be repeated at first more than twice or thrice in a week.

MM. Trousseau and Belloc have found repeated mild mercurial courses effectual in several cases of chronic laryngitis, not only those of syphilitic origin, but more decidedly in that kind. In this respect they follow in the track of British practitioners. In fact, their methods of treatment are far more active and like the practice of this country, than is usual in

France; and we should not be surprised to find that our volatile neighbours will soon outdo us in the boldness of their treatment. Laennec, with his large doses of tartar-emetic, and Bouillaud, with his bleedings "coup sur coup," have already led the way.

We return to our British authors. Mr. Porter and Mr. Ryland give good descriptions of the symptoms and history of croup; but as these do not present any new matter, we must pass them over. It is judiciously observed by Dr. Stokes that much confusion has arisen in consequence of authors not having distinguished between the two forms of croup, which he designates *primary* and *secondary*, which are diseases of a very different type and requiring quite distinct modes of treatment, yet, in consequence of having the same name, the most perplexing contradictions have arisen. In *primary*, or true inflammatory croup, the air passages are primarily affected and there is present an inflammatory symptomatic fever, requiring and sometimes cured by antiphlogistic means. In *secondary* croup, the *diphtherite* of Bretonneau, the laryngeal affection succeeds to disease of the pharynx or mouth; if fever be present it is generally of a typhoid character, requiring tonics, revulsives, and stimulants, instead of antiphlogistics. Primary croup is sporadic or endemic, but never contagious; it chiefly affects children. Secondary croup is always epidemic and (Dr. Stokes thinks) contagious, and often attacks adults. In primary croup the exudation of lymph spreads from below upwards, the pharynx being healthy, and deglutition generally easy. In secondary croup, the exudation spreads from the glottis downwards, the pharynx being affected, and severe dysphagia often present. In primary croup, catarrhal symptoms often precede the attack; pulmonary inflammation is commonly combined with it; and the breath is seldom fætid. In secondary croup, the laryngeal symptoms supervene without preceding catarrh; the lungs are seldom affected, and the breath has a characteristic fætor. The only pathological resemblance between these two affections, inflammatory croup and diphtheritis, is the secretion of plastic lymph or false membrane from the laryngeal mucous membrane.

Dr. Stokes thinks, in opposition to the opinion of Dr. Meriadec Laennec, that stethoscopic examination may be useful in true croup. In the early and more tractable stage of the disease, the breathing is not too stridulous to prevent us from hearing in the lung loud sonorous and mucous rhonchi diffused through the different orders of bronchial tubes, sometimes with peripneumonic crepitation with some dulness on percussion. The act of vomiting being often followed by a temporary suspension of the stridulous breathing, supplies an opportunity of hearing these signs more distinctly.

On the subject of treatment of croup our authors differ considerably. All recommend bleeding and antimonial emetics at the onset. Mr. Porter prefers venesection, especially jugular; and considers leeches highly objectionable. Mr. Ryland recommends leeches in the majority of cases, "because the disease more frequently occurs in weakly children." Dr. Stokes advocates general and local bleeding "as assistants to the principal remedy, which is the tartar emetic;" but objects to opening the jugular vein, lest the vomiting should afterwards burst the incision, and cause dangerous hemorrhage. Messrs. Ryland and Porter recommend tartarized antimony in an emetic dose only at first, the latter trusting chiefly to nauseating doses afterwards, and the former exclusively to

calomel. Dr. Stokes follows Dr. Cheyne in recommending doses to be given sufficient to produce vomiting at least once in every three quarters of an hour, and continued for several hours according to circumstances: the mercurial treatment he believes "to be insufficient and unnecessary." If we may be permitted to give a casting vote we would state our experience to be generally against bleeding further than by leeches, and this only at the first attack; and, although we trust more to tartar emetic in full doses (about a quarter of a grain every half hour) than to any other remedy, yet we have seen such depression rapidly produced by it, that we make it a rule to give calomel also, to the more durable action of which we entrust the case, when we are constrained to give up the tartar emetic. All agree, however, as to the inexpediency of attempting bronchotomy in croup. In the early stages of the disease there is too good a chance of the medical remedies being successful to warrant the interference of surgery; and by the time their efficacy begins to be doubtful, the condition of the bronchi and lungs, as well as the state of the system generally, is commonly beyond the reach of relief from tracheotomy. The few instances on record in which it has succeeded, appear to be quite exceptional cases. The same remark applies to the epidemic adult croup, or diphtheritis. The remedies most to be relied on in this affection, which appears to be a specific form of anginous inflammation, are mercury, and topical applications. "The topical applications on which reliance is chiefly to be placed are muriatic acid, nitrate of silver, and alum; and their mode of action seems to be the exciting of such a degree of common inflammation as shall supersede and set aside the special inflammation under which the patient is labouring. The safest of these, and the one which has effected the greatest number of cures, is alum: it may be reduced to a paste by admixture with water or honey, and it should then be applied to the inflamed surfaces by means of a small brush or the handle of a tea-spoon. Its effect is to detach the false membranes, and to repress the exuberance of the swelled and inflamed mucous surface. One application is seldom sufficient; it should be applied frequently till the albuminous exudations cease to present themselves, and till the parts are reduced to their natural colour and volume." (*Ryland*, p. 174.)

Mr. Ryland has an interesting chapter on "Spasm of the Glottis," which comprehends two different kinds of nervous affection, the *laryngismus stridulus* of Mason Good, referred by Dr. Hugh Ley to a paralysis of the inferior laryngeal nerves, generally from the pressure of glandular and other tumours, and spasm of the glottis from pressure on the larynx and trachea, or on their nerves. The last addition "*on their nerves*," destroys the pathological distinction between these two classes of cases; and it appears that Mr. R. arbitrarily applies the first exclusively to the crowing or croup-like inspiration of children; and the latter to similar affections in the adult as well as to obstructions of breathing from the pressure of tumours on the windpipe. Having recently noticed this subject we shall only observe that Mr. Ryland defends the views of Dr. Ley against the objections of Dr. Marshall Hall; and considers them to be confirmed and illustrated by the observations of Drs. Montgomery, Kopp, and Kirsch, on the affection of infants called by the latter authors thymic asthma.

Another class of nervous affections of the larynx are presented by those persons, generally females, who suddenly, on some strong mental

excitement or bodily impression, lose their voice, and sometimes as suddenly recover it. In other similar subjects, a violent convulsive cough comes on in paroxysms, sometimes accompanied with stridulous breathing and severe dyspnœa. Irregularities of menstruation, or the occasional presence of globus, hysteric convulsions, or some other indication of the protean malady, stamp some of these cases as belonging to the undefinable disease *hysteria*. But in other instances, there is no trace of any affection of the sexual function, or of the peculiar nervous symptoms that are commonly associated with it: but the symptom is a solitary nervous one. We know a lady not in the least hysterical, who for several years past has been subject to a loss not only of her voice, but even of the power of articulation, on any sudden impression on the body or mind, and the aphonia will continue for a period varying from a few hours to several days, and then cease as abruptly as it came on. For a long time an electric spark was efficient in restoring to this lady the use of her voice; now that fails; and swallowing a morsel of ice generally succeeds. The loss of voice is accompanied with a feeling of constriction in the chest and some oppression of the breathing, which is very indistinctly heard on auscultation. Hence the affection would appear to be spasmodic. Dr. Stokes judiciously remarks that spasmodic affections of the larynx most commonly occur in connexion with organic disease of the larynx or lung; but in a few cases, an affection at first spasmodic passes into one with structural change. He gives one case in which the patient, after many months of suffering from various hysteric and nervous symptoms ending with a convulsive cough which was temporarily relieved by antispasmodics, died soon after with symptoms of suffocation; the only disease found was an abscess of the size of a Spanish nut involving the cricoid cartilage. In two other cases death ensued in consequence of affections of the brain supervening on long continued convulsive cough. In a third case of frightful paroxysms of cough, acute hydrocephalus came on and was arrested by antiphlogistic treatment and mercury.

We have not space for any notice of the subjects of tumours and wounds in the larynx and trachea, which are fully discussed in the works of Mr. Porter and Mr. Ryland. The latter gives two plates shewing polypi of the larynx, taken from preparations in the museum of King's College, London.

The laryngitis from *swallowing boiling water* or *corrosive liquids*, and, as Mr. Ryland adds, *from the inhalation of flame*, is generally complicated with considerable injury to the mouth, fauces, and occasionally to other parts of the body; and the success of treatment, medical and surgical, will much depend on the freedom of the case from these complications. The access of the irritant to the larynx itself in these cases generally arises from the convulsive actions caused by the unexpected pain suddenly excited. A child, unconsciously going to suck water from the mouth of a boiling tea kettle, a person taking sulphuric acid in mistake, can scarcely actually swallow such bodies, but, in their violent efforts to reject it, enough passes under the epiglottis to produce the inflaming effects. Yet, Mr. Porter remarks, strong sulphuric acid is sometimes quietly swallowed by the determined suicide, without injuring the larynx at all, and, strange to say, without causing much pain. This we would explain by its property of coagulating the animal matter of the

mucus with which the passages are covered, and which thus affords a temporary shield against its corrosive effects. Mr. Ryland remarks, that one reason why tracheotomy has not been followed by success in many of the class of cases which we are considering is, that it has not been followed up by the means proper to subdue laryngeal and bronchial inflammation, which go on after the operation just as much as before it. The object of the operation is not to cure the disease but to supply air until the inflammation shall have been subdued by mercury and other remedies. In most of the recorded cases of recovery this treatment was persevered in after the operation. Mr. Ryland relates three cases which render it very probable that the inflammation affecting the air passages in some instances of burns, particularly from the upper clothes taking fire, is caused by the inhalation of the flame, or at least of very hot air. In these examples the face and neck were much burnt; and the inflammation in the air passages was most intense at their upper parts. We think, however, that the severe bronchitis, attending extensive burns and scalds also, is more commonly dependent on sympathetic or constitutional irritation; as we see the same inflammation accompany exanthematous and other severe fibrile disturbances, independently of any direct irritation of the bronchial membrane.

On the manner and result of foreign bodies getting into the air-passages, we quote the following general statement of Mr. Ryland, which is at once clear and concise.

“Preparatory to speaking or laughing, a full deep inspiration is necessary, and the air is very commonly drawn through the mouth; the edges of the glottis are at this moment widely separated from each other; the mind, interested in conversation, is off its guard; and therefore a particle of food, a plum-stone, or whatever the mouth contains, is easily taken with the current of air into the larynx or trachea. If the intruding body be of small size and light, a severe fit of coughing will, in all probability, soon drive it back again through the rima with considerable force; but, if it is heavy, broad, oblong, or of an irregular figure, it may become permanently fixed in the aperture or in the ventricles of the larynx; it may remain in the trachea; or it may fall into one of the bronchi. The external qualities of the extraneous body will therefore regulate, in a considerable degree, the situation which it will assume and maintain in the air-passages; and its situation will, in like manner, influence the nature of the symptoms that will result from its presence. The consequences of this accident may be, 1st, instant death, if the foreign body become impacted within the rima glottidis, or in the larynx immediately below this point;—2d, pain somewhere in the course of the trachea, severe dyspnoea, and frequent convulsive cough, followed by death, generally in three or four days after the accident, if the foreign body be small and light, and move with the air in respiration backwards and forwards along the trachea; and, 3d, symptoms resembling those of asthma and suffocative catarrh, which may terminate eventually in death, or in the expulsion of the irritating substance during a fit of coughing: in such cases the body is commonly heavy, and has fixed itself in one of the bronchi or at the entrance of some of their ramifications.” (P. 279.)

To detect the presence of a foreign body in the air-passages, auscultation may often afford a most valuable assistance. Its application to this class of accidents is chiefly due to Dr. Stokes, and several lives have been already saved by tracheotomy, performed under the guidance of the signs discovered by auscultation. The diagnosis is to be made rather by a comparison of the history of the case and of the collective and successive signs than from any one phenomenon in particular; and in this way we may generally arrive at a distinction between the effects of the pre-

sence of a foreign body in the air-passages and those of any disease that may resemble them. If a body be impacted firmly in the larynx, as in one of the ventricles, it will produce more or less violent and incessant attacks of cough and dyspnoea, in which auscultation finds the lungs to be sound and the larynx to be the seat of the constriction; the permanency of which, together with the history, will point out the nature of the case. When a foreign body is in the trachea, it generally gets into one of the chief bronchial tubes, (almost always the right, because this is larger, and lies more in the axis of the trachea;) and here will produce signs of obstruction and local irritation, which may be quite distinctive. When the body is smooth and roundish, as a plum-stone or bean, it may from time to time entirely block up the bronchus, and the consequent sudden and total absence of respiration on that side only, whilst the sound on percussion remains good, will pretty plainly shew the nature of the affection. The evidence becomes stronger when, on violent coughing, the air is suddenly heard again to enter the lung, and when the increase of laryngeal irritation implies that the body has been moved back into the trachea. The alternation of one set of these signs with the other, Dr. Stokes remarks, is not met with in any other case, and cannot fail to point out the source of the evil. When the foreign body is of an irregular shape, as a tooth or piece of bone, it will not completely block up the bronchus; but the constant irritation and secretion, with its accompanying rhonchus, which it produces there alternately with violent laryngeal cough when it gets upwards, together with the sudden occurrence of the symptoms with other features in the history of the case, will generally denote its nature. When the presence of the foreign body has been clearly made out, and sufficient trial given whether the natural forces can expel it, it should be removed by an operation without further delay; as its remaining in the ventricles, even although it do not obstruct the breathing, will not fail, sooner or later, to cause serious structural mischief, both in them and in the adjoining lung.

We have room only for a few more observations on the subject of bronchotomy, which we found to be indicated in several of the conditions of the air-passages which we have reviewed, and which experience has in many instances proved to be successful. Bronchotomy is the generic term, including laryngotomy, in which the opening is made in the membrane between the thyroid and cricoid cartilages; and tracheotomy, in which the trachea, with one or more of its rings, are cut. The former is the easier and safer operation, because there is little to cut through, and scarcely any risk of hemorrhage into the tube: it is, however, not effectual when the larynx is diseased, and the difficulty of keeping the aperture free, and of keeping a tube in it, on account of the extreme irritability of this part, render it ineligible, except in the more temporary cases of œdema or spasm of the glottis, and the like. Tracheotomy is not only more difficult to perform, on account of the depth of flesh to be cut through,—amounting, in cases of fat, œdema, or other swelling, to one or two inches,—but also more hazardous, from the risk of hemorrhage, which, taking place in the trachea, has in several instances caused instantaneous suffocation. The precautions necessary to prevent this and other untoward issues, and directions for the management of the operation, are fully given in the works of Messrs. Porter and

Ryland, to which we must refer our readers, especially young surgeons, who ought to be always prepared against an emergency in which any delay may be fatal, and where there will be no time to consult authorities. Mr. Porter is the more original writer, and speaks as one who has experienced the things which he describes, although not in the clearest language. Mr. Ryland has borrowed freely from him and from others, but he succeeds in placing his subjects distinctly and concisely before his readers; and, as we have before remarked, his work, although chiefly a compilation, is the most complete treatise that we have seen on the subject. There are a few physiological remarks in it which need revision: for instance, in the section to which we have just been adverting, he speaks of the left ventricle of the heart being paralyzed by the circulation of imperfectly arterialized blood in the brain; a notion, with regard to asphyxia, quite inconsistent with the best established views of the present day.

ART. VIII.

The Philosophy of Marriage, in its social, moral, and physical Relations; with an Account of the Diseases of the Genito-urinary Organs which impair or destroy the Reproductive Function, and induce a variety of Complaints; with the Physiology of Generation in the Vegetable and Animal Kingdoms: being Part of a Course of Obstetric Lectures delivered at the North London School of Medicine, Charlotte Street, Bloomsbury Square. By MICHAEL RYAN, M.D. &c. &c.—London, 1837. 8vo. pp. 364.

THIS work is offered, we observe, like several others noticed in this Number of our Review, for the instruction of the public at large; and we once thought of classing it with them: but it must stand by itself; it is in every respect unique. The physiognomy of its title is a true index to its inward character. The Philosophy of Marriage, being pure no-meaning, might have passed without remark; but the remainder of the title-page justifies the strongest condemnation. It is melancholy to see a physician and a man of information descend, from whatever reason, to cater such kind of reading for the ill-regulated minds of all who may choose to indulge in it; offending the eye of delicacy, like a quack-doctor on the wall. And, in the point of ingenuity of attractiveness, even the quack-doctor must yield the palm to Dr. Ryan, with his "account of the Diseases of the Genito-urinary Organs which impair or destroy the Reproductive Function." The work is divided into three parts: the first treats of Marriage, in its moral and social relations, and contains some useful information, intermixed with many topics that look as if they were purely meant to be exciting; the second speaks of the Physical relations of marriage, and may be similarly characterized; the third, which relates to the Morbid relations of marriage, comprehends all the subjects in surgery and midwifery which can offend and disgust, because the information relating to them, whilst it is not useful, pollutes the minds of those who seek it from curiosity, and without any view to its beneficial application. This portion of the work is, we suppose, that

alluded to in the title-page as being part of a course of Obstetric Lectures: it contains much that a lecturer would pass lightly over in such a course, even before his medical pupils, and much that, offered to the general public, constitutes an outrage against good manners which nothing can excuse. To attempt to palliate it by quoting Haller as introducing the subject of Generation by saying "there are no secrets in physiology," is as absurd as it would be to make the quotation an apology for walking about the streets of London without clothes, because there are no clothes in physiology, and man is born a naked animal. The author may class those who think as we do on this matter among the "prudish and the ignorant," or "the antiquated and hypocritical," whom he professes a wish to "reconcile" to the consideration of the subjects he dwells upon; and he may affect the air of a virtuous philosopher, while discoursing of the "health of parents and offspring," and the "improvement of morals and population:" but he ought to have been aware of the danger he was running of being confounded with those who, in times past, sought for reputation in the by-paths of literature. Their reward, in their own day, was aversion and contempt—not honour,

"And unhallowed they sleep in the crossways of fame."

We are greatly deceived if Dr. Ryan will not discover, ere long, that he has committed a grievous mistake, and that neither knowledge nor a facility of composition, nor even an unblemished personal character, can, in the present day, reconcile well-thinking persons to productions which obtrude on modest eyes all that civilized people agree to conceal with decent care.

We have spoken of Dr. Ryan's allusion to Haller in vindication of his own proceedings, although there is nothing in the pure and classic pages of that great physiologist to encourage such writings as the present. He speaks equally vaguely of Aristotle, Spallanzani, and Hunter; and, still evidently a little uneasy about the project he was commencing, says,

"The period has at length arrived when sexual diseases obtain as much attention as any other class of infirmities, and when the most distinguished medical practitioners devote themselves to their study and treatment. It is scarcely necessary to mention the works of John Hunter, Sir Astley Cooper, Sir Benjamin Brodie, Mr. Guthrie, and many others. *I go a step further*, and describe the moral and physical causes of impotence and sterility, with the physiology and pathology of the sexual organs." (P. 37.)

The mystification displayed in this passage requires no comment; and its motive is obvious: but, thus fortified, the author proceeds to say that "much error exists on the physical laws of reproduction among all classes of society, and especially in relation to the consummation of the marriage contract." He has the modesty to add, that "this requires to be exposed with all the delicacy of which the subject admits, and the precision necessary and privileged in medical works;" (p. 38,) and forthwith a kind of unacknowledged paraphrase follows of a passage in Armstrong's "*Economy of Love*," giving precise directions as to this delicate particular of consummation! Dr. Ryan might have learned that Armstrong's poem, though full of elegance, was too gross in its subjects for general readers half a century ago, and proved ruinous to the prospects of an experienced and accomplished physician.

The chapter on Premature and Late Marriages contains a few remarks that might be usefully conveyed to popular readers, but mingled with all the idle and fanciful opinions of the ancients,—merely, it would seem, to make up a show. We are told, for instance, that the ancient Germans entertained great respect “for the fair sex;” that a fine of fifteen shillings was levied for baring the arm of a free woman without her consent; that, if her bosom was touched, the fine was forty shillings; and, if a kiss was taken, the punishment was exile: and all this barbarous practice seems to be mentioned with commendation; and we are also favoured with the information that, in the spring of the year 1837, a man prosecuted a woman at the Surrey sessions for biting his nose off; the defence being that he had given the woman a kiss; and that “the judge” said she was justified in biting it off, and would have been justified “in eating it.” We can scarcely believe that any judge could disgrace himself by an observation only more disgusting than it is unjust and at variance with all social principles; an observation of such brutal import that it would equally justify murder or any other passionate outrage. But Dr. Ryan’s excessive devotedness to the sex makes him admire this ferocious dictum.

When speaking of late marriages, Dr. Ryan informs us that it is “a matter of observation that young women bear no children when united to old men,” although observation shews quite the contrary; and in a previous page Dr. Ryan himself seems so struck with the late vigour of men as to be quite ready to affirm, in any disputed case, that there is no physical reason why women also should not propagate at sixty. (P. 34.)

Polygamy and Monogamy are discoursed of with much array of learning. Dr. Ryan quotes Diodorus Siculus, and Xenophon, and Dion Prusæus, to prove that the marriage of brothers and sisters has no good effects: he also adverts to what was said by “Hermione, in his play of Euripides,” (a mistake in copying, doubtless;) also to Plutarch, St. Augustin, Vandermonde, Pallas, and Buffon, but without references to passages; and all to prove, what nobody now disputes, that the crossing of breeds is advantageous. We are likewise told at full length the manner of the Grand Seignior’s proceeding with a virgin, when allowed one on solemn festivals; and Lady Wortley Montague’s account of the eastern mode of dancing is introduced as something piquant on a dull subject: and in this way Dr. Ryan goes on, page after page.

In a chapter on Conjugal and Parental Rights and Obligations, the author dwells at much length on the “conjugal debt,” in his peculiar manner; stating the circumstances in which it is to be paid or refused. In what relates to children, he pronounces it barbarous to strike a child under seven years of age, as if striking after that age was quite allowable and proper. He quotes numerous authorities *against* the necessity of fidelity in men,—Samuel Johnson, Napoleon, and Luther;—of course, only to refute them. These instructive remarks precede a very attractive chapter on the Uses of the Sexual Organs; wherein he quotes the expressions of Buffon, “we must describe the history of this age (puberty) with great circumspection, so as not to excite in the imagination other than philosophical ideas:” and he immediately adds, “*we must treat it in the pure and unsophisticated state of nature*; that is to say, physiologically.” What Dr. Ryan means by physiologically, we shall presently

see; but he is evidently too pure, natural, and unsophisticated to dread raising other ideas in the imagination than philosophical ones. He describes the meetings of lovers, their sidelong glances, and the pressure of trembling hands; and at length, he says, "they finally vow to taste legitimate pleasure;" but he very prudently adds, "after swearing eternal fidelity to each other before the altar." He proceeds to give very good advice to young ladies. After some coquetry with the subject of copulation, (p. 135,) he grows shy of it, but very kindly refers the reader, as on many other occasions, to his other works for more precise information, (p. 137.) Not, however, to leave them to burst in ignorance, he tells them why certain organs are called "parts," and why "secret parts;" and adds, with beautiful simplicity, "the unthinking part of mankind consider allusions to these organs as indelicate," (p. 138.) Then he determines to explain coition, in a very matter-of-fact manner; then softens and meanders away into sentimentality again, about the "delights of a conversation full of tenderness;" and then, bolder grown once more, declares his full belief that woman has "a predilection" for a fine figure, a broad chest, a luxuriant growth of hair, and "eyes full of fire," (p. 139.)

In a chapter considerably devoted to unveiling the Causes which influence Fecundity, the author thinks it useful to mention that "many examples" are recorded of married and unmarried women being impregnated when in a profound sleep; and, speaking of antipathy or repugnance as causes opposing impregnation, beautifully remarks "that that which commences with apathy generally finishes with love, when the transport of pleasure ravishes the will." (P. 144.) But in this chapter he particularly lays himself out to be attractive. He enters into the highly useful question of the comparative degree of enjoyment of the sexes in the procreative act; and awards the superiority, for reasons satisfactory to his own mind, although purely fanciful, to the female. With his usual mindfulness of learning, he quotes Plazonus to prove "*unam mulierem pluribus viris sufficere*," (p. 152); and he accounts for this supposed fact by an hypothesis equally doubtful. For the instruction of the public in general, he makes some allusion to the subjects of nymphomania and satyriasis, and does not forget to mention cantharides. (P. 154.) To relieve the severe student, he relates the adventure of Proculus, a Roman general, who, having taken an hundred Sarmatian virgins prisoners, writes to a friend "*ex his una nocte decem inivi*;" and thinking this not enough, he adds the notorious testimony of a lying madman to an achievement of double extent. But then the illustrious Haller says "there are no secrets in physiology."

A chapter, more than half made up of quotations, is devoted to detailing many fanciful opinions concerning the procreation of boys or of girls at will; and another to recounting all the absurd notions that were ever entertained concerning generation; and thus the book swells pleasantly onwards towards a remunerating magnitude. Three more chapters are, however, consigned to the subject of generation, and are interlarded with anecdotes for the instruction of the public mind, very conveniently printed, we conjecture, from the author's lecture-book, as well as the remarks on Pregnancy and Delivery, added with the laudable intention,

we suppose, of making every woman her own midwife. But all this supplies materials for 104 more pages.

Yet, this great design being so far accomplished, its candid author, who keeps no secrets, seems to have suffered some solicitude as to whether the book contained all that its predestined readers would require; and the result is, that we have a Third Part, consisting of three chapters, of which the subjects are the pathology of the generative organs; a history of the venereal disease, and an account of urethritis, blennorrhagia, and gonorrhœa. Never before was seen such enthusiasm for the diffusion of useful knowledge among maids, wives, and widows; whose ideas of men, by such a course of instructive reading, must be wonderfully corrected if not purified; and who, thus accomplished, cannot fail to become as interesting to men as men must ever after be in their eyes. The false glosses of poetry, the decent sanctity of prudent concealment, vanish before this rigid declarer of all the secret operations of nature, who, while he thus holds up to view the pleasures and the pains of physical love, adds yet another hundred pages to the *Philosophy of Marriage*.

When commencing what he calls the pathology of the generative organs, he repeats for the twentieth time that "the human female is unfit to co-operate in the function of procreation until after the twelfth or fourteenth year in this climate," for which he gives, as is his wont, all the reasons, without disguise; adding, in his own manner, "*hence the cruelty and barbarity of violating female children of a tender age*" (p. 298); as if the age constituted the crime. He then enters upon the promising field of Impotence, where, of course, he freely expatiates; collecting from every possible authority everything capable of exciting and gratifying the curiosity of the reader. In this, as in all the other portions of his work, there is scarcely an original syllable: the work would be worthless to medical men on this account; for they all know these odd stories as well as Dr. Ryan. The novelty is in all this being laid so bare to the general eye; for what object the compiler best knows. One instance of his modesty, however, we will not conceal. After revelling in stories of impotence through thirteen pages, he says, "it would be contrary to the national taste and propriety to give a detail of the numerous diseases caused by the *abuse* of the reproductive function" (p. 311); and then goes on to enumerate them, and fills a whole page with evils greater than Thersites ever collected into an evil wish, including "incarceration of the penis in foreign bodies:" and he says he might give *several* illustrations of *each* of the hundred affections he mentions, so that his reserve and modesty on this occasion are too singular to be passed over. After this self-denial, Dr. Ryan gives the reader seven and twenty more pages full of further capital stories about impotence!

The most edifying thing of all is, that, after finishing this ill-arranged, offensive, and very indifferently written work, the philosophic author concludes it like a sermon. "I now submit these pages," he says, "the result of many years' study, reflection, and observation, to an enlightened age; impressed with the conviction that they are calculated to correct much error, and with sanguine hopes that they may benefit the present and succeeding generations." This is indeed admirable, but only for its unfitness as the conclusion of such a book, in the production of which Dr. Ryan has lamentably forgotten what is due to the public, to the profession, and to himself.

ART. IX.

Hygiène Publique; ou, Mémoires sur les Questions les plus importantes de la Hygiène appliquée aux Professions et aux Travaux d'Utilité publique. Par A. J. B. PARENT-DUCHATELET, &c. &c. Accompagné de 48 Planches. Précédé d'une Notice historique sur la Vie et les Ouvrages de l'Auteur, par FR. LEURET. Deux Tomes.—Paris, 1836. 8vo. pp. 552 et 708.

Public Hygiène; or, Memoirs on the most important Questions of Hygiène applied to Professions and to Works of Public Utility. By A. J. B. PARENT-DUCHATELET, &c. &c. With 48 Plates. Preceded by an historical Notice of the Life and Works of the Author, by FR. LEURET. Two Volumes.

THIS large work is a monument of the extraordinary industry of its author in a department of observation which, although of immeasurable importance to the community, has never presented a very inviting aspect in this country, where it has, indeed, attracted very little attention. What has been called Political or State Medicine, but is now more commonly denominated Public Hygiène, although it relates to no less important a matter than the preservation of the health of the whole population, in all varieties of residence and occupation, is scarcely recognized as a study. Those who labour in unwholesome manufactures die in rapid succession, and no man cares, for workmen are never wanting. Neighbours quarrel about the site and erection of gas-works, and of tall chimneys, and juries are puzzled by the contradictions of medical witnesses, and property is depreciated or not as the accident of the verdict may determine. Barracks or camps are fixed in deadly spots, and the military hospital is filled; but great expense has been incurred, and the evil must be endured. Bad food and drink are retailed in great quantities, with perfect impunity except to the purchasers. Epidemics appear and disappear, here fatal, there harmless, and the lessons they teach are not collected for public benefit; and crowded and unventilated courts spring up in and around every street. The laws of quarantine, considered useless and absurd by some, are regarded as vital safeguards of our shores by others. Of several towns, the citizens are in the utmost state of habitual uncertainty respecting the goodness or injuriousness of the water used for daily drink. These, and numerous other public inconveniencies unnecessary to mention, are felt from year to year, and little or nothing is done to promote the kind of knowledge which might lead to their removal.

On the greater number of these points it may appear ungracious to say, what is nevertheless strictly true, that the opinions of medical witnesses are little to be relied upon, and that their general authority is unworthy of confidence; in proof of which it is only necessary to refer to the question of the salubrity of the Thames water, so strongly agitated ten years ago, and to the more recent question of the safety of riding through rail-road tunnels. Strong assertions, resting on unstable doctrines, may there be found in abundance; but of careful observation and wise experience there are few traces. Many of the witnesses seem to have been actuated by a love of almost theatrical display, and to have de-

lighted in giving more force to popular prejudices and apprehensions; but teachers of valuable truths, important to the general health of the people, we find none. The evidence given during the enquiry made into the effects of certain parts of the factory system, although containing more information, presents equally glaring and disgraceful contradictions. Medical men, and the public in general, seem to come to the examination of these questions quite unprepared. Passion and various local influences, and many prejudices, mislead them; and they arrive at and announce no general principles that can be depended upon. Nothing can be so positive, and apparently so satisfactory, as the assertions made on either side, except the assertions made on the other.

Much of this would, we think, be obviated by making public *hygiène* a subject of more public interest; and no subject can deserve it more. "I have travelled," says M. Fodéré, one of the first among the French to shew the value of this study; "I have lived in different countries; I have reflected on the condition of man in the different circumstances of life; I have seen that it is in the power of governments to do them infinitely more good than all the books of medicine put together."—"There is a great department in the science of government," says Dr. Kilgour, who quotes this observation; "the application of legislation to relieve the physical necessities, extend the physical comforts, and improve the physical condition of the human race: in short, to make the public healthy, vigorous, and virtuous, in so far as moral perfection depends upon, or is connected with, the healthy condition of the body." Dr. Hawkins, in his writings on Statistics, has amply shewn the connexion existing between misery and disease; and that, wherever want and misery abound, there sickness, and miscarriages, and early death, and every indication of a degenerated people, are to be met with; but, whatever progress these principles may have made in the minds of intelligent people, an inspection of any of our towns will shew how much they are lost sight of. Large portions of the community of our highly civilized country are continually exposed, and perhaps needlessly, to causes which inevitably tend to make life wretched, and to shorten, first its efficacy, and then its duration.

The city of Paris has for nearly forty years enjoyed the advantage of a council of health (*conseil de salubrité*;) which at first consisted of only four members, whose duty it was to examine adulterated liquors, unwholesome workshops and manufactories, and the epidemic diseases of animals: they were subsequently charged with the visiting of prisons and the direction of public charities. It was afterwards found necessary to increase the number of its members to seven, and to enlarge the sphere of their labours. The treatment of epidemics was confided to them; and they were appointed to inspect the markets, rivers, cemeteries, slaughter-houses, dissecting-rooms, privies, sewers and collections of manure and filth, drains, pits, public baths, dépôts of mineral waters, &c.; they were also to attend to medical statistics and the tables of mortality; to institute enquiries concerning the means of rendering workshops and public places wholesome: to take measures for preventing inundations, or repairing their effects; to *repress quackery*; to determine the best methods of warming and lighting, &c. &c. &c. These manifold offices led to the drawing up, in the course of fifteen years, of no fewer than

4,330 reports. The institution is said to have obtained the confidence of the public, and to have been usefully imitated in other countries, and in some of the departments.

M. Parent-Duchatelet well observes, that an ordinary medical education by no means forms a sufficient preparation for the duties of a member of a council of this kind. They demand a great deal of knowledge not to be acquired in the medical lecture-room or in the hospital, and, in particular, a knowledge of general physics seldom possessed by the medical practitioner, and a practical acquaintance with workshops and various other localities. In a country like England, where no such council exists, and where the practice of the medical profession is lucrative, it cannot be expected that many students will devote themselves to such complicated and difficult enquiries. But, as the welfare of the community imperiously demands that attention should be given to the subject, in all its details, we should be most glad to see the institution of a Board, the duties of which would be attended with a sufficient remuneration to induce medical men to pay more regard to hygienic studies. The number so doing would, we feel convinced, soon be considerable.

A great defect, however, exists in this country, in the omission, in any curriculum of study, of the subject of *hygiène* altogether. The chair of *Hygiène* in the Faculty of Medicine in Paris has been filled by distinguished men; among others, by Andral; and valuable works have been published on the subject. In America, also, *hygiène* receives much consideration, and professors are appointed to teach it in some of the colleges. We cannot but be of opinion that great advantage would accrue to the public from some encouragement being given to the study; and nothing would tend so much to the removal of the too-general want of information concerning public *hygiène* as making it one of the subjects to be included in the studies of every physician and surgeon. Its general principles might, at least, be correctly learned by every practitioner; although its ample details might only be embraced by a few, who would thereby qualify themselves for important public duties. We trust we are correctly informed that this is likely to be one of the benefits conferred upon the English community by the Senate of the Metropolitan University.

Our previous notice of M. Parent-Duchatelet's work, on Prostitution in the City of Paris, must have made our readers well acquainted with the nature of the patient and diligent enquiries to which he devoted his life. Of these, the two volumes before us are an extraordinary monument. They comprehend twenty-nine sections, of which the subjects are generally of much importance, and, although repulsive to many enquirers, to the elucidation of which the author applied himself with all the enthusiasm that might have been expected in more attractive investigations. Of grave and studious habits from his youth, and after a careful education, he took the degree of doctor of medicine in Paris, in 1814, and was for a few years engaged in practice. In 1821, he published, conjointly with M. Martinet, a treatise on *Arachnitis*. He appears, however, to have conceived some disgust for medicine, in consequence of its uncertainty as a science; and, from 1821 to 1836, he gave himself wholly up to the prosecution of hygienic enquiries; of which the results are the

volumes which now occupy our attention and those reviewed in our Number for July, 1837. To whatever subject his enquiry was directed, he brought the same untiring industry and the same admirable method by which his researches concerning prostitution are characterized. He had even conceived a sort of fondness for situations and occupations which would be revolting to ordinary persons. In the midst of a fête at the Hôtel de Ville, burthened with a court dress, and embarrassed amidst a crowd of frivolous people, he whispered to a friend that he would rather go into a common sewer than into such a company. But in such a locality it was, even in a common sewer, that he had achieved some glory, or at least had essentially contributed to the public advantage. The Amelot sewer, one of the largest which flows beneath the Parisian capital, had, from long neglect, become totally obstructed. The consequences to those aboveground were hideous: cellars, houses, and streets were subjected to an obscene inundation. All cure of the evil seemed vain; many workmen were asphyxiated in the attempt, and the Amelot sewer became the dread of nightmen. It was thought that the remedying of the old sewer must be abandoned, and a new one constructed at an enormous expense. But the prefect of police appointed a commission, to undertake and direct, if possible, the renovation and purification of the sewer, with safety to the public health and also to the workmen. Of this commission Parent-Duchatelet was one; and under his direction this great and difficult work was undertaken, with so many precautions, and with so much care of the workmen employed, that, in the space of six months, the Amelot sewer and its branches were cleared of 2,150 cart-loads (*tombereaux*) of solid matter, and thrice the quantity of matters soft or semi-liquid; and, at the end of the task, the workmen were all in perfect health, and the whole expense incurred was only about 33,000 francs, (1,320*l.*)

Feeling all the importance of subjects of this kind, M. Parent remonstrates with the happy people enjoying themselves aboveground in Paris, concerning their small regard for those who toil in the rivers "dark and deep" below the soil,—those, without whose labours the air of the surface would soon become poisonous and the city a desert, and yet who are exposed to be carried away by the black and noisome currents which flow in that "sad Acheron," or suffocated by irrespirable gases, with no one to appreciate their useful labours or care for their fate. He reminds his readers that, among the ancients, the public salubrity was considered a matter of the first importance; that the Roman drains, among the wonders of antiquity, were under the protection of tutelary deities,—Sterquilinus, and Cloacina, and Mephitina; and that, among the Greeks, Epaminondas, and, among the Romans, Cicero and Agrippa, were superintendents of these essential structures. In the investigation of the drains of Paris, M. Parent visited them all, enquired of the workmen, compared their evidence, and spared neither time, nor money, nor labour, to attain the knowledge which he sought.

In all his investigations, disregarding mere conjectures and prepossessions, paying especial attention to facts, and applying to them, what in this instance was legitimately applied, the numerical method, he frequently arrived at conclusions opposed to all ancient prejudices. We pointed out some instances of this in noticing his work on Prostitution;

and M. Leuret mentions another, which very well illustrates such results in relation to the actual influence of tobacco on the workmen employed in the different preparations undergone by this plant. The opinion of all authors who had spoken of its influence upon health was, that it was especially hurtful. Ramazzini, and after him Fourcroy, and later, Cadet-Gassicourt, Tourtelle, Percy, Patissier, and Merat had given the same testimony. Tobacco was accused of making the workmen emaciated, sallow, asthmatic, liable to colic, vomiting, intestinal hemorrhage, vertigo, headach, tremors, narcotism. Of all these crimes, hardly thought of in King James's "Counterblast," M. Parent, in conjunction with M. d'Arcet, acquitted tobacco, after the most diligent examination; although we do not think they have succeeded in freeing the plant from all suspicion of guilt. Numerous valuable facts, connected with this and other subjects, are recorded by M. Parent, in the "*Annales d'Hygiène publique et de Médecine légale*," of which useful publication he was one of the founders. Among these, the results of his investigation of the effects of water in which hemp has been steeped, may be mentioned as worthy of remark. The fountains of the town of Mans are supplied by water in which this process has been carried on; and a commission was appointed by the minister of the interior to examine into certain alleged inconveniences resulting from it; which inconveniences they pronounced to be imaginary. A similar opinion had previously been given by M. Marc, concerning the flax-ponds of Gatteville. To this enquiry, relative to a subject on which much difference of opinion was entertained, Parent zealously devoted himself for about two years; and he came to the conclusion that water in which hemp has been steeped is *not* hurtful to the health of those who drink it; nor more hurtful to fish than water which has been employed for other vegetable macerations; that it is not narcotic; and that air charged with emanations from hemp is not improper for respiration. His experiments in confirmation of these opinions were extended to himself, his wife, and his family, who, as well as others, drank flax-water, and slept in rooms with damp flax, &c., with perfect impunity.

The first of the thirty-nine sections of M. Parent's book consists of Considerations on the Council of Salubrity of Paris; the second relates to certain medical prejudices hostile to the salubrity of cities and manufactories; the third is occupied with the plan of hospitals for old and infirm persons; the fourth treats of methods of respiring deleterious gases with impunity; the fifth, sixth, and seventh relate to the healthiness of the city of Paris, as affected by the river Bièvre or Gobelins, and by the cloacæ or drains, including an account of the processes employed to cleanse the great Amelot drain. The eighth has for its subject the influence of feculent and marshy emanations on the public health; the ninth describes the Artesian pits employed for carrying off impure waters; the tenth contains an enquiry into the influence of dissecting rooms, and the means of purifying them. Every one of these sections is full of curious particulars; but we can only very briefly notice a few of them, or of the other memoirs; and we shall confine ourselves, as usual with us when noticing works comprehending so great a variety of topics, to those chiefly interesting to medical men, although there is a difficulty of selection in regard to the present work, since every section has some bearing

on the means of promoting public health. From the general subjects, however, of the public health, M. Parent's observations, in his first section, are calculated to deter every practising physician; since, according to his views, it requires a variety of information which he cannot always devote himself to obtaining. To possess extensive medical literature, to be an excellent physician at the bedside, or an eloquent and able professor, do not alone form qualifications, he says, for one of the Council of Salubrity of Paris. A considerable acquaintance with the general physical character and constitution of the soil of the neighbourhood is necessary, and even of the geology of the neighbouring districts: above all, it is requisite to know the precise effects of professions and trades on the health of those engaged in them, and also of manufactories, &c. on the health of men collected in cities, and on animals; and this kind of knowledge cannot be acquired in the study, nor without exact notions concerning the arts and the greater number of particular processes of each trade. This kind of knowledge is not to be expected from mere physicians, but rather from those who have made public *hygiène* a special study, and from manufacturing chemists. Men so accomplished are, we fear, only to be expected in countries where councils or commissions are supported by the government, to pay especial attention to such subjects.

The section relative to the inconveniences of dissecting rooms, and the means of preventing them, is interesting to all who value the study of anatomy. It begins with an historical review of the Parisian school of practical anatomy; in which we are reminded that the first cultivators of this science in Paris had to encounter formidable obstacles, created by the ignorance and prejudice of the age, and were driven to pursue those investigations by stealth and in darkness, of which the results were discoveries that yet excite our admiration. In order to put a skeleton together, Vesalius passed whole nights in the charnel-houses of the cemetery of the Innocents, with much personal risk; and the same great anatomist laid the foundations of anatomical science, by resorting to the gibbets at Montfaucon, and disputing for the remains of criminals with vultures and animals of prey. This, we presume, refers to that period of the life of this most remarkable man when he studied under Sylvius at Paris, with so much ardour as to endanger his life; at a time when, no doubt, he was laying the foundations of the brilliant lectures on anatomy which afterwards attracted so many disciples to his theatre at Padua. But his life was not destined to be tranquil; and, having been to Palestine, for some unknown reason, either for the fulfilment of some vow, or, according to some authorities, to escape the indignation of the inquisition, excited by his having dissected a Spanish grandee, who proved not to be quite dead, he was, on his return from his pilgrimage, driven by a tempest upon the island of Zante, where he perished; so that, whether we regard his first stolen labours in Paris, when anatomy was considered unlawful and impious, or the supposed cause of his pilgrimage, and therefore of his death, he may be considered as having been an especial martyr to the science in connexion with which his name acquired immortality.

At a later period, at the beginning of the seventeenth century, when anatomy was protected in Paris by authority, the bodies of criminals

alone were given for dissection. These were consigned to the physicians, between whom and the surgeons there was great hostility. But the surgeons wished also to teach anatomy, and, as bodies were scarce, they arrayed themselves against the physicians, and took subjects from them by force, until prevented by an act of the Parisian parliament. Still, for some time afterwards, whenever there was an execution in the Place de Grève, the students fought with swords and pistols for the body of the dead.

At the period when these strange scenes occurred, the common people often took the part of the students against the officers of justice; but, after the commencement of the eighteenth century, when the bodies of persons dying in the hospitals began occasionally to be dissected, the anatomical pupils lost the popular favour; and they have never regained it. The dissections, indeed, were yet conducted with many of the disadvantages attached to unlawful proceedings. Regular dissecting rooms there were none, and the students either pursued their researches in their own rooms, up many pairs of stairs, or in lofts, and granaries, and sheds, on portions of the human bodies furtively obtained. Gravediggers were in league with them, to baffle the churchwardens and despoil the graves; and no proceedings were taken against them, except when they were unlucky enough to be found out in some glaring act of resurrection. In M. Parent's researches into antiquarian collections of manure and filth, closed at the beginning of the reign of Louis XVth, the frequent occurrence of human bones, and particularly of skulls, to which the saw had evidently been applied, furnished curious testimony to this part of dissecting-room history. It was not until the era of Desault, after the middle of the last century, that these clandestine proceedings became unnecessary. That celebrated surgeon established a large dissecting amphitheatre, in which there were generally not fewer than fifty or sixty bodies. Pelletan, Dubois, Lallemand, Boyer, Bichât, his contemporaries or the pupils of this more brilliant period, were led to imitate his exertions; hiring the upper stories of houses in situations little sought after, and establishing schools of practical anatomy. Bodies were supplied by depredations cleverly made on the churchyards, and were transported in hackney coaches: the remains were burnt; and, although the citizens in the neighbourhood were grievously incommoded by bad smells, the police were so careless of these proceedings that Professor Lallemand assured M. Parent they might at that time have killed as many people as they wanted, and have destroyed their remains in perfect security. It was not until after the commencement of the present century, in 1803, that the rooms of the dissectors were regularly placed under the superintendence of the police. Among the first regulations was one enjoining daily Guytonian fumigations, and another forbidding the dissection of those dying of contagious diseases. A later ordonnance enforced the fixing up of outside canvass blinds, as it was found that the windows commanding a view of what was going on in the dissecting rooms were for ever filled with women and girls, to the great interruption of domestic industry. The police found the dissectors unmanageable and contumacious. Nothing could exceed the filth, the neglect, and the indecency with which anatomical operations were carried on; and students, and dissecting-room porters, delighted to baffle and set at defiance

the agents of justice, who, half afraid to enter these dens of every horror, complained that the living were scarcely more respected than the dead. It required the vigour of Napoleon, aided by the experience of Dupuytren, to restrain these disorders; and the subject of the centralization of the dissecting rooms was agitated from 1806 to 1810, when a plan for a new arrangement was proposed, comprehending a central institution on a very complete scale; but the expense, 884,534 francs, (above 35,000*l.*,) and probably other circumstances, prevented its adoption.

About this time the police discovered and put an end to a singular trade, which had long been carried on between the dissecting-room porters and sundry unsuspecting tradespeople and artisans of the good city of Paris; no other, in short, than the sale of melted human fat. This preparation was sometimes sold for the simple purpose of greasing the wheels of carts; but it was also bought by quack doctors, who sold it as a salve sovereign for aches and pains; and in much greater quantities by enamellers and workers in pearl; the first of whom purchased it for combustion when a great heat was required, thinking it no other than dog-grease or horse-grease. The trade was conducted on a great scale, and the dépôt of the dissecting-room porters required a cart and two horses and three labourers for its removal. At the marriage of Napoleon with Maria-Louisa, the illumination-lamps of the Faculty of Medicine, and, less appropriately, of the palace of the Luxembourg, were supplied with a mixture of this notable product and tallow.

In order to check the numerous evils originating in private dissecting rooms, an ordonnance, dated in 1813, suppressed them all, and enjoined that dissections should be carried on in the amphitheatres of the Faculty of Medicine and the hospital of La Pitié alone: even the practice of operations on the dead body was forbidden, except in these authorized localities; but the rigorous conditions of this ordonnance were mitigated in 1814, at least in favour of the Bicêtre, to which M. Parent had been appointed physician. Much contention took place between the hospitals and the Faculty of Medicine, respecting the allotment of bodies. The regulation was, that the Faculty should have four-fifths, and the hospitals the remainder; but a more equitable arrangement was made in 1815, and has proved, we believe, generally satisfactory. Besides the amphitheatres of the Faculty and La Pitié, dissecting rooms for the use of the pupils of several of the hospitals are permitted to exist. The dissecting rooms of the Faculty are appropriated to those taking inscriptions, of whom, in the winter, there are about six hundred. The students from Great Britain and Ireland resort in great numbers to the dissecting rooms at La Pitié. For the rooms at the Faculty, a thousand bodies are required annually; and for La Pitié twelve or fourteen hundred.

With the intention of vindicating the practice of dissection from some of the charges brought against it as regards its effects on the health of the students, M. Parent enters into several curious particulars relative to the students of Paris, not unworthy, we think, of attention. He alludes to the well known fact that the majority of those who visit the French capital are soon attacked with diarrhœa and other inconveniences, in disproof of such affections being peculiar to the dissecting rooms. He is even disposed to deny the bad effects of the dissecting room emanations altogether. Although it is certain that either the air, or the diet, or the

water, or the wine of Paris, produces violent temporary disorder of the bowels in many of the visitors soon after their arrival, it is no less certain that the same kind of disorder is produced in some susceptible individuals whenever they pass a few days in a dissecting room; and that the effects are only put an end to by absence, being again and again produced by a return to dissection. However, M. Parent affirms that not one in a hundred is affected with severe illness during the course of anatomical study, and his affirmation, we may rest assured, is made on calculations. The greater number of Parisian students he describes as poor; many of them living almost entirely on bread, sometimes with the addition of a little brandy; the clamours, indeed, of many of them who live a little better, when the dinner hour brings them together at the restaurateurs in the neighbourhood, indicate a degree of hunger as well as of ferocity, which, added to the suspicious viands brought forth to allay the excitement, make up a scene as repulsive as can well be imagined. But if these causes are not sufficient to disturb the health of these young followers of science, the coldness and dampness of the dissecting rooms; the long confinement to one position, and the mental anxiety, may well be supposed to do it. Some of the students also pass many hours of the day in the hospitals, the air of which, whatever it may do for the sick, has notoriously bad effects on those who are in tolerable health. The most frequent maladies among the students, M. Parent says, are cerebral and gastro-intestinal maladies, by which he means fevers with cerebral or intestinal complication. Phthisis is also common. But all these affections are met with in the schools and colleges, particularly in students whose curriculum is nearly completed. If, two or three months before the end of the session, several medical students are obliged to desist from their labours and go home, it is also found that of 130 young men at the seminary of St. Sulpice, twelve or fifteen are affected in the same manner every year. In addition to these observations, M. Parent quotes against the specific unhealthiness of dissecting rooms the unquestionable testimony of Boyer, Dumeril, Dupuytren, Serres, Lallemand, Dubois, Ribes, Roux, Beauchêne, Jadelot, Breschet, and Andral. Independently of this evidence of the innocuousness of human dissections, M. Rousseau, the chief preserver in the anatomical department of the Museum of Natural History, states that he has been occupied there for thirty-six years; that they often dissect large animals, lions, bears, camels, elephants, and that the bodies are kept a fortnight or three weeks in the hottest weather; that the dissectors work all the day, and stop not for putrefaction, though it distends, bloats, *greens*, and detaches the hair of the dead animals; and that, notwithstanding all this, and the penetration of bad smells into their houses, he and his colleagues, as well as his father for forty years before him, have enjoyed good health.

It is unnecessary to quote the numerous other instances given by M. Parent of uninterrupted health enjoyed by persons who may be said almost to have passed their lives within the dissecting-room atmosphere; for we believe little doubt is now entertained concerning such facts; and it is well known that several of the most distinguished pathological anatomists have lived to a good old age. The consideration of the results of wounds received in dissection did not form part of M. Parent's plan; such results not being to be ascribed to peculiar emanations. He enters

fully into the subject of the means of keeping the dissecting rooms clean and wholesome, by ventilation, &c., although if we are to admit all the previous testimony to the fullest extent, such means would be more conducive to comfort than really important to health.

It was our intention to notice some other portions of M. Parent's work; but of the numerous details contained in the several memoirs which make up the two bulky volumes, few appear on examination to be unknown to medical men. Their collection attests the industry of the author, and the great attention paid to public health in the French capital. There is a short and interesting notice of the effects of the hasty burials of July, 1830, and of the subsequent and necessary exhumations. A memoir on the prevalence of ulcers of the legs among work-people contains some useful hints. So awake are the good people of Paris to hygienic precautions that even a poor beater of carpets cannot establish himself on a vacant spot of ground without becoming vexatiously involved. He selects what seems a happy locality; solicits and obtains leave from the commissary of police, and from the surveyor of the highways; sets up his lines, and commences active proceedings. Straightway the neighbours take the alarm; draw up a *procès-verbal*, and petition the council of salubrity. Five commissioners are appointed to look into the affair. The complaints of the excited neighbourhood resolve themselves into three heads; insalubrity, inconvenience, depreciation of surrounding property. Carpet-beating is suspended during the process of enquiry. First, as respects insalubrity; the dust of carpets, say the neighbours, consists of the *fæcal* dejections of animals, of moths and vermin; and these, borne on the wings of the wind, quickly destroy furniture, and even trees and plants; the same dust also absorbing in its flight the principles of many diseases, and thus causing inflammations of the eyes, cough, and pectoral irritation: it also drags with it woolly particles, which excite hæmoptysis, which brings on phthisis. In support of these charges they adduce some beaters of carpets themselves, found with some difficulty, but who, although receiving high wages, are pale, thin, and asthmatic. We must here observe, that M. Parent, not content with shewing that the dust of carpets is guiltless of many of these offences, insists on the perfect harmlessness of all the flocculent particles which thicken the atmosphere breathed by those who beat the hospital-mattresses, by beaters of the hair of cachemire and camel, of rabbit and of hare; of those employed in making hats; of those who drive in dusty roads, of those who live in the plaster-quarries, and of tailors in general. Herein we somewhat suspect the accuracy of M. Parent's enquiries. But the commissioners, rejecting the idea of the insalubrity of carpet dust, pronounce the noise of the trade insufferable, and hurtful to the value of surrounding land. The hammer produces no clatter so annoying as this. An ingenious beater of carpets lodged himself, years ago, beneath one of the arches of the Pont-Neuf, and laboured unceasingly from morning until night: but the whole neighbourhood cried out upon him: shopkeepers of the *quai des Augustins* were driven to desperation; inhabitants of the *quai de la Monnaie* could not hear themselves speak; and the police were obliged to unearth the poor carpet-beater for the peace of the parish. So it was also with the carpet-beater of the *rue de Marbeuf*, a more aristocratic quarter: he was pronounced a depre-

ciator of property, his privilege was withdrawn, and he was compelled to decamp.

Without extending our notice of these Memoirs, we can safely recommend them for reference when any of our medical brethren are called upon to give an opinion concerning supposed nuisances of various kinds. It is possible that the subject may have been too minutely studied in Paris; but it has certainly not attracted sufficient attention in England.

ART. X.

Practical Surgery. With one hundred and twenty Engravings on Wood.

By ROBERT LISTON, Surgeon.—London, 1837. 8vo. pp. 494.

THE object of Mr. Liston's volume is to present to students of surgery and young practitioners, a plain, common-sense view of the most important injuries and diseases which are met with in practice, unencumbered by speculations or theories, and accompanied by simple directions how to conduct the treatment. In noticing the work, it appears to us that we shall render Mr. Liston the most justice, and our readers the most service, by adverting chiefly to those points in which he recommends a different practice to that commonly adopted. For the mere elements of operative surgery, the common points of professional knowledge, we must recommend the perusal of the work itself.

In the first chapter many instructive remarks are made on the division of parts by the knife. The very first observation is important. It conveys useful advice to the junior surgeon, and we wish we could add that it was always remembered or acted upon by the veteran practitioner. When once the necessity for the performance of a surgical operation is clearly indicated, the first consideration that ought to occupy attention is the mental and bodily condition of the patient. If these are in a favorable state, "the less delay in resorting to operative procedure, and the shorter the period of uncertainty and suspense in which the patient is kept, the better." In hospital practice, it too frequently happens, that, when the performance of an operation is determined upon, the patient, perfectly aware of the trial he has to undergo, is left to the next "operating day," that every student may have an opportunity of witnessing the operation. He necessarily broods over the sufferings he has to undergo, and neither his mental powers nor his bodily strength can be in so favorable a state for subsequent recovery as if the surgeon had acted without delay, when the necessity for operating had been established. The following hints, too, may not be useless to the old as well as young surgeon.

"The operator should consider well the place which he himself should occupy during the proceeding, so that he may, without awkwardness or change of position, be able to effect his object efficiently and gracefully; he will also act wisely in general, so to dispose the instruments and apparatus, that he can at once put his own hand upon them, and thus render himself independent of lookers-on, who, in nine cases out of ten, owing to anxiety or curiosity, or to their hurry and agitation, hand anything but what may at the instant be required. If he have had experience in such proceedings, he will previously have ascertained that everything is in order, that the cutting instruments have good points, that their edges are keen, and that the joints of forceps and scissors move freely and readily." (P. 6.)

Upon more than one occasion we have seen confusion arise during the performance of an operation, and one instrument confessedly substituted for another, because the assistants were not aware of the particular implements the operator might prefer, and they were consequently not at hand.

Upon the subject of the "Union of Wounds," Mr. Liston observes that, so long as oozing of blood continues, no purpose can in general be answered by putting the divided surfaces into close contact. If this practice be pursued, the blood which oozes away from the smaller vessels is necessarily prevented from escaping. The consequence must be infiltration of the thin and loose open cellular tissue around, the distention of the cavity of the wound, and the separation of the surfaces and deep-seated parts. The following sketch of routine practice appears to us correctly drawn, and we really think the author is not amenable to the charge of exaggeration.

"A sort of routine practice has been long pursued in dressing wounds. They are put together without delay, and their edges squeezed into apposition and retained so, by various means, such as sutures, plasters, compresses, and bandages. They are carefully covered up and concealed from our view for a certain number of days. Then the envelopes of cotton and flannel, the compress cloths, the pledgets of healing ointment and plasters are taken away, loaded with putrid exhalations and a profusion of bloody, ill-digested, fætid matter. A basin is forthwith held under the injured part, and the exposed and tender surface is deluged with water from a sponge, and then well squeezed and wiped. Then comes a re-application of retentive bandage, of the plaster, of the grease mixed with drying powder, and surmounted by some absorbent stuff, as charpie or tow, to soak up the discharge. This is not unaccompanied with pain, often more complained of than that attendant upon the original injury or operation. This process is repeated day after day, the patient is kept in a state of constant excitement, and often falls a victim to the practice, worn out by suffering, discharge, and hectic fever. It would here serve no purpose to detail the mode of cutting the plasters, and of spreading the pledgets; nor would there be any use in giving an opinion as to whether a mixture of the earth called armenian bole, or the impure oxyde of zinc, the tutty or calamine, was the best ingredient to put into the digestive, drying, healing unguents, or as to whether they should be compounded of one kind of animal fat and vegetable oil, or another; nor would any good come of stating how the soiled bandages and filthy straps are to be removed, whether they are to be cut off or removed first from one end and then from the other. The system is a bad one, the applications filthy and abominable; the whole proceeding outrages nature and common sense. The wound is put in a forcing-bed, as it were; excited action, beyond what is required, is hurried on, and the consequence is, that union seldom, if ever, can or does take place. A suppurating surface, on the contrary, with profuse discharge, and a very tedious cure, if any, is obtained." (Pp. 28-29.)

Surfaces are not disposed to unite for many hours after the division and separation has occurred. So long as oozing continues, there is no good end to be achieved by their close apposition. It is only when reaction has occurred, when the vascular excitement around the solution of continuity has taken place, and the circulation has been roused; when plastic matter begins to be secreted and thrown out, that the process can be expected to commence. The edges of a large wound, as in amputation of the extremities, may be approximated in part, as soon as the bleeding from the principal vessels has been arrested: "but the close apposition, and the application of all the retentive means had better be delayed for six or eight hours at least. In the interval, extreme sensibility of the injured parts may be abated, the oozing moderated, and the

chance of secondary bleeding much diminished, by covering the parts with lint dipped in cold water, and frequently renewed." This is the best practice, in Mr. Liston's opinion, in wounds made by the surgeon, and those which are accidental, of the incised kind, that are fresh and bleed freely. Mr. L. has a great antipathy to poultices: for example, in speaking of the management of accidentally incised wounds, he says,

"After hæmorrhage has entirely ceased, applications of an agreeable warmth should be substituted, such as poultice of bread and water, or what is much to be preferred for its simplicity, lint of thick texture, and of sufficient size to cover the wound, soaked in tepid water, and that overlaid with an ample piece of oiled silk, to prevent evaporation. Heat and moisture, by which qualities a poultice produces its soothing and beneficial effects, by which the surface is relaxed, its capillary circulation encouraged, and discharge promoted, are thus amply afforded, without any of the weight, putrefactive fermentation, stench, and filth, which is inseparable even from the best and most scientifically contrived epithems and cataplasms." (P. 29.)

The adhesive plaster in common use is objected to: "it does not retain its hold sufficiently long; it is loosened by discharge, it heats the surface, and often gives rise to erythema." For many years a "better sort of plaster" has been used by Mr. Liston and his colleagues in Edinburgh and London. It is composed of a solution of isinglass in spirit, and may be spread for use, as occasion requires, on slips of oiled silk, or silk glazed on one side only, and on the unglazed side. This plaster is kept ready for use by Messrs. Fisher and Toller, of Conduit street. It is cut into strips of the desired breadth, and the adhesive matter dissolved immediately before it is employed by the application of a hot moist sponge. "It is adherent, keeps its hold to the end of the cure, and is quite unirritating." Being transparent, too, the plaster does not prevent any untoward process that may be going on underneath from being observed, and if any fluid collects, an opening can be snipped for its escape. Under some circumstances, and in some instances, the delay advised in bringing together the surfaces of wounds, and closing them permanently may be departed from with great advantage, as when the entire surface can be brought into close and accurate apposition, so that no clot of blood can by any possibility be interposed.

Under the head of *Injuries of Bones*, Mr. Liston includes Fractures of the cranium, of the pelvis, injuries of the spinal column, and fractures of the upper and lower extremities, disunited fracture, and diseases of bones. Passing over many excellent remarks as to the diagnosis of mere concussion of the brain and compression, we come to a practical opinion of Mr. Liston's on the subject of trephining, in which many surgeons would not coincide. It is laid down as a general rule, to which very few exceptions are admitted, that the only true and justifiable reason for employing the trephine, or sawing away any portion of the skull, is to remove pressure from the brain, which pressure must also actually occasion dangerous symptoms. Mr. Lawrence is reported to state in his lectures* "that, in the present day, we have entirely discarded the doctrine of employing the trephine, or instrument of any sort, in all cases of fracture of the skull; and consider it as a measure that we are not to have recourse to, except where there is a depression of the bone, and *that depression of the bone accompanied with symptoms of pressure on the*

* Med. Gazette, vol. vi. 599.

brain." Desault, in the latter years of his life, had entirely abandoned the operation, so numerous had been the unfavorable results of trephining in the Hôtel Dieu. Mr. Lawrence, from what he has seen at St. Bartholomew's Hospital, is "nearly" inclined to coincide with Desault. He says* that, "of the instances in which I have seen the operation performed in this hospital, the greater number have terminated fatally, so that I can cite to you, as far as the experience of this hospital goes, very few instances in which the life of the patient has been saved by the operation of trephining." Mr. Liston has not adverted to this general opinion upon the subject, and we refer to it, as we think the student should know the doubts that exist upon so important a practical subject. We are very far from doubting the solidity of the conclusions to which Mr. Liston's reflections and experience have led him. On the contrary, it appears to us that the observations he makes in the work before us, as well as in the second part of his "*Elements of Surgery*," and more especially the cases he relates, go very far to shew that the surgeon may often be justified in using the trephine, even though the symptoms of oppressed brain, or disturbance of its functions, do not exist. In the second part of the "*Elements of Surgery*," there are some excellent cases detailed which tend still more strongly to shew the occasional propriety of using the trephine when no serious symptoms exist, than those which are given in the work before us. We may briefly mention a case in point which we saw, and which was under the care of Mr. Arnott, at the Middlesex Hospital. The patient, a woman, was admitted with the iron leg of a pot sticking in her skull. The house-surgeon removed it with little difficulty. No serious symptoms arose, and she went on quite well. At the expiration of some days the wound suppurated, and Mr. Arnott removed several splinters of bone of the external table of the cranium. There was one larger portion of the inner table of bone, which was loose, but which could not be removed. Fearing the subsequent occurrence of inflammation of the dura mater from this loosened portion of bone, Mr. Arnott trephined the patient, although no symptoms existed of compression of the brain, and the patient's life was perfectly safe at the time. The portion of bone was thus removed, and the patient speedily recovered, and has remained well for three years. It is not, however, to be supposed that Mr. Liston is an advocate for the use of the trephine so frequently and almost indiscriminately as it was once employed in most injuries of the skull. He expressly states "that it is an operation, at this period, very rarely resorted to or witnessed."

For the management of different fractures of the extremities, we must refer to the work. Upon the subject of disunited fractures, cases so extremely distressing to patients, and generally so perplexing to the surgeon, Mr. Liston's experience cannot but be acceptable.

"Various means have been resorted to with a view to set up a process by which consolidation might be brought about. In recent cases, by changing the position of the limb if necessary, and taking great pains to prevent the least movement of the parts, retaining them in very close and accurate contact by means of splints and bandages, the object may be attained. But it is generally necessary to take means previously to promote a certain degree of excited action. The ends of the bones have

* *Med. Gazette*, vol. vi. 599.

been moved about, and rubbed against each other. Incisions have been made, escharotics applied, and portions of the bone have been sawn off in many cases, without much benefit accruing. The chance of success depends a great deal upon the relative position of the ends. If they overlap, and are in contact, the case may be looked upon as favorable. The action of the vessels of the membrane of the bone can be roused, and the best means of doing so is by the introduction of a perforator betwixt the two portions, followed by a strong needle, with an eye near its point, by which a coarse seton is passed in withdrawing it. Of course an opening will be made with a bistoury in the soft parts, close to the bone, taking care to avoid vessels, nerves, or other organs of importance, before the introduction of instruments to disturb and lacerate the deep parts. If the bones do not overlap, and they very generally are so placed, it will be advisable to make the attempt to put them in that position; a short and firm limb being more serviceable than one that dangles about, weak and unsupported by the muscles. This practice has succeeded in the humerus, which, by the way, is the bone most frequently the seat of false joint, in the thigh, fore-arm, and leg. I have had several cases in which the treatment by seton has been followed by a perfectly successful result in the fore-arm, upper-arm, and leg; I have also, as might be expected, had one or two failures, and have besides declined interfering in several instances, on account of the unfavorable circumstances attendant upon them. The plan I have pursued has been to pull about the parts a good deal at first, to introduce a larger and larger cord, and to remove the foreign body at the end of a few days, eight or ten, as soon in fact as a considerable degree of excited action had arisen in the bone and periosteum, and before it has begun to decline; the limb is then put up with great care, and every chance of the slightest motion taking place guarded against. The object of passing a seton is assuredly not to promote discharge, which is prejudicial to and often enough, when the result of accident, interferes with the union, and gives rise to the necessity for such operations as that now under consideration." (P. 86.)

The chapter on *Injuries and Diseases of Joints* is rich in very instructive matter and interesting cases. Our own experience has furnished us with abundant opportunities of seeing the truth of Mr. Liston's remarks on the subject of the much too-frequent employment of moxas, issues, perpetual blisters, &c., to the destruction of the patient's health, when patience and rest alone ultimately succeeded in effecting the cure of diseased joints.

"In all injuries and diseases of joints," says Mr. Liston, "in the slow, strumous degenerations, white swellings (a most comprehensive term), as in the most violent form of articular inflammation, perfect quietude and repose of the affected part form the most powerful and essential curative indication: neglect this, all other means are found nugatory, and were as well untried. Nothing but disrepute and disgrace can accrue to the profession and professors if hot irons, moxas, and issues continue to be used, as they often are, inconsiderately enough, to the neglect of more powerful and less appalling means." . . . "The sudden improvement in the health of patients worn down by the disease of a joint can be witnessed at any time, in the cases of morbus coxarius, treated on the principle here laid down, at the North London Hospital. In the first stage of the disease, during the period when there is apparent elongation, and also where ulceration has made progress and the limb is shrunk and shortened, great benefit and relief will be found to follow the adoption of this method; the joint is placed extended, the most favorable position it can occupy, should permanent stiffness arise. A splint, similar to that used by the old surgeons, composed of some soft substance, as tow and alumen, described by Scultetus, and delineated by him even to the eggs on a platter, is applied; we use coarse soft lint soaked in a strong solution of gum acacia; it is laid on in strips over the side and pelvis, from the short ribs down to below the knee, and made to embrace the limb fully. A layer of dry lint is first applied, and then two or three others, soaked in the mucilage, follow; this is covered by a fold or two of coarse calico, and the whole retained by a roller. In cases where the limb has been retained for a long period adducted and bent, and where some little trouble and uneasiness has arisen in placing it in a favorable position, it

will be advisable to preserve it so by the use of the thigh-splint, as for fracture, for some twenty or twenty-four hours, until the composition dries, and the splint has adapted itself closely to the parts." (Pp. 130-131.)

In acute inflammatory attacks of the joints, active antiphlogistic means must be adopted, so as at once to make an impression upon and extinguish the action. After free general and local bleeding, "fomentations will relax and keep up a determination to the surface; cold lotions, so often and inconsiderately used, must have the opposite effect. It is a thorough routine system, useful in few cases, and productive of aggravation in many." Mr. Liston doubts the good effects of cold applications in any case after inflammation has commenced, however useful they may be before the action has come on, with the view of preventing and moderating it. In chronic swelling of a joint the great object of the surgeon will be to prevent destruction of the cartilages and bones: he must also endeavour to promote absorption of the fluids effused into the synovial capsule and bursæ, and to bring their secreting membranes into a more healthy condition. In the more favorable cases speedy amendment will follow the fixing the articulation, and the application of uniform and gentle pressure.

"This is effected somewhat after the fashion of Mr. Scott's plastering, which has been employed very extensively, and rather indiscriminately, to all and sundry affections of joints, and to many swellings and pains in other parts. A greater part of Mr. Scott's process—all that part of it intended for effect—may well and safely be dispensed with, such as the bathing with camphorated spirit, the mercurial ointment, and a vast deal of the plastering. It is advisable to give support to the lower part of the limb by a roller, to near the affected joint; the surface should then be covered by lint, spread with some gently stimulating application, soap cerate with camphor answers well; the whole articulation is then to be surrounded and supported by long strips of plaster, crossed in various directions, and drawn with a very moderate degree of tightness, so as to give a feeling of support without occasioning uneasiness: the roller is then carried upwards over this dressing and to some extent above the joint. In order to prevent motion of the affected parts, which would tend to keep up and increase the mischief, leather splints should be applied outside." (P. 133.)

The general health must be carefully attended to. The alkalies, sarsaparilla, iodine, are all useful. Mr. Liston, we think, is a little incautious in his utter exclusion of mercurial alteratives. Even in the more painful and dangerous affections of the joints, where the cartilages are probably absorbed, good effects may arise from judicious treatment. Here the principle of preventing all motion, if well followed out, will be found advantageous. Great relief will often be obtained by establishing a permanent discharge from the *neighbourhood* of the diseased tissues.

"This can be done simply, and effectually, without causing alarm, or exciting much pain, by confining with a piece of lint and diachylon plaster, a small bit of caustic potash on the skin near the diseased joint. After the slough separates, the sore is dressed with any simple ointment, and it is deepened and made to discharge freely, when disposed to heal, by a few hours' application of the antimonial ointment. A seton may be preferred in some situations; certainly discharge can be kept up and derivation obtained from the affected parts thus, fully as well as by actual cauteries, moxas, pea issues, or other farrier-like practices. Great care must be taken in the placing of issues; they should be near, but not upon a joint. Serious results have now and then followed their careless application. Diseased action has been increased, the cautery having reached, or even penetrated, the synovial capsule. I write after some experience, and from cases and specimens now before me." (P. 135.)

Under the head of *Injuries and Diseases of Blood-vessels*, Mr. Liston observes that the ligature of the common carotid, has, thanks to Sir Astley Cooper, been placed among the regular operations. Before undertaking any operation for aneurism, more especially by ligature of a vessel near the heart, it will be advisable to see that the circulation is in a quiet state; and, if not contra-indicated, it may be found a safe precaution to diminish the quantity of circulating fluid by one or more venesections beforehand, and a low diet. The effects of disturbing the flow of blood to the brain was much dreaded in earlier times. The disease was looked upon as irremediable, and the patient was left to his fate. "A case is given by J. Bell, in which, so late as 1807 or 1808, a poor woman was allowed to perish by hemorrhage from the sac, under the eyes of himself and colleagues." Both carotids have been tied in the human subject, with but a short period of time intervening, and without any bad effects arising. Mr. Liston objects to the operation of tying the carotid artery, in order to arrest the growth of tumours of the face and jaws; or as a preliminary step to the removal of such diseases.

"The suffering of the patient is thus much prolonged, without his safety being at all enhanced, or the dissection of the tumour in any way facilitated. The flow of blood is quite as effectually commanded by pressure with the fingers on the common carotid of the affected side; pressure on both at the same time has given rise to most alarming convulsive movements in some cases, after great loss of blood had occurred. Even pressure on one trunk is not demanded in many of the operations in question. I believe I have had fully as much experience in the management of tumours of the mouth and jaws, and of the face and neck as any surgeon in this country, and have never seen occasion to tie the carotid previous to or during the operations for their removal; and have never regretted omitting this supposed precautionary measure." (P. 164.)

The arteries running to the thyroid body have been tied, with the view of diminishing the hypertrophy by which it is sometimes effected; "but no good has been answered by such proceedings." Aneurisms are met with at the root of the neck, and so situated that there is no possibility of reaching a sound portion of vessel betwixt them and the heart, so as to interrupt the flow of blood. It has been proposed to treat aneurisms of these vessels close to their origin by ligature on their distal side.

"This is a proceeding," says Mr. Liston, "which, in a favorable case of the kind and at the urgent solicitation of the patient, a surgeon might be induced to adopt, as the only though desperate remedy for an otherwise incurable disease; it is an operation, however, which he would not be warranted in urging a patient to submit to." (P. 172.)

It may be necessary to tie the brachial artery in consequence of wound of the palm, involving the deep or superficial palmar arches, or on account of spontaneous aneurism in this part; a most uncommon circumstance. The following case came under the notice of Mr. Grainger and Mr. Pilcher.

"A working goldsmith, about forty years of age, of a gouty diathesis, had a tumour formed beneath the ball of the right thumb; it was mistaken for an abscess; on careful examination, I discovered it to be an aneurism, and believed it to be situated between the adductor pollicis and abductor indicis, and to be a disease of the radial artery at its terminal division, probably induced by the repeated, though slight, blows from the handle of the hammer, which his occupation constantly obliged him to use. I proposed to tie the radial and ulnar arteries immediately above the wrist, provided the ligature around the radial was ineffective in diminishing the tumour and arresting

its pulsation. My expectations were realized; the closure of the radial artery was attended with diminution in the size and pulsation of the tumour, but still both remained to rather less than half the previous degree. I immediately tied the ulnar, when the tumour was much reduced in size, and the pulsation completely: slight secondary hæmorrhage occurred from one of the arteries at the seat of the ligature two or three days after the operation, which was arrested by cold water; the case progressed without any further untoward symptom, and was attended with a perfect cure.” (P. 177.)

In this instance success followed the operation; but Mr. Liston says there was a risk of the tumour being filled with fluid blood through the interosseous, and states that, if such a case were presented to him, he would tie the humoral artery low in the arm. The following remarks are too important to be condensed :

“Recent hæmorrhage from the palm must be commanded by ligature on the divided ends of the vessels, exposed farther, if need be, by dilatation of the wound. If the bleeding has been at first commanded by pressure, as it may be permanently, when but small twigs are implicated, and blood bursts out impetuously again and again, after the tissues have been altered, by inflammatory swelling, abscess, and infiltration of blood, then should the clearing out of the wound, and methodical pressure from the bottom of the cavity not prove effectual, recourse should at once be had to the operation on the brachial. There is no use in trying to include the vessels, even could they be got at readily and safely in the palm; they will not hold a ligature when in this state for any time, nor can ligature on the radial and ulnar be relied upon. Pressure on the vessels is sometimes resorted to in such cases, and in such a way as to operate upon their course, part of the circumference of the limb being uncompressed. Ring tourniquets, as they have been called, are invented and sometimes employed for the purpose; the veins cannot entirely escape obstruction, and the effect of this upon the diseased parts must be apparent. It is a remnant of the old and barbarous surgery. The practice may be successful at a time, and by chance, but if generally employed it must lead to mischief in the deep parts, bones, and joints, and dependence cannot be placed upon it for a cure. The ligature of the vessel under very unfavorable circumstances, might, after all, be called for, or even the amputation of the member.” (Pp. 178-179.)

In the next chapter, *on Injuries and Diseases of the Integument and Cellular Tissue*, we again find Mr. Liston strongly deprecating the application of poultices.

“A poultice, always a filthy and uncomfortable application from its weight and stench, may be used; what is much better, equally efficacious, and not liable to objection in any way, is a double piece of lint soaked in hot water, of an agreeable temperature, applied to the part; it is covered by an ample piece of oiled silk, to prevent evaporation, and this dressing, simple enough, but answering every purpose, is removed frequently; the lint may be moistened by merely removing the oiled silk, if the parts are very tender, and if it be not soiled by discharges; all the soothing effect of a poultice is thus produced, without any discomfort. This warm-water dressing is light and nice, and it is changed often if there be any unpleasant odour exhaled; it may be medicated with extract of poppies, or salts of opium, or it may be coloured, or have some aromatic added, if the patient does not put faith in simple means. A great deal has been said about water-dressing, and the merit of introducing it; water has been applied to sores from time immemorial. The simple element, water, was supposed to be congenial to wounds and sores; it was used to cool parts. The water-dressing has been used in my hospital and private practice for a long series of years, as a substitute for poultice, as a means of conveying and preserving heat and moisture, on a surface that should secrete pus for its protection, and as an accompaniment of the process of healing by the second intention.” (Pp. 197-198.)

We are by no means disposed to “gulf at every change,” as Burton

says, but there appears to us much good sense in this proposed innovation upon the old poultice system, and we cannot but wonder that Mr. Liston has not hit one more blow against the "abomination," by asking, how many hours out of the twenty-four does a poulticed patient enjoy the benefit of a warm application?

The next chapters treat of the *Restoration of Lost Parts*, and of *Morbid Growths and Enlargements*. We can only touch upon one point discussed here. It was customary to remove the chronic enlargements of the tonsils and uvula by ligature, and, strange to say, this method is still preferred and practised by some surgeons.

"It is a most difficult, tedious, painful, and unsatisfactory proceeding, so far as I have seen and can understand; there is no excuse for persisting in it, no risk whatever of bleeding is incurred, if the incision is practised properly. In many cases, by getting rid of the cause, or waiting until that disappears, until wisdom teeth make their way through the gum, (this may sometimes be accelerated) by the use of strong astringent applications and constitutional treatment, the necessity for any operation may be dispensed with. But in cases where other means have failed, where much inconvenience is felt,—above all, when the isthmus faucium is much narrowed, and any alarming difficulty of respiration has occurred, then the removal should not be delayed. It is by no means necessary to remove the whole tonsil, and the attempt would be attended with the greatest danger." (P. 251.)

We particularly recommend to the notice of the young surgeon the tenth chapter, on *Amputations*. The practical directions that are given in the text are rendered extremely clear and intelligible by numerous well planned and executed sketches.

In the next chapter, after pointing out the management of wounds and foreign bodies in the eye, *obstructions* in the nasal duct, and foreign bodies in the pharynx, Mr. Liston comes to the subject of *transverse Wounds of the Larynx and Trachea*, and gives a very good case that well exemplifies the danger of the too-common practice of bringing wounds of the throat together by sutures, bandages, &c., before the oozing of blood has completely ceased, and "the surface is glazed."

"Great and imminent danger, as has already been pointed out, arises from the closure of the wound and the consequent inhalation of blood, but even though the air-tube is not opened, the patient may be put in great jeopardy by the close apposition of the edges of the incision. The blood is apt to accumulate in the cavity, and coagulates; hæmorrhage is thus kept up; the size and pressure of the clot may even interfere with the function of respiration. A young woman was admitted into the North London Hospital, some months ago, on account of a transverse wound of the fore part of the neck over the upper part of the thyroid cartilage. It was ragged, had been inflicted by repeated application of the cutting instrument, and the integument had been somewhat detached from the subjacent parts. The wound was stitched closely before her admission; the then house-surgeon, disregarding the common-sense view of the case, and despite of the principles which I had over and over again inculcated, very foolishly did not throw the edges loose, even though the wound had bled repeatedly and the patient did not breathe with freedom; I was making my visit in an adjoining ward, when the nurse rushed in to say that her patient was dying of suffocation, and she was correct in her statement; the patient was gasping for breath, with a livid countenance, and scarcely any pulse. The stitches were immediately cut out and a large clot removed. There was no further bleeding, the breathing became unembarrassed, and all did well." (P. 339.)

In wounds which do not penetrate very deeply, the patient can take sufficient support at any stage of the case; but, when the pharynx is

implicated, it will be necessary, from the first, to convey, at proper intervals, liquid nourishment into the canal beyond the wound. "There is no use in passing long tubes into the stomach for this purpose; a large elastic catheter and gum bottle will be found quite sufficient for the purpose."

"A great error is sometimes committed in the treatment of cases of cut-throat. The patient is fed through the wound in the neck, the contraction is not favoured by position, and the surfaces are permitted to cicatrize separately; the voice is consequently lost, the patient is rendered perfectly incapable of exertion, not having any control over his respiration, and being thus unable to keep his chest expanded. The patient is, moreover, put in great jeopardy; he is subject to bronchitic attacks and to inflammatory œdema of the orifice through which the air enters; he may thus be cut off suddenly, if in the hands of ill-informed or inexperienced persons, or he may be worn out by cough and profuse expectoration. It is possible occasionally, to remedy even such mismanaged cases; the contracted air-passage above may be widened by the introduction of instruments, and the edges of the wound pared and brought together. A very remarkable case of the kind, which occurred in my practice, is detailed in the '*Edinburgh Medical and Surgical Journal*,' vol. xciv. p. 118, and in the '*Elements of Surgery*,' vol. ii. p. 243. The patient had the opening through which she had breathed entirely for many months, and in which, in fact, she wore a large round tracheotomy tube, closed, after dilatation of the opening leading upwards into the glottis, which was apparently almost obliterated; and although it was necessary to perform tracheotomy on account of the swelling which supervened and threatened suffocation, a few hours after the removal of a piece of elastic tube which had been worn for many days in the trachea, a perfect recovery took place, the breathing became free, and the voice was almost perfectly restored." (P. 341.)

After taking a general view of the signs and treatment of foreign bodies in the windpipe, Mr. Liston lays it down as his opinion that there can be no question when a foreign body has entered the air-tube; that the sooner it is got rid of the better, whether it lodges in the larynx, floats loosely in the trachea, or is impacted in one of the bronchi. In such cases the practitioner may easily be thrown off his guard by the long intervals of cessation from any serious or even troublesome symptoms; but the patient, so long as any foreign body is in either of these situations, must be in danger from the occasional and very violent fits of coughing to which he will be subject. If he escape from this hazard, he has to encounter the great risk of the supervention of organic disease of the lungs, which may not, it is true, take place for a very long period; but the records of surgery teach us that it will very generally occur unless the foreign body is removed by the efforts of nature or the assistance of art.

Mr. Liston relates Mr. Duncan's case, which we believe is the only one on record in which a foreign body was discovered in, and successfully removed from, the bronchus by operation.* Other cases are adverted to, in which it may become necessary to resort to the operation of tracheotomy, on account of obstruction to the free entrance of air into the lungs, caused by disease at the top of the tube. It is more frequently demanded in cases of œdema of the glottis than from any other cause; "and it is generally attended with a happy result." Mr. L. states that diseases of the larynx are extremely common in the northern metropolis; and that he must have performed the operation nearly twenty times, and with almost uniform success, for chronic swelling, with urgent and threatening symptoms. He objects to the removal of an oval portion of the

* *Lancet*, vol. ii. 1833-34, p. 419.

tube, for the purpose of keeping the wound patent: he regards this expedient as difficult to accomplish; believes that it does not serve the purpose long, and that it is apt to be followed, after the healing of the wound, by inconvenient and dangerous narrowing of the trachea. He prefers the introduction of a tube, and says "that, after a few minutes, all irritation from its presence ceases." We doubt this speedy cessation of irritation from the presence of the tube as a general fact, and we think that the objections stated by Mr. Liston to the removal of a portion of the trachea more imaginary than real. The practice is sanctioned by Lawrence, White, Carmichael, and Sir C. Bell; and it has been adopted by Mr. Arnott, at the Middlesex Hospital, with safety and success.

The section on *Injuries and Diseases of the Rectum and Anus* is very interesting; but, as we shall have occasion to notice these subjects in another article, we can merely advert to them here. Mr. Liston ridicules the attempts that have been made to mystify the subject of diseases of these parts, and to separate them in a great measure from general surgery: he contends (and justly too, in our opinion,) that there is no such difficulty as has been supposed in understanding their nature.

Mr. Liston commences the subject of *Injuries and Diseases of the Genito-urinary Organs* with a remark to which we would willingly object, but, in truth, we cannot. Our own experience has impressed the mortifying fact upon our minds "that by far the greater number of grave and serious injuries of the urethra and bladder have been inflicted by pretenders to surgical knowledge; and that a considerable number of diseases which come under the notice of surgeons are the product of mismanagement on some previous occasion." In the use of bougies, care and gentleness are particularly insisted upon; and Mr. Liston prefers a small silver catheter, if there is reason to suppose the contraction is considerable: if not, a plated metal bougie of moderate size, slightly curved throughout, and smeared with some bland liniment, may be used. These, he assures us, are much superior in every respect to the plaster, gum, or other soft and pliable bougies. There is no possibility of guiding the points of the latter, or of ascertaining what direction they take.

"Retention, when the prostate is enlarged, can in general be readily relieved if a proper instrument be used: in many such cases, the bladder cannot possibly be reached with those of ordinary length. The prostate catheter should be made of silver, and at least three inches longer than those employed for other purposes; the beak should be long, and the curve considerably greater. The careful employment of such an instrument will generally be followed by a successful result; whereas, attempts with short and elastic catheters must almost certainly end in disappointment to the practitioner and great injury to the patient. Innumerable cases have been presented to me, in which, for days, persevering attempts have been made to relieve an over-distended bladder: nothing but blood, and that in abundance, has flowed. It has then been imagined that the bladder was full of blood, and means have been employed, such as exhausting syringes and injections of warm water, to break down and extract the coagula supposed to exist. A long catheter has been at last used, with the effect of freeing the bladder of many pounds of high-coloured urine, but nothing else." (P. 389.)

In Mr. Liston's opinion, the operation of puncturing the bladder has been, and still is, much more frequently performed than there is any occasion for. It was not once performed during a series of years, when he was assistant-surgeon and surgeon to the Royal Infirmary of Edinburgh;

and it has been performed but once, and that before he joined it, at the North London Hospital, since it was opened; although many cases of bad urinary disease had been admitted at both institutions.

Mr. L. is an advocate for the operation of lithotomy, as it is at present improved and simplified. He regards it as applicable to a vast variety of cases, and thinks it likely to supersede, as a great measure, the cutting operation for stone. We refer to the work for an account of the cases which are adapted to this practice, and for the mode of employing it.

"The operation of lithotomy, if circumstances are favorable, if the patient's health is tolerably good and his kidneys sound, is every satisfactory in its results when properly performed with few and simple incisions, and on a good plan. The incision of the perineum, and of the prostate to a limited extent, is to be preferred in almost every case. The operation on the gripe, and the recto-vesical method, are not now in the list of regular and established operations. The operation above the pubes, still practised occasionally, is not to be resorted to from choice. If a stone of such a size as could not pass readily through the outlet of the pelvis, perhaps contracted unnaturally, were ever met with, the high incision might be thought preferable." (P. 408.)

As Mr. Liston has performed the lateral operation of lithotomy nearly a hundred times, his practical directions upon this important subject deserve the maturest consideration, and we will not weaken them by an abridgment.

In the chapter on *Injuries and Diseases of Serous Cavities*, Mr. L. lays it down as a rule, "that the evacuation of purulent fluid from the cavity of the pleura must be had recourse to as soon as its existence is ascertained." He has known instances, and we have known similar ones, in which the operation has injudiciously been delayed until the matter was about to make its way to the surface, by the absorption of the intercostal muscles. "Pointing and fluctuation must not be waited for here, as in many other situations." Much difference of opinion once existed as to the time when the operation of empyema should be had recourse to, but we believe that, in the present day, the great majority of surgeons will support the general rule laid down by Mr. Liston. The following account of a distressing class of cases is important:

"Watery swellings of the scrotum are also met with; a result of injury or of some disease in the neighbourhood. In inflammatory œdema of the part, the serosity occupies the cellular tissue. I have seen a number of these cases following sores of the prepuce or glands, sores or fistulæ about the anus; occasionally, also, this swelling, with erythema of the scrotum, arises without any local cause that can be discovered; sometimes it has been attributed to slight injury. In hospital practice, several cases of the kind have now and then presented about the same period. This distension of the scrotum is sudden and considerable; the tumour is slightly red and shining, and very soon a dark or greyish patch is perceptible at the lower part. Unless free incisions are resorted to, the part suspended, and active general and local means employed, the cellular tissue, the skin, and the coverings of the testis, will be lost as effectually as if urine had been extravasated. I had no less than six such cases in the Edinburgh Hospital at one time, in an unhealthy season, and in different stages of their progress. The loss of cellular tissue will depend upon the period at which the case is presented, and the activity and judgment with which the treatment is conducted." (P. 429.)

The work concludes with a very instructive chapter on *Hernia*, and a brief notice of the surgical treatment of *Congenital Deficiencies and Deformities*.

The impression made upon us by the attentive perusal of Mr. Liston's work is extremely favorable. As far as we can judge, there is no part of it which the young surgeon can study without great advantage; and there are many original observations interspersed in it, which merit the attention and consideration of experienced practitioners. If the work had been published anonymously, no doubt would have been entertained but that it came from the pen of one who was well fitted for such a task by extensive experience and sound judgment.

ART. XL.

Die Pocken-krankheit mit besonderer Rücksicht auf die Pathologische Anatomie. (Es giebt Pocken auf inneren Theilen.) Von ALEXANDER PETZOLDT, &c. Mit 4 Tafeln abbildungen.—Leipzig, 1836. 4to. pp. 120.

Small-Pox considered with particular Reference to Morbid Anatomy, and more especially to the occurrence of Pocks on internal Parts. By Dr. ALEXANDER PETZOLDT.—Leipzig, 1836.

IN the winter of 1832-33, small-pox prevailed epidemically at Leipzig, occasioning considerable mortality. The author of the monograph before us, then in attendance as a student at St. Jacob's Hospital, under Professor Clarus, had an opportunity of performing, and of seeing performed, both in that institution and in private, a considerable number of dissections of persons who died of this disease. Since that time he has had further opportunities of extending his researches during the prevalence of an epidemic, though of less severity, at Dresden, where he is now settled. Altogether, he estimates the number of bodies of persons dying of small-pox, which he has had an opportunity of inspecting, as not falling short of forty. In 1833 he published, at Leipzig, his inaugural dissertation, entitled "*Observationes quædam de Variolarum cum externarum tum internarum natura.*" The present more extended treatise is divided into four sections: in the *first*, he treats of the etymology, definition, and antiquity of small-pox; of the form of the pock, the course of the disease, and its consequences: in the *second*, of the morbid appearances found on examination after death: in the *third*, of the etiology, character, diagnosis, and prognosis; and in the *fourth*, of the treatment. Though he seems to have handled all these topics with great industry and ability, it is with the matter contained in the second section that we have been most interested; and to it we purpose at present to direct the attention of our readers.

Dr. Petzholdt has prefixed to his account of the morbid conditions of the Skin, and of the Mucous and Serous Membranes, some remarks on the healthy anatomy of these several textures; of the scope of which our readers may judge from his own summary of the topics on which he principally insists:—1st, that there exists a great correspondence between the structure of the mucous membranes and of the external skin; 2d, that the cuticle, although no nerves or vessels can be perceived in it, is still to be considered as living; 3d, that the cutis is composed of three layers, but these are only physiologically separable, being constituted by

the mode in which the blood-vessels are distributed; 4th, that the middle layer is the especial seat of the cutaneous glands; 5th, that these glands are distributed over the whole surface of the body, excepting the palm of the hand and sole of the foot; 6th, that the roots of the hair are not always imbedded in the glands; 7th, that mucous membrane has everywhere an epithelium; 8th, that the chemical difference between cutis and mucous membrane is scarcely appreciable.

The following is the description which Dr. P. has given of the appearances observable on the anatomical examination of the common integuments in small-pox.

*The Epidermis.** If this membrane be examined when the eruption of small-pox first breaks out, its undermost layers are found to be softened, almost spongy, and as if filled with a fluid. If a circular incision be made into the skin round the circumference of a papula, this, being loosened by the cut from its lateral connexion with the skin, can be removed pretty easily with the pincers, in the form of a little knot. This experiment shews that, at the period of the disease in question, the connexion of the cuticle with the cutis is nearly destroyed at those parts of the skin which are affected, whilst a perpendicular section affords us a ready opportunity of satisfying ourselves that there is no cavity beneath the cuticle. During the growth of the pustules, the spongy softening of the cuticle is increased; a still greater quantity of fluid collects between the substance of its lowest layers; there at length arises a small cavity filled with fluid, and, by the increased accumulation of this fluid, the cuticle is gradually pushed upwards. Its elevation is necessarily attended with expansion; the spongy texture is consequently compressed by the fluid, first at the apex of the vesicle, and it afterwards gives way towards its sides, and thus there remain only the superficial layers of the epidermis, which are clear and transparent. Whilst this is the state of parts internally, the pocks appear externally as clear vesicles. In all the pocks in which there exists a pit or umbilical depression, the cuticle is at this period connected with the cutis by a thread, (afterwards to be more particularly considered,) of which scarcely any trace can be found at the period of suppuration. When the pustule is perfectly formed, and therefore quite filled with pus, it is enclosed in only a very thin layer of cuticle; which, however, varies in thickness, as can best be seen by drawing off the cuticle carefully from the pustules, and after removing the purulent matter attached to it by washing it with water, laying it upon a black body, when it appears almost quite black in the centre, shewing that at that part it is very thin and transparent, whilst at the circumference, on the contrary, it is lighter or quite white, and consequently less transparent, or perfectly opaque, from being thicker at the circumference than at the centre. It is also particularly to be remarked that frequently, when we detach the thin covering of a pock, in which there exists a pit or umbilicus, a little hole is found in the centre of the exposed surface, which can never be observed externally, whether we examine the pocks in their natural condition, with the naked eye or with the microscope.

* Under this term the author includes the cuticle, properly so called, and the rete mucosum.

The Cutis. When the thin covering of a part of the skin occupied by a pock is removed, the cutis does not come immediately into view, but it is covered by a substance, varying in colour and consistence according to the degree of ripening of the pustule. At the time at which the formation of the cavity or hole described in the preceding paragraph commences, the fluid that covers the cutis is clear; at a later period it is turbid, more tenacious, and at length it becomes pure pus. If all these matters be removed, which is best done by a pretty strong stream of water from a small syringe, so as not to injure any of the subjacent parts, the following appearances can be seen with the aid of a microscope:—In all the pocks, where pus has formed, there remains some of it behind, which cannot be washed off; and, if we employ for these investigations portions of skin that have had their vessels filled with red colouring matter, it can be seen with the naked eye that the pus is, as it were, wedged in between the bundles of vessels, and is retained by them and between them. If an accurate examination be made of the bottom of the pock, which seems sometimes a little raised, on account of the uncommon accumulation of minute vessels, but sometimes a little depressed and as it were eroded, in almost all instances there comes into view a small depression or aperture. Sometimes, indeed, there are several such apertures, according to the condition and varying size of the pustule; but these lie scattered without order or symmetry. In a considerable number of pocks no such openings are to be found, never on the palm of the hand, and seldom on the sole of the foot. Dr. P. never failed to find them in pocks in which there exists a pit. That these apertures belong to the excretory ducts of the cutaneous glands is easily shewn by a perpendicular section, which brings the subjacent glands into view. The portion of the cutis that is not covered with pocks is also in a morbid condition, presenting everywhere a white puriform matter, which, just as in the pustules, adheres firmly to it, and wedges itself in between the plexus of vessels. If the vessels distributed on the upper surface of the cutis be subjected to minute examination in small-pox, which may be best done in skin artificially injected with colouring matter; a remarkable difference is found between those which lie immediately below the pocks, and consequently on their base, and those which surround their borders; for, whilst those which occupy the bases appear sunk in the substance of the cutis, and are not much filled with the colouring matter, those round their circumference project out of the substance of the cutis, and surround the seat of the pock in a radiated manner and in great abundance. Those, again, in the part of the skin remote from the pock, as can be well observed in the discrete form of small-pox, are far from reddening to such a degree the surface of the cutis; and, indeed, have often the appearance as if they lay deeper buried and in smaller numbers in the substance of the skin, and were only seen shining through it. These appearances may, Dr. P. thinks, be very well explained by admitting, with Gendrin, that, in inflammation, the smallest vessels are rendered altogether or partially impervious; whilst in congestion, on the contrary, they are dilated. There unquestionably exists the highest degree of inflammation at the base of the pock; and hence it is that the vessels in this situation are more difficult to fill with injection, whilst those on the circumference of the pock, having been

during life more in the state of congestion, become much filled with the injected matter and very prominent. Lastly, Dr. P. remarks that the so-called wind-pocks are by no means empty, but contain on their base, sometimes a dry matter that crumbles between the fingers, and sometimes, and indeed more frequently, a viscid soft mass: it appears as if only the more solid constituents of the puriform matter filling these pocks had remained, whilst the more fluid had disappeared.

The Cutaneous Glands and Hairs. The examination of the skin in a person who has died of small-pox brings to light the existence of cutaneous glands, even in those parts of it where, in the healthy state, they cannot easily be detected: for they are all more or less swollen, so as to assume a pyriform shape; and even their excretory ducts are frequently much distended by the secretion of the glands. If a pock be seated exactly on the spot where the excretory duct of a gland opens, the duct is raised upwards with the cuticle. In consequence of the stretching it thus experiences, it at length tears, and is destroyed by the pus in which it floats; so that, when suppuration has made a certain degree of advancement, the duct can no longer be found. But as, at an early period of its stretching, it still possesses some degree of firmness, it draws the part of the cuticle with which it is connected towards itself; and thus the pit or umbilicus is produced. Accordingly, as soon as the excretory duct has either been torn, in consequence of excessive stretching, or been destroyed by suppuration, the umbilicus disappears. There then remains upon the basis of the pock in the cutis, at this part, the above-described punctiform aperture, which is the passage of the excretory duct to the gland, and which can be particularly well seen if the portion of the skin to be examined has been kept for some time in spirit of wine; when these openings gape, in consequence of the contraction of the cutis. The destruction of the excretory duct is commonly accompanied with the falling out of the hair. Provided the whole gland has not been destroyed, the excretory duct, as well as the hair, may grow again: but, when this has happened, as Dr. P. has remarked, in such glands as were seated very superficially on the cutis, a new formation of gland and hair is impossible. The glands appear much more vascular than in their healthy state, which may account for their being commonly so full and so much swollen; for the mass of blood pressing towards the skin in general will excite the glands to the more powerful exercise of their function, and consequently to an increased secretion of sebaceous matter.

As regards the *Fila sudorifera*, Dr. P. professes himself unable to determine whether or no they undergo any morbid alteration in small-pox. At the time when he had ample opportunities of examining fresh portions of small-pox skin, the threads going from the cutis to the cuticle, to which the name of *fila elastica* was given, were usually regarded as the means of union between the cutis and the cuticle; and appeared of too little importance to deserve much time or trouble being expended on them. Now that attention has been turned to their real importance by Purkinje and Wendt, Dr. P. has had recourse to preserved portions of skin taken from small-pox patients, with a view to their examination, and has compared these with fresh skin in a healthy state. The only difference which he has been able to detect is, that the *fila* of the diseased

skin appeared larger than those of the healthy: but whether this might have been the case in the fresh state, and what changes they might have undergone in course of time by steeping in spirit of wine, are points respecting which his investigations lead to no conclusion.

We come next to the results which Dr. P. obtained from the examination of the internal parts of the body, in persons dying of small-pox. It is well known to be a subject about which much diversity of opinion has been entertained, whether the formation of small-pox pustules on internal parts does or does not occur at all; and, if it does, on what surfaces or in what organs? Dr. P. has given an exceedingly good summary of the results which former observers have stated themselves to have obtained from their investigations. Our space will not allow of our transferring this to our pages, as we could have wished to do; and we must therefore content ourselves with pointing out, as the principal sources of fallacy against which pathologists, even of the present day, require to be on their guard in judging of this matter, the liability of the glands that furnish the secretion by which mucous surfaces are lubricated to become enlarged in febrile and other disease, and subsequently to ulcerate, exhibiting, during the several stages of their progress, considerable resemblance to small-pox pustules. The fallacies into which some of the older anatomists seem to have fallen,—such as mistaking for small-pox pustules the little collections of muco-purulent matter that appear at the divided extremities of the bronchial ramifications, when in a state of inflammation, or possibly tubercles in parenchymatous organs and serous surfaces, would hardly mislead any competent pathological enquirer of the present day.

The Cavity of the Mouth and the Tongue. Sometimes, at the commencement of the disease, small vesicles are visible on the inner parts of the lips and cheeks, as well as on the point of the tongue; but it is impossible to trace the further development of these in the living subject, in consequence of their becoming covered with tenacious mucus. Dr. P. was frequently able, however, to continue his observations relative to them on the dead body, and from these he has deduced the following account of their course.

There appear, on the lips and inner sides of the cheeks, small white spots, of a round or oval form, the centre of which is very frequently somewhat darker in colour. The epithelium is at these places much softened, and at length rises so as to form a small white vesicle, which is at no period transparent, the softened epithelium remaining always opaque and white; it is incapable of any great expansion by the fluid collecting beneath it, and soon bursts. The subjacent mucous membrane is seen to be at some points eroded on its surface. The course of such a pock is consequently very brief; the constantly moist state of the mouth rendering its actual filling with pus and desiccation with the formation of a scab, altogether impossible.

The accurate investigation of the appearances which the tongue presents, is a matter of somewhat greater difficulty. It is well known that, in consequence of the papillæ on its surface, this organ is very uneven, and the epithelium, which surrounds the individual projections very closely, forms such an immense number of very small folds, that it is impossible, in the human subject, at least in the healthy state, to draw it

off as a continuous membrane. But, if small-pox matter be effused on the surface of the tongue, then, at the place where a pock is to arise, the epithelium being pushed off from the roots of the papillæ and pressed upwards to their apices, becomes loose, and appears to the eye as a white speck, in the centre of which also there is very frequently found a dark-coloured small circle or point. This circle or point, however, can be seen for only a very short time, as the whole of the raised part of the epithelium very soon appears white; and this also soon disappears entirely in the living subject, the tongue commonly becoming covered with a thick and variously coloured mucus. If the tongue be examined in the dead body, particularly after it has been subjected for some days to maceration, it is found that the coating of mucus, which is frequently several lines in thickness, can be very easily removed with the handle of the scalpel; the epithelium, which cannot be distinguished from mucus, and which indeed appears nearly dissolved into mucus, being removed at the same time. But, on the apex of each lingual papilla, there remains a trace of the epithelium, and giving to the papilla the appearance of a small mushroom, with its top adhering to the circumference of the stalk. On the sides, on the contrary, the papillæ are bare, and deprived of epithelium; as is best seen by subjecting some of them to the microscope. At the point where, at an earlier stage of the disease, there were seen in the living subject those white and somewhat raised spots that have been mentioned, there is found a depression of the mucous membrane, of a round or of an oblong or oval shape; in which last cases examination shews that the depression is formed by the coalescence of two contiguous pits. On a first inspection of such a depression, the suspicion readily occurs that it is formed by the loss or destruction of some papillæ; but Dr. P., having repeatedly had an opportunity to observe such depressions at their commencement, has satisfied himself that they are not produced by the destruction of papillæ, but by the papillæ being pressed aside by a fluid secreted between their roots, and accumulating there. That it is only in this way that these seeming holes in the tongue are produced, is also shewn by this circumstance, that the papillæ which form the sides of such a depression converge towards its basis, and consequently their apices only are separated from one another. Dr. P. is far from thinking, however, that the papillæ adjoining such a depression may not, sooner or later, be destroyed by suppuration; but that the commencement of these depressions is produced in the manner that has been described, without any destruction of their substance. Those papillæ which are in the immediate neighbourhood of such a depression, and which therefore form its walls and its margin, are somewhat enlarged, and are covered on their apices by a greater accumulation of mucus than the others. In the base of the pit itself, Dr. P. found, though not invariably, the excretory duct of a gland; and the mucous membranes frequently exhibited erosions of various depths. As to the mucous glands or follicles of the tongue, it is well known that they are so closely set on the back part of this organ as to make it appear almost as if covered with them. Dr. P. found them almost always very much distended, and with widely gaping mouths. This, however, is still more the case in the tonsils and in the mucous glands which are seated in the back part of the cavity of the mouth, in the soft and the hard palate, and in the uvula; in which

situations one can, without trouble, introduce a large sound into the open glands. In this state of the glands it can be very clearly seen how they are formed only out of folds or pouches of the mucous membrane. All of them abound in thick tenacious mucus. Dr. P. has never found pocks on any of these parts.

The Pharynx. Here also Dr. P. was unable, in frequent examinations, to discover small-pox pustules, or any trace of them; but the glands, particularly on the upper part of the pharynx, shewed the same fulness and gaping as those in the back part of the cavity of the mouth. They frequently bore a resemblance to a pock of the outer skin, flattened on its apex, so that an inattentive examiner might easily mistake them for such: but their true nature was fully established by their rounded openings, which could not be torn at any point; by their being obviously lined with mucous membrane; and by the principal orifice of the gland being often perceptibly connected with several other neighbouring orifices. The epithelium over the whole surface of the pharynx was thicker than usual, and could easily be stripped off in patches.

The Œsophagus. This portion of the alimentary canal Dr. P. found, in several instances, studded with true pocks. As on the external skin, so also in the Œsophagus, a fluid is secreted at a number of points on the surface of the mucous membrane, which detaches and raises the epithelium, and thus forms a pustule. The epithelium is in this way commonly very loose, and softer and thicker than in healthy subjects, and cannot undergo any considerable degree of elevation and distension without tearing. The pustule consequently flattens at a very early period, and discharges the fluid which it contains, so that it is very rare to meet with one that is unbroken; in place of which there is usually found only an abscess partially covered by the raised and loosened epithelium. It is obvious, therefore, that the perforation of the epithelium, produced by the bursting of the pustules, cannot have any determinate form; and accordingly the author found them to be sometimes round, sometimes triangular or quadrangular, or to consist of a mere fissure. If the epithelium be removed, as can easily be done, there can be seen upon the proper mucous membrane eroded portions, which correspond with the perforations and fissures in the epithelium, but are sometimes of greater circumference than one would presume from the external apertures: at other places, however, the mucous membrane is quite free from erosion, and, when the epithelium has been drawn off, all that is to be seen is an effused matter. These erosions, when minutely examined, Dr. P. always found to be round or oval; and he never saw them so deep as to have perforated the true mucous membrane. On the contrary, the *membrana vasculosa*, or cellular texture connecting the mucous with the muscular coat, was sound and uninjured. On the base of many of the pocks there was found a white body, which Dr. P. ascertained to be a gland. The glands of the Œsophagus, according to his observations, are seated, for the most part, below the proper mucous membrane; but as in the bodies of persons dying of the small-pox, which he examined, these glands were greatly distended with mucus, they could easily be seen through the thin proper mucous coat, particularly where it was eroded, and had the appearance as if they actually lay in the bottom of the small ulcer. But,

when the mucous membrane was carefully removed with a knife, the glands remained seated in the membrana vasculosa; whilst the mucous coat, thus stripped off, was seen to be unperforated; which could not have been the case if the glands were actually imbedded in it. Dr. P. very seldom found such a gland affected, when it lay accidentally in the true mucous coat. It appears not infrequently to happen that the epithelium of the œsophagus is quite destroyed, and as if completely converted into mucus, coming away, in washing the surface, along with the mucus which covers it.

The Stomach and Intestinal Canal. In the stomach Dr. P. found nothing that could be conceived to depend on small-pox, except very great enlargement of the glands and slight softening of the mucous membrane: but, in the rest of the intestinal canal, particularly in the small intestine, his attention having been several times called to portions of different sizes, and not of any determinate or regular form, by the circumstance of their not glistening in the light, like the surrounding mucous membrane, he found, on accurate examination with a simple microscope, that the papillæ of the mucous coat were apparently abraded, and quite wanting at these places, without its being possible to perceive in the abraded part an ulcerated base. The mucous membrane around these portions, as on other parts of the intestine, shewed no particular appearance, except occasional dendritic vascular injections and greatly enlarged glands passing into ulceration. These greatly enlarged glands, particularly the *glandulæ solitariae* of Brunner, might, Dr. P. observes, frequently lead people to suppose that they had found true pocks both in the small and in the large intestine; but, from a minute examination of the different forms of intestinal ulcers, he feels himself justified in expressing his conviction that those who have described small-pox as occurring in these situations had deceived themselves, and taken for small-pox pustules things of an entirely different nature.

Nasal Cavities. The results of Dr. P.'s examinations of these cavities he himself considers very inconclusive; for sometimes they were rendered imperfect, in consequence of his not being allowed to disfigure the body; and, in other instances, when there was nothing to prevent his doing so, the nasal canals were so filled up with mucus and pus hardened into crusts, that it was impossible to acquire an accurate knowledge of the state of the epithelium in the different stages of the disease, it being always found destroyed. The proper mucous coat frequently exhibited, after it had been cleared of all pus and mucus, ulcerated parts, of different breadth, depth, and form.

The Larynx and Trachea, with its Branches. Dr. P. is disposed to believe that he has found the mucous membrane of these parts more frequently affected in small-pox than that of any other organs. During the life of the patient, indeed, one might have suspected considerable alterations in the mucous membrane of the respiratory organs, from the great dyspnoea that occurred. Accordingly he always found it much inflamed, and the extremely tender epithelium, (which is here, however, much firmer than perhaps in any other situation,) could be drawn off with little trouble, although sometimes only in fragments. As in the state of health the connexion of the epithelium with the mucous membrane is so intimate

as to have led many anatomists to entertain doubts respecting its existence, it may be inferred that the destruction of this connexion must have been effected by the interposition of some extraneous matter. At this stage of the disease, however, Dr. P. could discover in only one instance a fluid effused between the two membranes, so as to present a distinct vesication: yet he thinks that, from what appears in the further progress of the disease, we are warranted in believing that such an alteration occurs in all instances. In addition to the traces of inflammation, the surface of the epithelium was studded over, as it were, with a number of small dark or black points; the gaping orifices of the glands. In a short time the epithelium, which here resists softening longer than in other situations, becomes here and there dim and opaque, so as partially to conceal the redness of the subjacent mucous membrane. These dim spots are more or less round in form, and in general of the size of lentils; they can be seen very distinctly on a trachea, the vessels of which have been successfully filled with red pigment, as they are thereby brought more clearly into view. We can then see, too, that these different dim spots are not all alike; for some allow the subjacent mucous membrane to shine through, in a greater or less degree, and others not at all. If the finger be drawn over them, they are felt to be prominent; which, indeed, might be inferred, as regards the larger of them, from the shadow which they cast. These dim spots are produced by the exudation of a fluid, which begins to be effused at the time when the epithelium, as above shewn, separates from the mucous membrane. By the turbid degeneration of this fluid, and its more rapid and copious effusion, the epithelium is still more raised; and the opaque white layer of mucous or puriform matter, forming between the two membranes, prevents the mucous coat from being any longer seen, as previously, through the epithelium. That this matter really lies beneath the epithelium, is proved by the circumstances that it cannot be washed off, and that, when the light is thrown properly on the portion under examination, it is reflected equally from the whole surface, so that the epithelium, which, in consequence of its greater degree of firmness, does not begin to soften till a later period, appears everywhere equally shining and regular: and that this substance is not produced by thickening of the epithelium is shewn by the fact that the last-named membrane can be stripped off it, of the same thickness as on other parts of the mucous membrane not so affected, but merely inflamed.

In a still more advanced period there occurs softening, and the consequent destruction, of the epithelium, bringing into view a very considerable growth and expansion of these little heaps of pus, so to speak, and their frequent coalescence, till at length the whole is changed into a homogeneous, mucous, and frequently a discoloured mass, which lines the air-passages, and below which, on washing it away, is found the mucous coat much inflamed, and here and there ulcerated. Frequently, however, Dr. P. found the disease of the mucous membrane limited to particular distinct spots, so that the appearances described were perceptible only at scattered points, and ulcerated parts were seen surrounded by parts nearly in a healthy state, just as on the external skin; with this difference, that the epithelium of the air-passages could be easily separated even from the parts which were not affected. These distinct ulcers

sometimes occurred in considerable number, being in some instances superficial, in others going deeper, and affecting likewise the submucous cellular texture. In all cases the glands were pretty constantly observed to be distended. Dr. P. has not seen any instance of complete perforation of the whole coats, or of ulceration extending to the cartilages. Whether appearances of the kind that have been described occurred also in the minute ramifications of the bronchi, he cannot determine, but he has certainly traced them into the third series of branches.

Mucous Membrane of the Genital Organs. The results derived from an examination of the mucous membrane covering both the male and the female organs of generation, were very inconsiderable; for, with the exception of the larger and smaller labia and the entrance of the vagina, in the female sex, and the inner surface of the prepuce and external surface of the glans, when not covered by the prepuce, in the male, Dr. P. found nothing remarkable. The small-pox pustules existing in these situations seemed to observe quite the same course as on the external skin; only they ran through their stages quicker, became earlier ripe, or did not reach the state of maturation at all, in consequence of their flattening before they had become filled with perfect pus, particularly on the nymphæ and the entrance of the vagina. In no instance could Dr. P. find inflammation on the mucous membrane of the urethra and vagina; but he several times found increased secretion of mucus, with great swelling of the glands of these parts.

The Serous Membranes. The only morbid appearance which was found in repeated examinations of the *arachnoid* was a slight collection of turbid fluid between it and the *dura mater*, and here and there the effusion of a mucous, gelatiniform matter, or plastic lymph, upon its surface; but these did not differ in any respect from what is found in the same situation after other diseases. The vessels lying under the *arachnoid* were very turgid, and everything indicated the state of congestion, or even of inflammation. In the same way, the *pleura* and the *pericardium* presented no peculiar pathological appearances: at least, the effusion of plastic lymph upon the pulmonary and costal *pleura* exhibited the same appearances which Dr. P. has himself repeatedly seen after pleurisy, or has found described and delineated by others. The *peritoneum*, however, presented some unusual appearances; for though, as in the case of the serous membranes of the head and chest, the serous covering of the intestinal canal exhibited nothing but what is usually remarked after inflammation of the intestines, (viz. the effusion of plastic lymph, and numerous adhesions of different parts of the intestine to one another or to the serous lining of the abdominal parietes, with frequently more or less effusion of serous or viscid fluid,) the part of the *peritoneum* covering the liver and the spleen presented appearances which Dr. P. thinks himself justified in regarding as the product of the small-pox; inasmuch as he has never himself seen anything similar in other diseases, nor found it described in others, notwithstanding the trouble which he has taken for this purpose. The appearances alluded to, and which presented themselves to Dr. P. in four instances, were the following:—The surface of the liver, as well as of the spleen, was found studded with white spots of different sizes and forms. The size varied between that of a lentil and that of a penny. The form appeared constantly circular, or somewhat

oval, surrounded with not sharply defined, but rather slightly rounded, margins. In some instances, two or three neighbouring spots had run together, so as necessarily to produce a small difference of form; the cause of which, however, was easily discernible. If the point of the finger was drawn over these spots, it was distinctly perceived that they projected somewhat, and, when they were scraped pretty firmly with the handle of a scalpel, a tender membrane separated from them. This, however, tore off at the circumference of the spots, at the place where the parenchyma of the subjacent organs, which could not be seen through the spots in consequence of their opacity, became visible. In a few instances only did a small portion of membrane separate in continuity with the portions described from the parts which were unaffected. With a pair of pincers the thin membrane could be separated from all the spots continuously; and, under a compound microscope, it shewed nothing but a homogeneous mass, which reflected the light in such a way as to appear granular. No trace of blood-vessels or lymphatics, nor of globules, laminae, or cells, could be discovered in it. On an accurate examination of these spots, after the outer membrane had been stripped off, nothing observable could be washed off from them, even by the aid of a sponge or the handle of a knife: they appeared like portions of the cellular texture filled with a white matter.

We have now given a pretty full view of this interesting and controverted subject, as given in the work of Dr. Petzholdt. We shall terminate our notice by transcribing the summary which he has given of the principal results deducible from his investigations of the morbid appearances found in the bodies of persons dying of small-pox.

1st. That there occurs softening of the undermost layers of the cuticle, and, at a later period, destruction of the connexion with the cutis.

2d. That orifices are never observable in the cuticle covering pocks.

3d. That what is called the pit, or umbilicus, is produced by the retention of the cuticle by the excretory ducts of the cutaneous glands.

4th. That all pocks have not a pit or umbilicus.

5th. That the pit, or umbilicus, never exists in pocks seated on the palm of the hand or sole of the foot.

6th. That, on the base of the pock, and consequently on the surface of the cutis, the orifice of the gland can be seen, except in the palm of the hand or sole of the foot.

7th. That the portion of the surface of the cutis not covered with pocks is also in a morbid condition.

8th. That the vessels at the base of the pock exhibit marks of inflammation; those at its circumference marks of congestion only.

9th. That the so-called "wind-pocks" are by no means empty.

10th. That the cutaneous glands are generally swollen in small-pox.

11th. That the excretory ducts of the glands, when destroyed, as well as the hairs that fall out, are regenerated.

12th. That the mucous membranes of persons affected with small-pox often exhibit inflammation, with the formation of vesicles and ulcers.

13th. That vesicles and superficial excoriation are met with on the tongue.

14th. That on the pharynx and back part of the mouth there are never

pocks, but the glands there seated are much distended, and have very wide orifices.

15th. That there occurs softening of the epithelium of the œsophagus, rising into pustules.

16th. That, at a later period, the epithelium is completely destroyed.

17th. That ulcers are met with in the œsophagus, but never with *perforation* of the proper mucous coat.

18th. That no pocks are found in the stomach and intestinal canal; but there occurs destruction of the papillæ of the mucous membrane, particularly in the small intestine.

19th. That there is frequently dendritic vascular injection of the mucous membrane of the alimentary canal.

20th. That ulcerated parts are occasionally found in the mucous membrane of the nasal fossæ.

21st. That vesicles form in the trachea;

22d. And ulcers occur in both larynx and trachea.

23d. That the mucous membrane of the genital organs is free from pocks, except at its junctions with the external skin.

24th. That on the serous membranes nothing more is, in general, to be seen than what is found after their inflammation in other diseases.

25th. That, in a few cases, an eruption was found on the serous coverings of the spleen and liver.

ART. XII.

On Diseases of the Rectum. By JAMES SYME, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh.—*Edinburgh*, 1837. 8vo. pp. 138.

WITH the style and general arrangement of the present volume we are not inclined to find fault, nor with the greater part of the matter; but we entertain doubts whether such a work,—in which nothing actually novel is introduced, in which, in fact, there is only a concise reconsideration of the several diseases by which the rectum is affected,—was really required by the profession. If we had not read the preface, we might, indeed, have had some difficulty in determining whether the work were intended for the profession or for the public generally. Such passages as the following, for instance,—“A collection of matter is formed under the integuments of the hip;”—“but as the matter is situated between the skin of the hip and the mucous coat of the rectum;”—“the presence of a flat induration in the hip,” and many similar which might be quoted,—seem so much wanting in precision that we could scarcely think them intended for medical men: and even the following sentence in the preface is rather ambiguous: “I have,” says Mr. Syme, “by a plain statement of the seat, nature, symptoms, and treatment of the different affections which are met with at the extremity of the rectum, endeavoured to assist practitioners in discharging their duty to the patient, and to protect patients against unprincipled or reckless practitioners.” We would fain hope that our comprehension of this passage is correct, and

that the author really means to protect the public by informing the practitioner; and yet, if this be his meaning, he is certainly chargeable with the want of precision. We cannot believe, however, that he has intended to do such a very silly thing as to seek to instruct those who cannot be substantially the wiser for his instruction; that he has attempted to make popular a subject which, above all others, is not likely to be mastered by the unlearned.

The first subject treated of is *Fistula in Ano*, respecting which the author says, "It is not easy to perceive why the slight incision required for its remedy is performed in the theatre of the hospital, with all the pomp and circumstance of a great operation." If it were a matter of moment, it would not be difficult to find excuses for this proceeding, although we believe the operation is very frequently done in the ward. Here, as in other cases, it is often desirable that the sufferers around should be as little as possible shocked by operations which are not necessarily done in the ward; and, besides, in the greater number of hospitals, there are students who are desirous of knowing something of such operations, and who have a better opportunity of acquiring this knowledge in a theatre than in the ward.—We think that, in enumerating the causes of abscess near the rectum, the author ought not to have passed over unnoticed hemorrhoidal tumours, which are perhaps one of the most frequent causes of such abscesses.

Mr. Syme states (p. 8,) "that, however long the fistula may be permitted to continue, no more than one internal opening is formed." Now, with the strongest moral conviction that this opinion is incorrect, our practice has never afforded us an opportunity of verifying, by an examination after death, the impression which existed on our mind. But, as the author thinks well enough of M. Ribes to quote him in support of his position, we will, from the same paper, quote him in support of ours: "In the greater number of cases which he has examined, (the whole amounting, in twenty-seven years, to eighty,) there was only one internal orifice; in some phthisical patients there were two; in one alone there were three." With regard to treatment, Mr. Syme says, "The inefficacy of all remedial measures except the knife for curing fistula, still remains unquestioned, unless by inaccurate observers or unprincipled empirics." This, we think, is stated too absolutely: there are, we believe, few experienced practitioners who have not had presented to their notice cases of fistula in ano, occurring in persons the state of whose pulmonary organs rendered it inexpedient to perform the usual operation, and yet in which it was necessary that something should be done for the present comfort of the patient. In such instances, stimulating injections have been used, and pressure prudently made; and now and then with complete success. We readily admit that this treatment is so uncertain, and the incision commonly so effectual, that no comparison between the two can hold: still this system has succeeded, and not only in such cases as we have indicated, and also in some cases in which no pulmonary complication was present, but in which the timidity of the patient, and perhaps also of the practitioner, has interfered to prevent the use of the knife.

It might be inferred, from the manner in which it is stated, that our author has been the first to discover that, in making the incision for the cure of this disease, it was unnecessary that the knife should be pushed

to the end of the sinus. We know, however, that, for many years, the practice of our best London surgeons has been, when they have been unable to discover an internal opening, which is an unfrequent occurrence, to make the probe pass through the thinned portion of the rectum, and to incise the part intervening between this point and the sphincter. This point is never at the extremity of the sinus; neither is the internal orifice. The following passage, from the article *Fistula*, in the Dict. de Med. et Chir. prat., published several years back, establishes the same fact: "Formerly a great importance was attached, on the one hand, to incise the rectum as far as the superior limits of its denudation; on the other, to remove, with great accuracy, all the callous structure by which the fistula may be surrounded. Experience has done justice to the painful manœuvres to which practitioners had recourse to attain this double object. It is in the present day well demonstrated that, when all the parts comprised between the fistula and the intestinal canal are divided, and with them a certain extent of the sphincter ani, the portions of intestinal membrane which may remain floating and deprived of cellular tissue at the superior parts of the wound, are not long in adhering."

We object to the distinction which has been drawn in surgical works generally, as well as in those, like the present, especially devoted to diseases of the rectum, between hemorrhoidal tumours, according as they are external or internal. There is no sufficient reason for such distinction, and it does not appear to us to answer any good purpose: if their structure were absolutely dependent upon their situation, this arrangement would be unobjectionable; but it is not so. A varicose tumour may, and often does, exist at the margin of the anus; an erectile tumour is not unfrequently seen in the same situation; and another variety, which may be described as encysted, is commonly found there. We think it would be much better that the distinguishing character should be their structure alone. For instance: two tumours may be identical in structure; one may be situated a third of an inch superior to the other; one may, "distended by inflammatory engorgement, project beyond the sphincter," the other does not: of what use is such a distinction in this case?

The account given by our author of *Hemorrhoids* is far from complete, and is in several respects inaccurate. For the sake of our junior readers, we will here give a somewhat fuller view of these common and very distressing ailments.

1. Hemorrhoidal tumours, depending solely on the dilatation of a vein, often occur: they present a more or less elastic, irregular, lobulated swelling, of a bluish colour, and completely disappearing on pressure; they may bleed, either in consequence of an exhalation consequent upon distension, or of abrasion during the passage of indurated fæces; they may be complicated by the formation of a fibrinous coagulum within the cells, or by an effusion of lymph into the neighbouring parts, and then they do not disappear upon the application of pressure. They are now and then found almost surrounding the rectum, a little way above the anus, forming a kind of collar during the effort of stool; their development is gradual, and usually they are not very painful. These simple tumours, however, occur less frequently than the others.

2. A second variety of hemorrhoids have their seat in the dense submucous tissue of the rectum. They are roundish, more or less elongated,

more or less tuberculated on their surface, sometimes pediculated, and of a palish red colour: their surface is sometimes bleeding, and occasionally dilated veins are seen to pass over them. When irritated and inflamed, they become very red and acutely painful: after having been inflamed they diminish, and as it were wither and become wrinkled, and may be seen pendent among the folds of the anus. These tumours sometimes disappear when the cause by which they were developed is no longer in action: under opposite circumstances, they increase and multiply. When recent, we find at their centre a cavity filled with blood; when of long standing, we may still find blood which has undergone change, surrounded by a delicate smooth cyst. At the internal surfaces of these cysts may be seen the orifices of very small vessels: these tumours are most probably a consequence of the extravasation of blood.

3. A third kind, which is commonly internal, is an erectile stricture; and we are quite at a loss to comprehend how Mr. Syme can have come to the conclusion that internal hemorrhoids can never have an erectile character, and "that erectile tissue must be congenital." Has he never known an erectile tumour in the uterus of a person advanced in life, or in the breast, or, in fact, in many other situations? and are not these accidental productions? Certainly this tissue is not an uncommon element in hemorrhoidal tumours; whether or not it be as frequent as a varicose state of the hemorrhoidal veins we are unable to determine. We are inclined to believe that, when hemorrhoidal tumours bleed periodically and freely, from very slight irritation, there is strong reason for suspecting that the hemorrhage does not proceed from the rupture of a varicose vein, but that it is the result of a vital action, of a transudation which occurs at the surface of an erectile tumour. When tumours of this sort are much distended and become ruptured, we perceive the spongy tissue of which they are formed, and not a single cavity dependent upon the dilatation of a vein. They are commonly single, varying in size from that of a pea to that of an egg. They frequently enter into a species of orgasm increasing in bulk, becoming very red, hot, and painful, and then exhaling arterial blood; and, when the irritation is past, returning to their former condition. The anatomical structure of these tumours seems to confirm the opinion of their nature: injection pushed into the hemorrhoidal arteries penetrates their substance, which then seems entirely vascular.

When inflammation attacks hemorrhoidal tumours its symptoms depend upon their character. In the case of the varicose tumour, there is burning heat, a sense of weight and distension; the varix soon opens, blood is poured into the rectum, and the symptoms speedily yield: sometimes the blood is poured out into the cellular tissue; lancinating, pulsating pain is felt; an encysted tumour is developed in the rectum or at the margin of the anus. When inflammation attacks an encysted tumour situated above the anus, there is lancinating pain with much heat, and a white puriform fluid, frequently mixed with blood, is discharged; after which the pain has suddenly ceased. If the tumour be projected beyond the anus, it becomes very red, hard, painful, and burning; the slightest contact augments the insupportable pain which the patient feels; afterwards it becomes softened, and, when opened, a puriform fluid, mixed with blood, escapes. In the treatment of "internal hemorrhoids" our author strongly advocates the employment of the ligature in preference

to excision; and this, we think, is the proper as well as the prevailing mode of treatment: but, when he says, "that he feels warranted, after very extensive employment of the ligature, to state, that it may be used without the slightest risk of serious or alarming inconvenience," he states a circumstance which is opposed to general experience,—even "when the ligature is tightly drawn." To say nothing of the pain produced by it, numberless cases might be adduced in which peritonitis and phlebitis have succeeded to its employment: we hold, therefore, that it is never used without risk of serious or alarming inconvenience.

We can see no necessity for the change proposed by Mr. Syme in the signification of the term *prolapsus ani*; viz. to confine this term to those protrusions in which the whole thickness of the gut is concerned, the other forms of the disease being all referred to the head of "Hemorrhoids." If any change were necessary, we should say that it would be more reasonable to confine the term *prolapsus* to the descent of the mucous coat, and use the term *invagination* to distinguish the descent of the whole: this distinction is certainly founded on pathological anatomy. Of the cases which occur in practice, there is not one in twenty in which the muscular descends with the mucous coat; and, where it does, it is almost always a case of intussusception or invagination of the superior part of the rectum. Mr. Syme gives us nothing new on the subject of treatment.

We readily admit that great comfort may be afforded to the patient suffering from simple stricture of the rectum by bougies, but we do not, like the author, admit that they effect a cure, except in rare cases. Like a stricture of the urethra, it may be dependent upon induration of one or more of the tissues surrounding the canal, and may be dilated; but rarely will the tissue be softened or the dilatation become permanent. We do not quite agree with our author as to the best mode of using the bougie: instead of the immediate removal as soon as it shall have passed the obstruction, we would recommend that it should remain the greater part of an hour, if it can be borne without inconvenience by the patient. When once dilatation has been procured to a sufficient extent, it must be preserved by the occasional introduction of the bougie ever after.

Fissure of the anus should not be confounded with spasmodic stricture of the rectum. The author himself admits that he has met with fissures producing great pain to the patient, unaccompanied with spasmodic stricture, and with spasmodic stricture unaccompanied with fissure. "In a considerable proportion of cases," he says, "in which the sphincter is firmly contracted, there is no perceptible fissure."

The history of *Fissure* is not sufficiently well known, and we cannot say that our author has thrown any new light on it. What are its causes? A number are assigned; but we doubt if any of these serve to explain the suffering by which it is characterized. The anatomical lesion is apparently extremely slight, compared with the symptoms. It is believed that a fissure of the same apparent character above or below the sphincter is easily cured: is it, then, any connexion which is maintained with the muscular fibres of the sphincter that causes the suffering? Many cases are given in which the fissures occasioned violent pains, though they had no discoverable connexion with this muscle. Is, then, the spasmodic contraction cause or effect? It is certain that constriction is often met

with without fissure, and fissure perhaps never without constriction: it is also certain that the section of the sphincter at any other point than that where the fissure is situated soon relieves the patient.

M. Boyer, for such reasons, maintained the first hypothesis; others adhere to the second, supporting their opinion upon the circumstance that ulceration may exist without being perceived. Although M. Boyer believed that the incision of the sphincter might be made anywhere, we think prudence would commend the choice of the fissured point: if, however, that point be at the median line anteriorly, the incision may be made at some other point of the circumference, as there may be some risk of wounding the vagina or urethra. Our author says it is not necessary to divide more than the external sphincter. We apprehend this is all which was desired by Boyer: still we should advise the surgeon to cut through all the muscular fibres which are called into action, or he may have to perform a second operation. The young surgeon must not, however, believe that the performance of this simple operation is without danger, and to be undertaken without careful deliberation; without, perhaps, attempting to cure the disease by dilatation, by lunar caustic, belladonna, and other means; as operations of this kind upon the pelvic organs not unfrequently occasion phlebitis.

We fear that certain cases of chronic inflammation of the rectum have been mistaken for cancer. The stricture, also, which attends an ulceration at this point occasionally misleads the practitioner, and is indeed a most fearful complication. It is, however, certain that, like a chronic ulcer of the stomach, which is now and then confounded with cancer of this organ, the chronic ulcer of the rectum occasions symptoms so analogous to those of cancer that it is confounded with it at the patient's bedside, and even sometimes upon the dead body. Stricture from chronic ulceration may be cured when it is accessible to dilating agents; and it is probable that it was in such cases, and not in true cancer, that Desault obtained such great success from gradual dilatation.

Our author says, "that, in common with other malignant affections, carcinomatous stricture of the rectum does not admit of being remedied by any kind of treatment, directed with the view of restoring the diseased part to its natural state; and its situation forbids any prospect of benefit from removal by the knife, or any other means." He further says, "If there are any cases in which this excision of the rectum has been followed by a permanent cure, the disease could not have been of a malignant nature." Now, we believe that carcinomatous matter, deposited spontaneously in a part in which chronic inflammation or ulceration does not precede it, is necessarily a disease of the system, and, so far as we at present know, irradicable; but we believe also that, where an irritation of a mucous, or perhaps cutaneous, surface has been long kept up, the part may take on a malignant character, and, unless removed, may end by destroying life; and yet the disease be purely local. Such cases, we apprehend, the author must have seen on the under lip, and on the penis: we would also maintain that a similar affection may exist in the rectum. In the lip, an ulcer may present all the characters of malignancy; but, if the neighbouring parts be unaffected, and the disease not too extensive, we may fairly hope for success: so in the penis; so, we believe, in the rectum. If the carcinoma of this part has succeeded to long-continued

local irritation,—if it does not extend beyond the reach of the indicator finger; if the surrounding parts are healthy, the intestine moveable and susceptible of being brought down easily; if the disease have resisted ordinary treatment, and the patient's life be threatened,—we hold that excision should be attempted; and, unless this disease be very far advanced, we think it may be performed with a fair prospect of success. M. Lisfranc has now operated on a considerable number of cases, and with very considerable success. The parts removed by him have been examined by some of the most accomplished pathologists of the French capital, and there has been no hesitation in admitting that the structure was malignant—schirrous.

ART. XIII.

1. *Memoirs on the Nervous System.* By MARSHALL HALL, M.D. F.R.S. L. & E. &c.—London, 1837. 4to. pp. 113.
2. *Observations on the Structure and Functions of the Spinal Cord.* By R. D. GRAINGER, Lecturer on Anatomy and Physiology.—London, 1837. 8vo. pp. 159.
3. *Powers of the Roots of the Nerves, in Health and in Disease; likewise on Magnetic Sleep.* By HERBERT MAYO, F.R.S., Senior Surgeon to the Middlesex Hospital.—London, 1837. 8vo. pp. 39.

BEFORE giving any detailed account of the works whose titles head this article, we think it proper to sketch, as briefly but comprehensively as possible, what we may regard as the present state of our knowledge of the general structure and functions of the nervous system; and in this summary we shall freely avail ourselves of the materials which their respective authors have contributed. When any important modifications in received views demand introduction, it seems peculiarly desirable that they should be examined, not only as to their separate validity, but with regard to their correspondence with what was formerly known or understood; for many who have devoted their lives to the pursuit of truth naturally become wedded to their habitual trains of thought; and, when a novelty has been announced, are more apt to test its merits by its conformity with their opinions than to examine how far it may really accord with the facts upon which those opinions have been formed.

No physiologist has any doubt that one office of the nervous system is to bring the conscious mind (using this term in its extended sense, to include the psychical endowments of animals in general,) into relation with the external world; by informing it, through the medium of the organs of sensation, of the changes which material objects undergo, and enabling it to act by its appropriate organs upon other beings, animate and inanimate: and that it is also concerned in the production of many changes in the system of the individual, immediately connected with the maintenance of organic life. The enquiry into the mode and degree in which the nervous system ministers to these purposes involves a great variety of questions, some of which we shall not at present dis-

cuss. Our immediate object is to determine the extent to which purely *mental* acts are concerned in the production of bodily changes, and through what instruments their influence is communicated; and we shall defer until the close of this enquiry what we have to say of the respective merits of the numerous individuals for whom a claim to rank as discoverers of important truths in this field of enquiry, has been raised by themselves or others.

We have remarked in a former article that nothing is likely to be known of the functional alterations in the state of the nervous system, since these are of a kind imperceptible to our senses, even when aided by various means of rendering them more apparent. The microscope and the galvanometer have alike proved useless in the most skilful hands. We know, or rather we have the means of knowing, as much of *nervous influence* as of electricity or gravity: of the existence of any of these as distinct entities we may reasonably entertain doubts; and we only take cognizance of them as properties of matter in particular forms. No researches, therefore, into the functions of the nervous system can ever unveil more than the *mechanism* (if we may use the expression) by which impressions are propagated through its fibres, and the properties by whose action on other structures a manifest change is produced. The similarity of these properties to those of some other agent might lead us to suspect the identity of their nature; but this would not give us any further information as to the abstract character of either. Our chief means of information, therefore, on the functions of the nervous system is derived from its effects on the other parts of the organism; and, by the attentive study of these, we may arrive at a knowledge of the conditions essential to its various operations, though we cannot precisely determine the nature of the operations themselves. Hence no enquiry into these functions can be complete which does not include an examination of the vital properties of the organs which it immediately influences: and this examination is particularly desirable at the present time, in order to correct the inaccurate notions which many have entertained regarding the dependence of organic life upon nervous influence; a doctrine which may be regarded as a remnant of the physiology of the Vitalists, which was scarcely less erroneous as a system than the chemical and mechanical theories which it displaced.

The property of *contractility* on the application of a stimulus is one of the most important of those enjoyed by living tissues. It is easily distinguished from that elasticity which is simply due to their molecular structure; the latter being retained as long as there is no evident decomposition, whilst the former is an essentially vital endowment. In the vegetable kingdom it is probably possessed, in a greater or less degree, by all the tissues actively concerned in the operations of nutrition and reproduction; and it is manifested under the influence of the normal stimuli of light, heat, air, moisture, &c., as well as, in some peculiar cases, in obedience to impressions of a mechanical nature. In the lowest and simplest animals, whatever degree of contractility is possessed appears to be almost equally diffused through the system; and we can neither discover in them any special structure peculiarly endowed with this property, nor anything resembling a nervous system fitted to call it into exercise. In proportion as we ascend in the scale, however, we find a distinct muscu-

lar structure evolved, in which the general contractility of the body becomes, as it were, concentrated; and, in proportion to its development and complexity, it supersedes the corresponding but more feeble powers of the remainder of the system. This view not only includes the animal function of locomotion, which in the highest classes depends entirely on the muscular system, (being accomplished in the lower by different means;) but also those parts of the functions of organic life which, in the more complex organisms, are subservient to it; and a very beautiful example of its application may be found in the circulation, which is evidently more dependent upon the impulse of its central muscular organ, and less aided by the vital properties of the general vascular system, in proportion as we ascend the scale.

The whole system of the vegetable is *immediately* dependent for its maintenance upon the action of external stimuli. All its vital properties are closely connected with the support of its organic life and the continuance of its race: all its energies are directed towards these ends. Each organ possesses a distinct vitality; since each, when separated from the rest, can perform its own function as long as the conditions essential to it are maintained. All are, however, blended and harmonized in the perfect plant by means of the circulating system; for the motion of fluid, unnecessary in the simplest organisms, where every part performs its share of all the changes necessary to the general existence, is required in those more complex structures which have a distinct organ appropriated to each function. There is in such beings no necessity for any other bond of union; nor do we find, in the ordinary phenomena of vegetable nutrition, any reason to suppose that such a one exists, since they can all be distinctly referred to the vital endowments of the several parts, brought into connexion with each other by the transmission of the nutritive fluid. But in some particular cases we observe evident motions performed in obedience, not merely to the ordinary vital stimuli, but to excitement of a mechanical kind. Such are the motions of the filaments of the Berberry, the closing of the fly-trap of the *Dionæa*, the flexion of the stalks of the pinnules and leaves of the *Mimosa*, and many others. In some of these we can trace a definite object connected with the performance of the organic functions, and it is beautiful to observe how this is attained. Nature, in effecting a new purpose, has accomplished it, not by adding an entirely new structure, but by modifying those already existing. In many of the instances to which allusion has been made, the sensible motions are the immediate result of the contraction of the irritated part; and we think it capable of proof that the property by which this is produced is but an exaltation of that common to most of the vegetable structure. In other cases, however, irritation of one surface produces motion of a distant part, as in the *Dionæa* and *Mimosa*; and this has afforded to the advocates of the existence of a nervous system in plants, a plausible ground for their arguments. But a full and cautious examination of the motions of the sensitive plant has shewn that this communication is effected through the medium of the circulating system, which not only serves as the bond of union between the organic functions, but even ministers to those which so nearly approach the endowments of animals. The contraction of the vesicles in the irritated part of the leaf forces a portion of their contained fluid through the vessels of the leaf-

stalk into the intumescence at its base, by the distension of one side of which curvature is produced.* Other cases might probably be explained upon similar principles.

In the lower classes of the animal kingdom we regard the evident motions as scarcely less directly dependent upon external stimuli than those of plants, being, in fact, the result of the general diffusion of that exalted degree of irritability which is restricted in most plants to particular parts. We cannot believe the motions of the gemmules of the sponges to be much more voluntary than those of the sporules of some Algæ, which are equally active;† and the contractile tentacula of the Hydra and other Polypes would seem to act in obedience to direct mechanical stimuli. In proportion as the food of the animal is, like that of the plant, constantly within its reach, do we find the apparatus for obtaining it simplified and expanded through the system; but, when more laboured means become necessary for bringing it in contact with the organs of digestion, we find a special apparatus superadded to, not superseding, that already in existence. Thus, the Hydra may be regarded as *all stomach*; and the contraction of the tentacula by which its orifice is fringed, upon any object accidentally brought within their reach, would seem to us similar to that of the muscular parietes of the alimentary canal in higher animals, the functions of which are stimulated, and the continuance of those functions provided for, by the action of more complex systems directed to this especial end.‡

The question as to the degree of sensibility which is possessed by the lowest of the animal kingdom is one which we have not at present the means of solving. The motions which follow the impressions of external agents are our only means of judging of it; and the analogies which we have pointed out would seem to indicate that, if these motions are accompanied with sensation, they are not dependent upon it. In fact, it is as impossible to draw a distinct line between the animal and vegetable kingdoms, guided by the possession or absence of the powers of sensation and voluntary motion, as by any other criterion; since, when these powers are exercised in a very faint degree, their manifestations do not perceptibly differ from those of mere organic irritability.§ A nervous system would seem to be only required in a being possessed of a number

* See Dutrochet sur la Motilité des Végétaux, § ii.; and Mayo's Physiology, fourth Edition, pp. 10-13.

† For a very interesting account of these motions, see the memoir of L. C. Treviranus, in the *Annales des Sciences Naturelles*, tom. x.; and that of J. G. Agardh, in the same Journal, N. S. Botan. tom. vi.—Another corroboration of the view here taken is the fact that the ciliary motions by which these gemmules and many animalcules are entirely propelled, are known to be completely involuntary in the higher animals.

‡ We can scarcely express the pleasure we have felt in the perusal of Hunter's Croonian Lectures on Muscular Motion, published for the first time subsequently to the composition of the greatest part of the present article. In the first of these lectures we find the following passage, which will be seen to coincide entirely with the views expressed above: "These actions [of vegetables] are similar to what arise in many animals from external stimulus, more especially those not endowed with sensation, and also to the actions of many parts of animals which do not appear to be directed or stimulated by the brain and nerves; as the actions of a polypus, which has no brain, and the peristaltic motion of the intestines in the more perfect animals, which does not arise from the stimulus of the brain and nerves."—*Hunter's Works*, 1837, vol. iv. p. 201.

§ In a note to the passage just quoted from Hunter, Mr. Owen remarks, that observation of the motions of the living polype will shew that *all* its motions are not performed

of distinct organs, whose actions are of such a character that they cannot be brought into mutual relation without a more immediate and effectual method of communication than the circulating system affords. "Mr. Hunter," says Sir G. Blane,* "by a happy turn of expression, calls the function of the nervous system *internuncial*. It is evident that some such principle must exist in the complicated system of the superior animals, in order to establish that connexion which constitutes each individual a *WHOLE*." Between the purely organic functions, the circulation, even in animals, is an important means of connexion; but the nature of the apparatus which is provided to ensure their continuance, and to adapt them to the conditions of animal existence, frequently (as in the respiratory organs) necessitates the intervention of the nervous system.

If we analyze, however, the connexions of the functions of organic life with those peculiar to the animal kingdom, we shall perceive that the actions of the latter may be divided into classes according to their more or less immediate relation with the former. Thus, in the very simplest cases in which muscular structure is introduced into the apparatus of organic life,—namely the heart and alimentary canal,—it is merely endowed with the property of vital contractility upon the direct application of a stimulus, and is not dependent upon nervous influence, though manifestly subjected to it for other purposes.† But were animals requiring food of such a nature as not to be immediately attainable, to be unfurnished with some other means of conveying it to their digestive organs than the organic contractility which the lowest and simplest possess, it is obvious that their existence could not be maintained: and, accordingly, we find the first appearance of distinct nervous cords in connexion with the buccal orifice of the alimentary canal. We can scarcely believe, however, that, as long as the whole surface is capable of receiving impressions, and no special organs of sensation have been evolved, this simple nervous system fulfils a much higher function than that which the spinal cord in vertebrated animals performs in relation to the actions of the constrictors of the pharynx and sphincter ani, the movements of respiration, &c. As we have before said, it is impossible to determine what degree of sensation and consciousness such beings possess; but we regard these psychical conditions as rather *superadded* to the chain of actions immediately connected with the organic func-

in obedience to external stimuli, but that some have the appearance of being voluntary. Without wishing to deny this statement, (since to do so would reduce the being to the level of a plant,) we may remark that these animals probably act, like vegetables, in obedience to other stimuli than those purely mechanical; the application of which may escape the most careful observation.

* Medical Logic, p. 133.

† It will be seen that we employ throughout the Hallerian doctrine of the independent irritability of muscular fibre; not only because it is perfectly consistent with what we know of the vital endowments of other tissues, which the opposite doctrine is not, but because all the most recent and well-conducted experiments appear to confirm it. We may especially notice those of Dr. J. Reid, (Fourth Report of the British Association, p. 671,) who has shewn that the exhausted irritability of a muscle is recovered as speedily when its nerve has been divided as when it is entire; and those of Dr. Madden, communicated to the last meeting of the Association, who found that narcotics, acting through the nerves, destroyed their power of conveying stimulation to the muscles, long before the irritability of the muscle itself was impaired.

tions than as links necessary to their performance. In fact, we think that it will not be difficult to demonstrate that the class of associated movements whose object it is to supply their materials, (if we may use the expression,) and to get rid of their excrementitious products, not only are independent of the will, though in part under its control, but do not necessarily involve sensation as a condition of their performance. The movements of respiration, for example, are not, strictly speaking, a part of that function, any more than is the action of the heart: the former ensure a constant supply of pure air, and the removal of that which has been vitiated; whilst the latter maintains a flow of venous blood to the lungs, and withdraws that which has been arterialized. If either of these conditions be impeded or checked, the real function of respiration is proportionally impaired.

In proportion to the exaltation of the sensibility, and especially to the evolution of the organs of special sensation from those more general forms which are presented in the lower parts of the animal scale,* do we find that the supply of materials for the maintenance of the organic functions is made to depend remotely upon their exercise; but still the actions immediately connected with the sensations excited by external conditions would appear to have but little of a voluntary character, and to involve no intellectual process whatever. These actions are usually denominated *instinctive*, and have been distinguished from the *automatic* motions, not merely of the heart and alimentary canal, but of the muscles of respiration, defecation, &c. We confess, however, that we do not see why the term *instinctive* should not be applied to *all* the actions which are performed in *direct response to an external stimulus*; and, in fact, we shall hereafter render it extremely probable that some of those which have been regarded as excellent examples of the restricted class to which we have just alluded,—as, for instance, the act of sucking in the infant,—are really not only involuntary, but not dependent on sensation.†

The highest class of muscular motions excited by the nervous system is that which is strictly voluntary, being produced by operations of the intellect, which are only remotely dependent on sensation. The will is capable, if sufficiently powerful, of modifying or controlling the involuntary actions to which we have already alluded; and every one must be aware of its influence in morbid states of the system. Thus, the muscular movements in hysteria and tetanus are, in their causes, equally independent of the will: in the former disease, however, a predominance of the will over the power which stimulates them may prevent their performance, although a deficiency in the exercise of the will too frequently

* There appears little doubt that a diffused sensibility to light and sound exists in animals which present no special organs of vision or hearing.

† It has been objected, by a distinguished physiologist, to this use of the term *instinct*, that "it is employed, both in common and scientific language, to denote a *mental* act; something of which our own minds are conscious, or of which we believe that other minds (whether of man or animals) are conscious." To this we reply, that the term *instinct* belongs to a confessedly doubtful and debateable ground; that any writer is therefore at liberty to extend or limit it as he pleases, provided he defines his own meaning; that *we* (as will subsequently appear) do not regard as *dependent* upon sensation, or any other act of mind, many actions which are usually considered in this light; and that many writers speak of instinct as a "blind impulse," an "organic necessity," &c., phrases which certainly do not imply any "mental act."

allows them free play; whilst, in the latter, the strength of the stimulus is such that no effort of will can resist or overcome it. The actions which are the result of intellectual processes appear, in general, but remotely connected with the gratification of the bodily appetites, when compared with those which are purely instinctive; and, in proportion to the difficulties which attend the acquirement of the means of maintaining the organic life, do we find the mental powers, adapted to a variety of conditions, predominate over the instincts whose scope is limited to a few.

The nervous apparatus being composed of two distinct structures, it is a matter of some importance to ascertain their respective functions before entering upon the discussion of the offices of particular portions of the system; and accordingly we find the second chapter of Mr. Grainger's book devoted to an examination of the "Properties of the Grey and Fibrous Substances." The opinion entertained by Mr. Grainger is, that the grey matter is the sole source of power, and that the white fibres are conductors only. In support of it he adduces the great vascularity of the former as compared with the latter, and the well-known immediate dependence of the functions of the nervous system upon the continuance of the circulation. This we regard as a very fair argument, and it is one which has been much insisted on from the time that the injections of Ruysch first substantiated the extraordinary vascularity of the cortical substance: it by no means proves, however, that the cortical substance is the *exclusive* originator of power; still less that it is the peculiar seat of sensibility.* Mr. G. also urges, in confirmation of his opinion, that "the grey matter increases in quantity in the exact ratio of the nervous energy." This we believe to be a general fact, but by no means so universal as Mr. G. supposes. We find that in those of the lower Articulata, whose motor organs appertain equally to each segment, the ganglia are nearly uniform in size throughout the body, as in the Myriapodes and larva condition of the insect tribes. In the perfect insect, where the legs, which in the caterpillar belonged to the nine posterior segments, have altogether disappeared, and where, in addition to the legs of the thoracic segments, two pairs of wings have been developed, the ganglia are found to be more or less concentrated in the anterior region, as Herold long ago demonstrated: but these enlargements are not directly connected with the motor nerves, or at least with the column believed to have that office, which passes over the ganglia without entering them. Mr. Newport has recently shewn, in a paper read to the Entomological Society, that there are similar enlargements on the motor columns of one of the Tenthredos, or saw-flies, where they pass over the ganglia, and where large nerves are given off; and stated that these are

* We can scarcely regard that as a valid argument against this hypothesis which we have known urged by more than one eminent physiologist,—that the possibility of exciting motions by stimulating a nerve separated from the brain disproves the idea of the origination of the motive power by the grey matter. A similar argument would lead to the inference that *no part* of the brain or spinal cord is the source of that influence by which muscular motions are excited; and that the impressions which produce *sensations* are not transmitted from the peripheral extremities of the sensory nerves, because similar sensations may be produced by pinching a nervous trunk disconnected from them. These effects are produced by the artificial stimulus *imitating* the changes ordinarily produced by the natural one.

entirely composed of *fibrous* matter, no grey matter entering into the structure of these columns. It is impossible to come to a definite conclusion on such contradictory testimony, and with such imperfect evidence.

That the office of the white matter of the nervous centres is similar to that of the nervous cords themselves, it would seem fair to infer, from their similarity of structure, were it not that, on subjects of this kind, we are constantly obliged to feel our uncertainty in arguing as to similarity of function, from the insufficient data which our imperfect means of research afford. That the *white matter*, wherever it exists, has a tubular structure seems now generally acknowledged; but whether the tubes are essentially different in character in the substance of the brain and that of the nerves must still be regarded as a controverted question. The *cortical* substance of the brain is stated by Ehrenberg to consist of a minutely reticulated plexus of blood-vessels, in the interstices of which are seen a number of free granules, apparently similar to the small colourless globules of the blood: these resemble in size, though not always in form, the nuclei of the red particles, and are believed by Ehrenberg, as well as by Müller, to be identical with the globules of chyle and lymph. In the cortical substance, especially towards its junction with the medullary, a delicate fibrous appearance is seen, which is apparently the commencement of the tubular structure of the latter; and it is conjectured by Ehrenberg that the detached granules may pass into the open mouths of these tubes. He has remarked also that the red globules in the vessels of this part are smaller than usual, and are partially deprived of their external envelope. In watching the early development, also, of the cerebral substance, he found it to consist of a coarse granular matter, from which cylindrical tubes were produced at a later period.

Highly appreciating, as we do, the importance of an accurate and extensive series of such observations, we cannot yet think them sufficient, either in number or uniformity of tendency, to bear the weight of any important inferences; and we think that many more facts must be collected from the sources laid open by anatomical research and pathological observation, before we can regard that as *proved* which, at present, is, certainly, at most but a plausible theory. When speaking of the structure of the spinal cord in Vertebrata, we shall adduce one or more facts apparently adverse to it; but we may recommend to the attention of our readers Mr. Grainger's own statement of his argument, with which we have no fault to find but its narrow basis and its onesidedness.

We shall now briefly sketch the principal varieties which are met with in tracing the arrangement and combination of these elements in the different divisions of the animal scale.—The group of Acrita is regarded as comprehending those classes in which no definite nervous system can be discovered. It is generally believed that, in the animals which belong to it, the nervous matter is present in a “diffused form,”—that is to say, incorporated with the tissues; but we see no reason whatever for such a gratuitous supposition. The simplest office of a nervous system is to establish a communication between parts specially modified to receive impressions, and others particularly adapted to respond to them. Where every portion of the body has similar endowments, there can be no object in such a communication; just as, where every part of the surface is

equally capable of absorption, and every part of the tissue equally permeated by nutrient fluid, there is no necessity for a circulating system. Many beings comprehended in this group manifest much less irritability than that possessed by the *Mimosa* or *Dionæa*; and the mere possession of this property to any amount may be quite independent of a nervous system; as was strongly and, we think, satisfactorily argued by Hunter, in the Croonian Lecture already referred to. The life of the Sponge and the Polype we regard as much more organic than animal; and such beings form the natural links of transition from one kingdom to the other.

The earliest distinct trace of a nervous system consists of a simple cord without ganglia, which in the Radiata is circular, in the Articulata longitudinal. As we rise in the scale, however, we find grey matter in the ganglia, which, in the higher Radiata, are disposed at definite intervals round the circle; and, in the Articulata, generally correspond with the division of the body into segments. In proportion as the endowments of all the segments are alike, the distribution of the ganglia is uniform; but, as the organs of special sensation increase in number and perfection, a concentration of the ganglia towards the head is evident; and, according as the locomotive apparatus of any particular segment is developed to a greater extent than the rest, does the ganglion of that segment exhibit an increased size. Anatomical research has shewn that the knotted nervous cord of the Articulata is not simple, but consists of distinct tracts; and that, independently of this, there is a system especially distributed to the respiratory apparatus, taking its origin from the cerebral ganglion, and another peculiarly connected with the viscera. It is usually believed that, of the tracts of the ganglionic cord, one is for sensation and the other for motion; the sensory nerves alone entering into the ganglia, and the motor column passing over them. If the view which is taken by Mr. Grainger of the structure and functions of the different parts of the spinal cord should hereafter be substantiated, we must seek for a different explanation of the endowments of the divisions of the nervous column of the Articulata: we shall presently allude to one that appears to us more consistent than that just given. In those Articulata whose several divisions have equal nervous endowments, the separation of the segments appears to produce little immediate influence upon their locomotive powers; and the movements effected by each portion are so simple and uniform that the general will would have little else to do than to control and direct them. In the articulated classes we see the instinctive propensities developed to their full extent, and the animal appears to be almost entirely guided by them. The perfection of the locomotive apparatus appears to be the governing principle in the structure of this division of the animal kingdom; and, accordingly, we find not only the nervous system but almost every other part of the body arranged on a symmetrical design.

In the Mollusca, on the other hand, the nervous system is connected with the maintenance of the organic life rather through the medium of the organs of sensation than those of locomotion. The extraordinary development of the digestive system of these inactive and often fixed creatures required that the animal functions should be immediately connected with the entrance to the alimentary canal. Accordingly we find that the ganglia are disposed around the œsophagus and connected with

others scattered through the body; and, in proportion to the development of the organs of special sensation do we find the nervous centres brought together into a situation superior to the œsophagus, so that in the highest cephalopodes the distribution of the elements differs little from that which is found in the lowest of the vertebrata.

In the Vertebrated classes we find united the forms which the nervous system has presented in the articulata and mollusca. The spinal cord, composed of cortical and fibrous matter, like the chain of ganglia in the former, traverses the body longitudinally, ministering particularly to the function of locomotion; whilst the ganglia connected with the organs of the senses, on which the supply of nutriment immediately depends, are placed in the neighbourhood of the alimentary canal,—not however, surrounding the œsophagus as in the mollusca, but entirely above it. In fishes the hemispheric ganglia and cerebellum make their first appearance as distinct organs; but they are quite subordinate to the cerebro-spinal axis and its ganglia. It is interesting to remark that the different forms which the nervous system assumes in this class, distinctly correspond with those which are typical of the sub-kingdoms whose peculiar characters are combined in it. Thus in the eel and other vermiform fishes, the spinal cord is of nearly uniform size throughout, and the cerebral ganglia are scarcely more prominent than those of the leech or caterpillar. In proportion as distinct organs of locomotion are developed, do we find enlargements of the spinal cord corresponding with the origins of their nerves, just as in the ganglionic column of insects; and where the anterior members are very powerful, as in the *Trigla* (gurnard), the spinal cord presents enlargements evidently ganglionic.* In such fishes as the *Lophius* (frog-fish), on the other hand, whose head and nutritive organs are enormously developed at the expense of the rest, and the locomotive apparatus principally in the anterior region, the nervous centres seem entirely confined to this division of the body; for the true spinal cord quickly separates into a bundle of distinct nerves analogous to a cauda equina.

We shall not minutely trace the progressive complications of the nervous system through the different classes of vertebrata, since our readers have other means of becoming acquainted with them. In proportion as we ascend towards the higher mammalia do we usually observe an increased development of the cerebral lobes or hemispheric ganglia with respect to the cerebro-spinal axis and the ganglia immediately connected with the nerves of sensation; the surface becomes convoluted, so as to augment the quantity of cortical substance; and the complexity in the arrangement of the fibres of the medullary portion greatly increases.

The anatomy of the spinal cord of vertebrata has been an object of frequent research; and the views taken of the relations of its different parts have necessarily varied much with the physiological doctrines which have been appended to them. According to the latest description of Sir C. Bell, the anterior roots of the spinal nerves are connected with the anterior columns upon which he believes motion to be dependent; whilst the posterior roots arise, not from the posterior columns, as he originally

* Mr. Owen informs us that these ganglia are found, in relation to sensitive organs, superadded to the pectoral fins. They would thus seem to resemble the thoracic ganglia of insects; and, if the latter should be shewn to be excito-motor, we suspect that these would prove to have a similar function. (See p. 500.)

supposed, but from the lateral. Other anatomists, however, take a different view; and Mr. Grainger (*Observations*, p. 30,) agrees with Mr. Swan in the belief that the anterior and posterior lateral fissures appear definitely to limit the two roots; or, in other words, that the nerves are entirely connected with the lateral column alone. Our own observations do not accord exactly with either of these views: but we are far from asserting that we are certainly right, and these eminent anatomists certainly wrong; as we have always regarded the determination of the precise mode in which the roots of the spinal nerves are connected with the nervous matter of the cord as one of the most difficult problems in anatomy.

The following is Mr. Grainger's account of the alleged connexion of the roots of the nerves with the grey matter of the cord.

"After the two roots have perforated the theca vertebralis, and so reached the surface of the cord, it is well known that their fibres begin to separate from each other: of these fibres some are lost in the white substance, whilst others, entering more deeply into the lateral furrows, are found to continue their course nearly in a right angle with the spinal cord itself as far as the grey substance in which they are lost: but this arrangement has no resemblance to the distinct division into fasciculi depicted by Mr. Mayo; on the contrary, it is with great care only that small delicate and individual threads or striæ, as it were, are traced dipping into the lateral fissure, and at length joining the grey matter." (*Grainger*, p. 34.)

"It is extremely difficult, owing to the delicacy of the parts, to determine the exact relations which exist between the above filaments and the grey matter, but in a few dissections I have been able to perceive these fibrils running like delicate striæ in the grey substance. In one instance, the fibres being more distinct than usual, an appearance was presented having a remarkable resemblance to that which is seen on making a section of the corpus striatum in a recent brain after the method of Spurzheim. My friend and colleague, Mr. Cooper, in this case counted distinctly five separate fibrils passing from the anterior root of one nerve, and there were some other fibres derived from the same root which were not so plainly seen." (P. 35.)

"From careful dissection, I am convinced that it is only a part of the fibres belonging to the two roots which are attached to the grey substance, and that a considerable number of threads are lost in the fibrous part of the cord. The exact mode of their connexion, however, with this latter substance is not known." (P. 37.)

Without pronouncing on the accuracy of this description, we do not think that Mr. Grainger was aware when he stated his results how nearly they corresponded with those of Bellingeri, published many years ago.* He quotes this author as having "attributed both to the anterior and posterior roots a triple origin; the former arising by two of its roots from the white fibrous parts, and by the third root, *perhaps*, from the grey matter; the latter arising by two roots from the fibrous part, and by the third from the posterior horn of the grey substance." Now the fact is that this description applies to the spinal cord in the human subject, in which Bellingeri confesses the difficulty of tracing the anterior root into the grey substance, although he maintains that several of the filaments pass into the antero-lateral grooves, where the anterior cornu of the grey matter approaches the surface; perhaps from the cause mentioned by Mr. Grainger, that the lapse of a few hours after death softens the parts so as to render this minute dissection impossible. But it is distinctly stated by Bellingeri,† that he *was able* to trace the connexion of the fila-

* De Medullâ Spinali. Aug. Taurinor. 1823.

† Op. cit., p. 160.

ments of the *anterior* root with the *grey matter* at the inferior part of the lumbar and superior part of the sacral region of the spinal marrow in the ox. Mr. G. does not tell us that he has traced this connexion in the human subject; and as the plates which he has given are taken from the dog, we infer that he has not. We do not see, therefore, that he can be said to have made any particular advance in the state of our knowledge on this subject: his researches possess merit, as tending to confirm or correct certain previous statements; and the mode in which his views are brought forward will probably excite more general attention than was given to the statements of Bellingeri.

We will now briefly state the result of our own observations and dissections; only remarking that the observations relative to the connexion of the roots of the nerves with the grey matter of the cord are drawn from recent examination of the spinal cord in the dog, made within a couple of hours after the animal's death, according to the suggestions of Mr. Grainger, and with the other precautions recommended by him.

In regard to the *posterior roots*, on opening the postero-lateral fissure at several places along its length, we could perceive, opposite the attachments of some of the pairs of nerves, though not of all, white streaks running into the grey matter. These striæ were decidedly nothing else than white nervous matter, (certainly not blood-vessels:) they were exceedingly delicate, but distinguishable by colour from the grey matter into which they penetrated; they ran from the lips of the fissure, and nearly at right angles to them into the grey matter. It is not improbable that these white tracts were continuous with some of the funiculi of the nervous roots; but this point we could not determine. In their aspect they correspond more with Mr. Grainger's description than with his figures, to the broad stripes represented in which these exceedingly fine striæ have no resemblance. In regard to the other and principal attachment of the posterior root,—viz. to the white substance,—our observations, as already stated, lead us to differ from Mr. Grainger's views, which are those of Sir Charles Bell. These anatomists restrict this attachment to what is usually called the lateral column of the cord; or, in other words, to the anterior border of the fissure. We are satisfied, however, that this description will not apply to all the nerves; for, in regard to some of the pairs, (for instance, those going to the upper extremity, which have very large roots,) the bundles of the posterior root have most certainly an attachment also to the posterior column.

As to the *anterior roots*, we have not yet been able to trace them beyond the surface of the cord; but we do not, therefore, dispute the accuracy of the descriptions of Bellingeri and Mr. Grainger. We must also here say that we have never yet been able to satisfy ourselves as to the existence of a fissure at this part of the cord. But, admitting that the irregular furrow on the surface, where the anterior roots come off, and the peak of grey matter corresponding to it within, really indicate a division of the cord at this part into an anterior and lateral column, we suspect Mr. G. is in error when he limits the attachment of the anterior roots to the so-called lateral column and the grey matter. As to the alleged connexion of these roots with the grey matter, we do not mean to deny it, although we have been altogether unable to perceive it; but the scattered mode of origin of the funiculi composing the

anterior roots,—some being placed more anteriorly, and others further back,—seems to us to be much opposed to the view that the anterior roots, in their attachment to the white matter of the cord, are confined to the lateral columns.

Mr. Grainger is of opinion that all those fibres of a spinal nerve which are not lost in the grey matter ascend, in an uninterrupted and independent course, to the brain, and exclusively form, as he supposes, the internunciate chords of sensation and voluntary impulse.

Now, though it would be difficult to demonstrate the incorrectness of this view by dissection in the human subject, yet it is evidently open to, if not subverted by, the following objection,—a sufficiently obvious one, but not foreseen, it would appear, by Mr. Grainger. Say that the spinal cord has a certain thickness where it communicates with the 28th spinal nerve, it must be thicker where the ascending fibrils of this nerve are added to those of the 27th; and, though at first the enlargement would be imperceptible, yet the increase of size should be obvious at the part where the cord contains the accumulated fibrils of all the spinal nerves. In rebutting this argument, an opponent might take advantage of the partial enlargements, due, according to Grainger's theory, to nervous matter in association with the automatic or reflex fibrils, and thus dispose of this additional matter: such an objection is not, it must be admitted, worth much. But, let us take a boa-constrictor, where the spinal cord has no partial enlargements; the cord is not thicker where it joins the brain than at the posterior quarter-part of the body: this fact seems to be incompatible with Mr. Grainger's ideas of its structure.

The following is the account which Mr. Grainger gives of the cerebral nerves:

“By minute dissection, it becomes apparent that these nerves resemble those of the vertebral canal, as from their similarity in function might be anticipated, in being attached both to the grey and fibrous substances. Their connexions, indeed, are in some respects more easily traced, in consequence of several of them arising in large trunks, whilst the two roots of the spinal nerves are spread out into single threads when they reach the cord. In this respect the third or motor oculi is particularly distinguished, and, as it may with strict propriety be received as the type of the motor nerves of the spinal cord, its anatomical relations demand special consideration. It is stated by Mr. Mayo that this nerve arises by many fibrils from the black matter in the crus cerebri. This account is perfectly correct as far as it extends, but it is not complete; for, as Mr. Solly has shewn, some of the fibres are attached to the motor cord in the pons varolii. But what is particularly interesting, is that after the fibres have spread out into the grey matter or locus niger, some of them may with care be followed into the fibres which constitute the upper portion of the crus. Now this part of the crus cerebri has been described by Sir C. Bell as receiving certain fasciculi derived from the posterior or sentient column of the spinal cord where it forms the calamus scriptorius of the fourth ventricle, and which subsequently pass upwards through the thalami nervorum optidorum to the cerebral hemispheres.” (Pp. 38-39.)

As the optic nerve is connected in its origin with this tract, a relation is established between it and the motor oculi, which is considered as affording a satisfactory explanation of the facts established by Mr. Mayo regarding their mutual action in producing closure of the pupil. Mr. G. thinks that by a similar continuity of fibres between the posterior and anterior roots of the spinal nerves, and of the portio major and minor of the fifth cerebral, established through the grey matter of the cord, may be

explained the mode in which impressions made on the nerves of the skin are transmitted to those of the muscles; this continuity he has not succeeded in tracing, but only infers it from analogy. The close approximation of the origins of the portio major of the fifth and of the portio dura of the seventh is another similar fact of great physiological interest; and was stated by Mr. Mayo some years ago, with several others of the same character, as leading to the belief that "nerves of motion take their rise from the same region or segment with those sentient nerves which transmit the impressions by which their action is usually regulated." We shall presently see that Mr. M.'s explanation will not apply to all the phenomena of sympathetic actions; although we have little doubt of the fact that a direct relation effected by a continuity of structure exists between all the excitor nerves and the motor nerves respectively called into action by them.

Mr. Grainger compares the constant relation between the size of the spinal cord and the nerves proceeding from it in the different tribes of vertebrata, with the variable proportion which the cerebral hemispheres present; and hence draws a fair argument for the independent power of the former. The enlargement of the column at the origins of the nerves of the extremities seems also to lead to the same inference. The doctrine founded by Mr. Grainger upon these and other anatomical facts will be best stated in his own words. He regards the spinal cord as composed of two structures anatomically and physiologically distinct from one another.

"One of these structures consists of the grey matter, confined, it must be recollected, not merely to the vertebral portion of the cord, but extending into the cranium, as far as the striated bodies, the optic thalami, and the optic tubercles. Now it has been shewn [?] that the grey substance, in general, is the source of the nervous power; and I believe it is further susceptible of proof that the portion of that substance which is lodged in the spinal cord is the source of the peculiar powers possessed by that organ; that, in fact, it constitutes, with the nervous fibres attached to it, the true spinal cord, the existence of which, as a structure independent of the brain, was first declared by Dr. M. Hall. The second portion of the spinal cord consists of the white fibres, all of which, after a most complicated disposition, seem to extend to the convolutions of the cerebrum and the layers of the cerebellum; it may, therefore, with propriety be called the cerebral part of the cord." (Pp. 45-6.)

Connecting this view with his previous statements he infers that

"In the compound nerves of the body there are in reality four instead of two different classes of fibres; and when the physiology of these parts is considered, it will be made apparent that these several fibres transmit different impressions: that of those going to the brain, the fibres derived from the sentient nerves transmit impressions which excite sensation, and those belonging to the motor nerves, volition; that of the fibres attached to the grey matter of the cord, those derived from the sentient nerves transmit impressions made on the skin to the true spinal cord, the peculiar power of which they excite; whilst those derived from the motor nerves transmit to the muscles the effects of the power thus excited." (Pp. 46-7.)

The connexion of the cerebral nerves he regards as not dissimilar.

"It is well known that whilst such nerves are admitted by some physiologists, the majority of writers in the present day contend that all the nerves contained within the cranium are spinal nerves. Neither of these opinions is correct; for each of the cranial nerves is in reality composed, like those attached to the vertebral part of the spinal cord, of a true spinal and a true cerebral portion: the former being attached to the prolongation of the grey matter of the cord, and the latter to the sensiferous and volition fibres

which ascend to the grey matter of the cerebral convolutions. The only nerve which perhaps consists of cerebral fibrils alone is the olfactory." (Pp. 47-48.)

It is well known that Dr. M. Hall proposes to admit as a part of his system of nervous physiology a set of excito-motor fibres distinct from those connected with sensation and volition; this admission we strongly opposed when formerly treating of this subject, and we cannot see that the facts brought forward by Mr. Grainger are yet sufficient to warrant it. It must be recollected that the inference which he deduces from them is entirely dependent on the opinion maintained regarding the respective functions of the grey and white nervous matter; and though we may consider the preponderance of evidence to be rather in favour of the doctrine held by him, we cannot consider this doctrine as resting on proofs sufficiently valid and stable to allow us to employ it as the foundation for another superstructure. Its application to comparative anatomy is not without its difficulties; thus Desmoulins contends that the grey matter does not exist in the spinal cord of fishes and reptiles; but that there is in the centre a canal filled with a serous fluid. "This statement," says Mr. G. "is, I believe, altogether erroneous; for, although it may be impossible to demonstrate in the cord of these animals the presence of a substance distinguished by its grey colour or consistence,* this circumstance is not sufficient to prove that a structure possessing the same properties as the grey matter does not exist." This explanation, we presume, would be applied by Mr. Grainger to the fact which we state on the authority of Mr. Newport, that in insects nothing but fibrous matter exists in those portions of the cords which are believed to be the motor, and he thinks not in the sensitive portions, except at certain points where they surround and inclose nodules of grey matter to form the ganglia;† but we cannot regard Mr. Grainger's explanation as altogether satisfactory; since the vascularity of the grey matter is not only its evident characteristic, but the source from which its peculiar properties, whatever they are, may reasonably be regarded as derived. Some of the difficulties arising from the examination of the nervous system of insects may perhaps be explained by an hypothesis of Dr. Hall's, which has a plausible aspect, when taken in connexion with Mr. Grainger's. He regards the ganglionic column of insects as strictly *excito-motor*; and the tract which does not enter into the ganglia, and is usually regarded as *motor*, he seems to consider as conducting the motive influence of volition.‡ If

* We have the authority of Mr. Owen for stating that grey matter does exist in the spinal cord of the turtle, crocodile, and python: and the same is stated by Mr. Swan, in the recent part of his "Illustrations of the Comparative Anatomy of the Nervous System."—REV.

† The manner in which the sensitive portions of the cords inclose the nodules of grey matter to form the ganglia, is shewn in the plates belonging to Mr. Newport's Paper in the Philosophical Transactions, 1834, part ii., plates 15 and 16, figures 32, 33, and 36. The nodule of grey matter is there shewn to be distinct from the sensitive portions of the cords which surround it.

‡ Since the above was in type, we have observed, in Dr. Hall's "Lectures on the Practice of Medicine," at present in course of publication, the following experiments bearing on this subject:—"I took a lobster, and laid bare the nervous columns. I first stimulated one of the ganglionic nerves: the muscles to which it was distributed, and *they alone*, were contracted. I then stimulated a ganglionic nerve. Muscles, both *anterior* and *posterior* to the part stimulated, were excited into combined action." (*Lancet*, Feb. 3, p. 650.) A great number and variety of experiments will, we think, be neces-

conjecture have any truth, we should regard this tract as corresponding with the cerebral portion of the spinal cord supposed by Mr. Grainger to exist in vertebrata; and therefore as ministering both to sensation and motion. There is certainly much correspondence between the structure of the double cord in the Articulata in general and the medulla spinalis of the Vertebrata, inasmuch as the ganglia always contain cineritious matter; whilst the cords which connect them and the nerves which proceed from them are almost entirely transparent and colourless, and differ very much in appearance from the granulated substance of the ganglia. Mr. Mayo may reasonably lay claim to having some years ago arrived at an opinion similar in its general bearings, which we quote from the second edition of his *Physiology*, published in 1829. "The cords which unite the nodules in the nervous systems of invertebral animals, we may presume, are intended to transmit reciprocally the influence of the different segments of the nervous system from one to another. The white fibrous strings which form the outside of the spinal cord in man and vertebral animals, have probably the same office." This opinion harmonizes well with the fact, not of very unfrequent occurrence, that when the arms have been palsied or permanently contracted without the lower part of the body being affected, the segments connected with the nerves of the former have been found upon dissection wasted, whilst cords of white matter have passed over them to the lower part of the spinal cord which has exhibited a natural appearance. On the other hand, it may fairly be argued that the enlargement of the cord where the nerves of the extremities are given off is produced by the accumulation of grey matter; and as these nerves are seldom put in action except voluntarily, there would seem but little cause for strengthening at these parts the supposed excito-motor portion of the cord. Again, there is no particular accumulation of grey matter in connexion with the origins of the respiratory nerves, whose associated movements, depending on the spinal cord alone, are in constant operation. Although we do not regard these and other similar facts as disproving Mr. Grainger's theory, we consider ourselves justified by them in withholding for the present our assent to it.

We believe that we have now put our readers in possession of all the essential facts which recent anatomical researches have contributed towards the elucidation of the different physiological questions on which we must now enter.

Before going further, however, we must specify the use that we intend making of certain terms, which, from their conveying ideas of an abstract rather than a tangible kind, have been peculiarly liable to be understood in a variety of senses. In fact, the difficulty of defining these terms so as to give a precise idea of the functions of the nervous system has been a fruitful source of unprofitable discussion, and has much retarded the progress of real knowledge. We cannot describe the properties of a sensation as we do those of the bile or the urine; and yet the production of the former is as much a function of the nervous system as the secretion of

sary to the determination of the precise functions of these nerves; and we cannot regard these as either decidedly confirming Dr. Hall's hypothesis, or as opposing the improvement of it which is above suggested. —REV.

the latter is of their respective glands.* The chemist judges of the properties of secretions by observing their action with various reagents whose characters are known; whilst the psychical changes produced by sensation are more obscure than the very object of our examination. The term *sensation* is used by metaphysicians in two distinct senses; first, to denote the communication to the conscious mind of an external impression conveyed to the sensorium; and second, to designate the mental condition which is the immediate result of such communication. In other words, it may imply either the *act*, or the resultant *product*. The term *secretion* is in exactly the same predicament. It is evident that *consciousness* is necessary to the production of any change in the state of the mind; and, therefore, to speak of sensation without consciousness is a contradiction. At the same time consciousness is not identical with sensation as some writers have maintained, but is that condition of the mind which renders it open to the reception of sensation; just as, to carry out the parallel we have already employed, the peculiar vital properties of an individual gland render it prepared to secrete a particular product when the blood which contains the elements of that secretion circulates through it. The term sensation is often confounded by physiological writers with *sensibility*, which is the capability of receiving impressions so as to give rise to sensation. We often find the brain or spinal cord spoken of as the seat of *sensation*, when the *property* of sensibility is all that can be strictly allowed to exist there. On the other hand, we find Mr. Grainger saying (p. 119,) "There is but one kind of *sensibility* possessed by animals; that, namely, which is *perceived* by the mind." The *property* of sensibility cannot become an object of perception until it manifests itself by an act of sensation, the *result* of which gives rise to the mental condition termed perception; the correct expression would therefore be, "that which is indicated by sensations of which the mind is conscious." The term *motion* has been used with similar inaccuracy; for we find it used by physiologists in three distinct senses; namely, the *capability* of producing motion (a property inherent in certain parts of the nervous system), the *act* of producing motion, and motion itself or the *result* of the act. We have entered into these details simply with the view of shewing the prevalent errors on this subject, and of enforcing the necessity of attention to accuracy among all those who aim at rendering metaphysical physiology as definite as the more tangible parts of the science.

Just as it is the function of the absorbents to convey to the centre of the circulation from all parts of the surface or interior of the body the fluid which they have imbibed, is it the property of certain of the nervous fibrils to transmit to the central sensorium the changes produced at their extremities. Until the mind takes cognizance of these impressions no sensation is produced; to whatever motor changes they may give rise, as long as the mind is unconcerned in them, their character is the same; but the consciousness of an impression is an act of *sensation*; and here the purely mental functions commence. Few physiological writers keep in view the distinction which is metaphysically required between sensation and per-

* We wish to be understood as referring to no distinctly *mental* act, but merely to that communication to the mind of an external impression which most physiologists believe to be effected by an organic change in the nervous matter.

ception, and the terms are frequently used synonymously. This is, however, a great error, since *perception* is, strictly speaking, the act of forming that elementary notion of the properties of the body occasioning the sensation, which is quite different from the sensation itself, and which must intervene before any reasoning process can be founded upon it.*

All acts of thought are either immediately or remotely dependent upon sensations; and if all its inlets were closed, the mind would remain dormant like the seed buried deep in the earth. The activity of the mind is just as much the consequence of external impressions by which its faculties are called into play, as is the life of the body the result of the excitement of its several vital properties by external stimuli; and just as many animals are capable of retaining a certain degree not only of vitality but of vital action, when deprived for a time of these stimuli, (as in hybernation,) so could the mind which had once been roused retain its powers by the recall of its former sensations, though debarred from the excitement of new ones.

We shall presently endeavour to shew that a certain class of muscular actions is as closely and necessarily connected with the excitement of *sensations* (these being usually of a *special* kind,) as another is with the stimulus of *impressions* only; and we think that it betokens a hasty and partial view to assert, as Mr. Grainger does, that, "if sensation be at all operative, it can only be so by exciting the will; for to say that an involuntary muscular action is the result of perception is to deny the involuntary character altogether." (P. 84.) All physiologists allow that the directly *emotional* actions, which are of course dependent upon changes excited by sensation, are independent of the will, and frequently opposed to it. It is only when these emotions are strongly excited, however, that the actions performed in obedience to them have this character; if less vehement, or partially subdued by the will, the excited emotion merely stimulates the intellectual processes to the formation of a desire (of which it then becomes an element) from which an act of volition results. The distinctness in the channels of emotional and volitional actions is beautifully evinced by pathological cases; since experiment is here entirely unavailing. We have, in our last volume (p. 500), recorded an instance in which the control of the will over the muscles of the face was entirely lost; yet, when a mental emotion was excited, the movements of smiling and laughing were performed as in the natural condition. Dr. M. Hall also mentions (*Memoirs*, p. 102,) a case of hemiplegia, in which the paralysed hand and arm were contracted and convulsed in the most extraordinary manner whenever the patient was excited by meeting an acquaintance, or in any similar way.

We shall not enter into a metaphysical argument on the nature and sources of *volition*, but leave it to the common sense of our readers to inform them how far this mental condition, resulting from the exercise of the intellectual faculties and the stimulus of the moderately excited emotions, is under the control of the individual himself: but we must observe, that in man its power may be exercised not only upon the body but over the mind. In the latter it maintains a general regulation over

* The term *perception* has, like *sensation*, a double import; being used to designate the act of forming the notion, and the *resultant notion* itself.

the passions and emotions, recalls and combines ideas into conceptions, and controls the sequences of thought. On the former it can only act by producing that change in the nervous system, which, when propagated to the extremities of the motor nerves, excites muscular contraction; and the result of this contraction is either evident motion or the resistance and prevention of the involuntary movements which would otherwise occur. This change, effected in the body by the influence of the mind, and transmitted from the centre to the circumference, is manifestly reciprocal to sensation: it has been designated as the influence of volition, but this is an objectionable term, since (as we have just observed) volition may be exercised in the direction of the mind as well as of the body. The term *motive action*, which has also been suggested, would be preferable, not only on this account, but as applicable to the similar change in the nervous system excited by purely emotional acts of mind. It is, therefore, extremely incorrect to speak either of the brain, the spinal cord, or the nerves, as the seat of motion: none of them can be regarded as such, any more than the retina or the optic nerve can be said to be the seat of sensation. It is the function of these parts to excite and propagate those changes which, by their action on other parts, produce the effects in question.

The emotions, passions, and propensities are all conditions of an analogous nature, but vary as to the degree in which they are connected with the operations of the intellect, or with the performance of the organic functions. The acts *immediately* resulting from all of them, however, are carefully distinguished from those of volition; though, when only moderately excited, they act by supplying motives to the will. In proportion to the degree in which they predominate, whether in health or disease, does the being lose his freedom of will, and his actions approach towards an instinctive character.

It will readily be perceived how great is the tendency to union in these two sources of action, volition and emotion. A purely instinctive action is altogether involuntary; whilst a purely voluntary action is the result of intellectual processes alone, and derives no stimulus from the propensities or emotions. A large proportion of the actions of man are of a mixed character, and result from the influence of the natural passions on the intellectual operations. It is in these alone, therefore, that the propensities act through the will; since the observation of the habits of animals, and satisfactory physiological inferences from their structure, tend to prove that their purely instinctive actions are not the result of reflection or judgment, but as directly and certainly follow the excitement produced by external agents as do the most purely emotional or other involuntary actions in man. In all the higher animals we find more or less of the reasoning power superadded; but the exercise of this power is probably always connected with the instinctive propensities of the being, instead of having an independent operation, as in man. We have no reason to believe that a brute can ever bring its will to bear upon its own mind, so as to control its irregularities or direct the sequence of its ideas; and this appears to us a grand distinction between the intellect of man and that of the highest quadruped.

We have thus endeavoured to analyze the psychical as well as bodily conditions under which muscular contractions may be produced; and we

shall now connect together these two classes of facts in such a manner as to lead to more particular inferences. Our object will be to determine what parts of the nervous system are severally concerned in the production of the voluntary and instinctive actions of the animal frame; and we think that this may be accomplished most certainly and readily by considering these actions in the order of their progressive complication. We have employed the term *instinctive*, here and elsewhere, to denote much more than is included under it by many writers: some have restricted it to one class of excited actions, some to another; but we think that it may be applied with the greatest propriety to designate all those changes in the muscular system which are immediately excited by impressions from without, which are not dependent upon the exercise of the will, though more or less capable of being controlled by it, and which, if acting alone, deprive the being of the character of a free agent. It is obvious, therefore, that these actions may be subdivided into several classes; and we think that the following arrangement of them will be satisfactory, both as regards the function they involve and the instruments by which they are effected.

1. Muscular motions directly excited by external stimuli applied to the organs which perform them, immediately connected with the functions of organic life, and incapable of being controlled, directed, or antagonized by the will. Such are the actions of the heart and muscles of the alimentary canal.

2. Muscular actions excited indirectly by impressions acting through the nervous system, but not of necessity producing sensation; less closely connected with the functions of organic life, though necessary for their *continued* performance; and generally capable of being, for a time at least, restrained or antagonized by the will. Such are the naturally associated movements of respiration, deglutition, &c.; the involuntary movements excited by application of artificial stimuli; the movement of the pupil, &c.

3. Muscular movements excited, still less directly, by the production of sensations which act upon the motor nerves without the intervention of reasoning processes; remotely connected with the maintenance of the functions of organic life, but generally having a particular tendency to the preservation of the system or the perpetuation of the race; and always capable of being controlled or directed by the will, when the latter is sufficiently powerful. Such are the involuntary movements excited by the special sensations, directed towards the acquirement of food, the construction of habitations, or the congress of the sexes; those concerned in balancing the body, and otherwise preserving it from suddenly impending injury; and those immediately connected with the excitement of the mental emotions; as also various acquired habitual movements. Each of these classes we shall now consider in detail.

I. Of the first we have but little to say, as it is not our intention to enter upon the old and, we had hoped, decided controversy regarding the source of the irritability of muscular fibre. The analogies we have drawn from the vegetable kingdom must be allowed some weight; and we can only stop to express our surprise and regret that Mr. Grainger should have so far suffered his usually discerning mind to be led astray by a favorite hypothesis as to suppose, not only that the contractions of the

heart and alimentary canal are of an excito-motor character, the sympathetic nerve being their centre; but that the motions of plants may be "readily explained upon the principle of the reflex action." It certainly does appear to us a little strange that Mr. Grainger and other writers cannot allow contractility to be a property of living vegetable or animal tissue, as much as the capability of producing organic compounds or any other vital endowment. No vegetable physiologist of any intelligence now finds any difficulty in the "presumed necessity of sensibility in plants;" and no anatomist that we are acquainted with now imagines that the traces even of a nervous system can be discovered in them. It certainly behoves those who maintain that the irritability of muscular fibre is dependent on nervous energy to bring forward positive proof of it; as the *onus probandi* cannot be regarded as resting on those whom every analogy supports. Mr. G.'s deficiency in clear ideas on the subject is further shewn by his remark (true in itself) that "the first great step in the path of improvement (regarding our knowledge of the relations between nerve and muscle) was made by Haller, when, completing what was begun by Glisson, he succeeded in proving the distinctness of the muscular property, contractility, and of the nervous power, sensibility." If contractility be a distinct property of muscular fibre, where is the necessity for supposing, contrary to all observation and experiment, that the action of the heart depends upon nervous influence? With regard to the individual cases in which this application of muscular contractility is exercised in the living body, it may be observed that it is immediately connected in all with the performance of the organic functions, (to suspend which, even for a brief space, would be fatal;) and that this muscular apparatus forms a part of those additions to the essentially vital organs which are required by the conditions of animal existence.

II. The second class of instinctive actions cannot be dismissed so briefly, since to establish its real nature is the main purpose of the present article. We shall defer until its close what we have to say of the past history of the enquiry, concerning ourselves at present merely with the facts which it has disclosed, and the inferences which we think ourselves justly entitled to found upon them. The actions comprehended in this class are of a very diversified character; those originally belonging to it are probably all connected closely, either with the maintenance of the functions of organic life,—as the movements of deglutition with the supply of the digestive system, or those of respiration with the continued aëration of the blood; or with the preservation of the body from being injured by external agents, whose presence is usually recognized through the organs of common sensation,—as when a limb is involuntarily withdrawn from a flame, a pinch, a prick, &c.

All these subdivisions, which differ only in the tendency of the actions they respectively include, agree in the channel through which these actions are performed. In all it is necessary that the nervous communication from the part excited and the part exhibiting motion should be complete with the spinal cord,* and that the division of this organ, with

* Here and elsewhere we include in this term the medulla oblongata and the spinal origins of all the nerves. Similar views, expressed in a more general form, will of course apply to these parts of the ganglionic system of the Invertebrata which correspond with the cerebro-spinal axis in higher animals.

which the nerves conveying the impressions are connected, should be continuous with that from which the motor nerves arise. Frequently, and indeed generally, the *afferent* and *efferent* nerves are connected with the same segment of the cord; and, if this be entire, nothing more is necessary for the excitement of actions of this class by their appropriate stimuli. We shall now consider in some detail the character of each of the subordinate divisions to which we have adverted.

In the adult human being, the motions adapted to the ingestion and mastication of aliment, which were originally dependent on separate acts of the will, are probably performed subsequently through a shorter channel. The action of sucking in the infant, however, is from the first purely instinctive in its character, and belongs to the class we are now considering. The experiments provided for us by nature in the production of anencephalous monstrosities fully prove that the nervous connexion of the lips and respiratory organs with the spinal cord alone is quite sufficient for its execution; and the following experiments, performed under Mr. Grainger's direction, still more clearly reveal its nature.

"The brain was removed in a young puppy, which was then put to a large bitch, not the mother, but which was suckling at the time. The puppy, on touching the mamma, threw up its nose and moved the mouth, trying to get hold of the nipple; which, however, was too large. My friend and colleague, Mr. Barron, to whom I am much indebted for the performance of this and many other experiments, then moistened his finger with sugar and water, and put it into the mouth, when the puppy sucked, the tongue being wrapped around the finger. Mr. Barron also observed that the movements became more distinct when he pressed against the tongue.

"The brain was carefully removed in another puppy: the animal performed the same actions as in the last experiment, only that they were more vigorous; which was attributed to the hemorrhage being less in this than in the former instance. But the most remarkable fact was, that, as this puppy lay on its side sucking the finger, it pushed out its feet in the same manner as young pigs exert theirs against the sow's dugs." (P. 80.)

Our opinion as to the degree in which sensation is implied in this action must necessarily depend upon the view which we take of the functions of the spinal cord in general; and, though nothing in these experiments affects the question most at issue, yet the following interesting facts on the same subject must be allowed some weight.

"Nature, in the generation of the Marsupialia, displays, among the other extraordinary phenomena observed in those animals, several which are the exact counterpart of the actions of the anencephalous infant and of the animal deprived of its brain. In this interesting class of animals the fœtus quits the uterus at a very early period: in the Virginian Opossum, uterine gestation continues twenty-six days; and in the Kangaroo, according to the accurate observation of Mr. Owen, only thirty-nine days. The fœtus of the latter animal resembled an earth-worm in its colour and semi-transparent integument; adhered firmly to the point of the nipple; breathed strongly but slowly, and moved its fore-legs when disturbed. Its whole length, from the nose to the end of the tail, when stretched out, did not exceed one inch two lines. The brain in a mammary fœtus, one inch and a half in length, corresponds to the same organ in the human fœtus at the ninth week. Are not these the very actions of an anencephalous infant, or of an animal without its brain? The mammary fœtus breathes, moves its limbs *when touched*, and by the contraction of its mouth grasps the nipple. It is true it does not suck, because at this early period the muscular force is insufficient for such a continual action; and hence the beautiful provision of the compressor muscle described in the interesting memoir of Mr. Morgan; but it is certain that the

lips, when they first are touched by the nipple, must contract upon and grasp it; and that subsequently the same action must incessantly be continued till, as the development proceeds, the fœtus becomes at times separated from the nipple. Can it be imagined that in this case there are sensation and volition in what can be proved anatomically to be a fœtus?" (Pp. 81, 82.)

The analysis of the actions next concerned in the supply of food to the digestive system is very important to the present enquiry. It is now generally allowed that the will has no direct power over the muscles concerned in the act of swallowing, and that an impression on the fauces is essential to the excitement of their actions. Until recently, however, it was believed that, though this action is constantly being performed during sleep or in profound coma, *sensation* is a necessary link in the chain of causation. In the act of deglutition, under ordinary circumstances, sensation accompanies the impression which acts as the real stimulus; but, as this action is performed equally well when the brain is removed, and when (as we shall presently shew) there is no sensation, we cannot regard it as *dependent* even on this primary mental condition. It was originally stated by Dr. Hall (*Memoirs*, p. 7,) that, when the food has once passed the fauces, its progress down the œsophagus is caused by the irritability of the muscles immediately excited by contact, as in the lower part of the alimentary canal. Dr. Reid's experiments have shewn, however, that the contraction of these muscles is occasioned, like the action of deglutition, by the impression propagated through their afferent nerves to the spine, and reflected by the efferent motor fibrils; and that the continuity of the nervous circle is therefore essential to it.* No one is conscious of any sensation accompanying the usual passage of food down his œsophagus; so that, as this action exhibits the necessary conditions in their simplest form, we can scarcely doubt that the movements of deglutition are excited by a similar influence. With regard to the nerves concerned in these actions there is much difference of opinion. By Mr. Mayo and Professor Müller it is believed that the glosso-pharyngeal is both a sensory and motor nerve; and Mr. Grainger regards it therefore as partly a sensory nerve, conveying impressions to the brain from the base of the tongue and fauces, and partly a true spinal nerve, exciting, by its incident and reflex action, the dilatation of the pharynx (by the contraction of the stylo-pharyngæus muscle,) consequent upon the contact of food with the fauces. Dr. Reid, on the other hand, regards it as only a sensory and excitor nerve; believing the pharyngeal and œsophageal branches of the par vagum to be the motor nerves of the muscles of this part. He found that section of the glosso-pharyngeal nerve by no means prevents deglutition, the stimulus being conveyed by other nerves; but that, when the pharyngeal branch of the par vagum is divided, the muscles are paralysed, and that small morsels only of the food can be forced into the œsophagus by the action of the tongue and flexion of the neck. The lower part of the œsophagus depends for its contractility upon the

* As we do not perceive that Dr. M. Hall has anywhere corrected the above misstatement, we cannot regard it as either just or generous on his part to say, as he has recently done, (*Lancet*, Feb. 17, p. 735,) "Dr. Reid's experiments confirm my own in every respect, and, I think, add nothing to what I had done." To Dr. Reid appears to us justly due the credit of having first established a clear case of *reflex action without sensation*, as one of the ordinary functions of the nervous system.

œsophageal branches of the par vagum, part of which seem to act as excitator and part as motor nerves. Dr. Reid very justly remarks, that we have here a satisfactory instance of the excitement of motions by simple impressions unaccompanied by sensations. "The food is propelled along the œsophagus without our consciousness and without our volition; and yet we have seen that, before the presence of ingesta in this tube can excite its muscular fibres to contract and propel their contents onwards, the same conditions of the nervous system are necessary as for the production of those sympathetic or instinctive actions which are not excited by mental acts."*

It was ascertained by Leuret and Lassaigne that the sphincter of the cardia is paralysed by division of the pneumogastric nerves; and the actions of this orifice, entirely independent of sensation, are therefore due to the maintenance of its nervous connexion with the spinal cord. The experiments of M. Brâchet tend to prove that the motions of the muscular parietes of the stomach, so essential to chymification, are also due to the influence propagated from the medulla oblongata, in obedience to the impressions conveyed to it, the par vagum being the channel of communication in each instance. Although the results of similar experiments performed by other physiologists have been of a different nature, we are inclined to believe that this is the true explanation; and we understand that the recent experiments of Dr. J. Reid dispose him to adopt a similar conclusion.† We can scarcely regard it as doubtful that, in the lower part of the alimentary canal, muscular action is altogether independent of nervous agency, and occurs in its simplest form, being excited by stimuli applied to the fibres themselves.

The foregoing is a brief sketch of the successive stages of the connexion between the animal powers and the most important of the organic functions. We have seen that, in man, the purely voluntary motions required to obtain and convey food to the mouth are followed by others which habit probably renders independent of the will, the whole process being in the infant a purely instinctive action; that these movements give place to others in which sensation even is not required, but in which the nervous system appears to act simply as a conductor; and that the object of these is to supply the stimulus to others immediately connected with the organic functions themselves, and not involving any vital property of a purely animal character. If we were to contrast these consecutive stages with those which a similar analysis of the actions of the lower animals would present to us, we should find that, as we descend the scale, we gradually lose the connexion of volition, next of sensation, and finally of any nervous system whatever, with the actions ministering to digestion; and that, in the simplest animals, as in plants, the necessary motions appear to result from mere excited irritability.

Some difficulty exists with regard to the character of the powers by

* Edinburgh Med. and Surg. Journal, No. xlix. p. 155.

† Two of his experiments already published countenance the above supposition, (*Op. cit.* p. 154;) and the distribution of the great nerve to the stomach in insects is so analogous to that of the par vagum in vertebrata as to afford some anatomical confirmation of it. We cannot, however, regard it as very improbable that the stimulus of innervation may be confined to the cardia, and that the muscular contraction there excited may be propagated along the stomach, as in the lower part of the alimentary canal.

which the other extremity of the alimentary canal is guarded; and we shall again look for assistance in comparative physiology. The formation of excretions is an act purely organic, and is performed by plants as well as animals. In the former the act of excretion is as constant as that of absorption; and in the lowest of the animal kingdom it would seem to be on the same footing, being as involuntary as the introduction of aliment; and both processes continue, with no observable intermission, during the life of the being. In more complex organisms, where the aliment is retained for the purposes of digestion, the discharge of excrementitious matter from the cavities provided for its reception is only occasional; and there is therefore a demand for a special set of associated movements to carry this purpose into effect, these movements being involuntary in their character, and excited by the quantity or stimulating quality of the contents of the reservoir. But, had volition no control over them, great inconveniences would arise: hence, sensation is excited in the perfect organism by the stimulus which would of itself produce the movements, in order that, by arousing the will, the otherwise involuntary motions may be to a certain extent restrained or directed. There can, we think, be little doubt that, from the experiments of Dr. M. Hall,* as well as from other considerations, that the associated movements by which the rectum and bladder are emptied correspond with those of deglutition, except in being capable of greater voluntary restraint or assistance; whilst the discharge of the contents of the vesiculæ seminales would seem to be completely automatic, and thus to bear a close resemblance to the action of deglutition. Here, again, we have evidence that the unbroken connexion of the nerves with the spinal cord is all that is requisite, and that sensation does not necessarily intervene in their production. These expellent actions are antagonized by the sphincters; and the question of the nature of their action is one of considerable difficulty. From an experiment of Dr. Hall's, (*Mem.* i. § 37,) as well as from the observed actions of anencephalous fœtuses, it would appear that the sphincter ani is entirely dependent upon the spinal cord, and that its contraction is of a simply automatic kind. On the other hand, the sphincter is certainly in part controlled by the will, and made to act in obedience to the warning given by sensation; and this voluntary power is frequently destroyed by injuries of the brain, when the spinal cord is healthy, and when the motions of respiration and deglutition are normally performed. We are inclined to believe that, in their moderate action, the expulsors and the sphincters balance one another as far as their excito-motor character is concerned; that, when the quantity or quality of the contents of the rectum produces an excessive stimulus to the former, their action predominates, unless the will is put in force to strengthen the resistance of the sphincter; and that, when the stimulus is deficient, the will must aid the expulsors, in order to overcome the excited contraction of the sphincter depending on the spinal cord alone. We are much mistaken if this view will not reconcile the difficulties under which Mr. Grainger labours on the subject; and at the same time we would refer our readers to his interesting discussion of it. A precisely similar view may be applied to the associated movements concerned in urination.

* Memoir, ii. § 172. &c.

The movements concerned in the function of respiration constitute a very interesting group, which we fully agree with Dr. Hall and Mr. Grainger in referring to the present class. That the medulla oblongata is not their centre, otherwise than by serving as the medium of communication between the afferent and efferent nerves, we are perfectly convinced. That the actions of deglutition and respiration, which are most essential to the maintenance of the organic functions, should be connected with a part of the cord most secure from injury, can excite no surprise; and, when this connexion is borne in mind, it affords a full explanation of the influence of the medulla oblongata upon the general vitality of the system. It is also to be recollected that, being the part of the spinal cord which is connected with the brain, its relations with the functions of that organ are necessarily of a very important character. No motions can be produced by mental influence, except through its intervention, and the communication of, probably, all the impressions by which sensation is produced takes place through its medium. Hence we have no difficulty in understanding its importance, without having recourse to any vague suppositions of its peculiar vitality.

What is the nature and channel of the stimuli to the ordinary movements of respiration is a question not yet fully decided. There can be no doubt that the spasmodic closure of the glottis, occasioned by the contact of foreign substances or of injurious gases, is due to an impression conveyed by the incident portion of the pneumo-gastric nerve, and reflected along its motor fibrils; and Dr. Reid's experiments seem to prove that the superior and inferior laryngeal act in these capacities respectively. It is believed by many physiologists, and by Dr. M. Hall and Mr. Grainger among the rest, that the impression produced upon the pulmonary extremities of the par vagum by the presence of carbonic acid or of venous blood in the lungs, is the ordinary stimulus to inspiration; and they rely partly upon the experiments of M. Brâchet in proof of this opinion, and partly upon the fact stated by Dr. Hall and Mr. Broughton, and confirmed by Dr. Reid, that compression of the nerves is followed by powerful respiratory movements. A great difficulty arises, however, from the fact that, after this nerve has been divided, the regular respiratory movements continue, although generally more slowly than usual. This is attributed by Brâchet to *custom*; by Dr. M. Hall to *volition*; and by Mr. Grainger to the continued conduction of the impression by the sympathetic nerve, which appears to be also the opinion of Dr. Reid. The first explanation we regard as entirely fallacious, since no habitual actions are excited without a stimulus. Dr. Hall's view is supported by a statement, which is certainly an important one if entirely correct, that respiration will not go on when both the cerebrum has been removed and the par vagum divided, though either operation may be performed singly without checking the movements. Dr. Reid, on the other hand, states that the motions continue after section of the nerves, even when all volition has been destroyed by a dose of prussic acid; and he has also proved that the anastomosis between the superior and inferior laryngeal nerves (to which we should otherwise have been inclined to attribute the effect, where the nerve, as is usual, is divided between their origins,) is not the means of sustaining them. We cannot pass by Mr. Grainger's experiments on this subject without noticing a serious

objection to which his mode of performing them is liable. When unmutilated animals are placed in a limited quantity of air, they *gradually* become stupified by the accumulation of carbonic acid, and do not manifest the uneasiness which would be produced by the *sudden* diminution of the respiratory function in corresponding proportion. Hence, when animals whose par vagum has been divided are similarly circumstanced, it can excite no surprise that they should not manifest symptoms of greater disturbance. For the present we are compelled to incline to the last of the three hypotheses above mentioned; but we would suggest whether the cutaneous nerves in general may not in such cases, as in that of the first inspiration, conduct the stimulus to the respiratory movements. Dr. Hall has justly dwelt upon their importance to this action, and has adduced an instance in support of his view, which we shall quote as being of an interesting though not unique character.*

"My friend, Dr. Heming, witnessed an interesting fact in proof of this opinion. The infant, just born and covered by the bedclothes, did not breathe. Dr. Heming, after waiting a few seconds, proposed to himself to adopt some measure for this asphyxia, and lifted up the bedclothes. The contact of the cool atmosphere instantly excited an act of inspiration. This view of the subject is confirmed by some facts in pathology to be detailed shortly, and by some experiments." (*Memoirs*, p. 88.)

That all the actions hitherto described are dependent on the spinal cord and its nerves alone, has been abundantly proved by vivisectioners, as well as by observation of the movements of anencephalous fœtuses. For some very interesting and judicious remarks on the latter subject, with which we fully coincide, and the length of which is the only obstacle to our extracting them, we refer to Mr. Grainger's Observations, p. 76, &c. Those of our readers, however, who have been accustomed to regard this part of the nervous system as endowed with sensibility, may not yet feel altogether satisfied that sensation is not a necessary link in the chain of excited actions. We think that the candid examination of the phenomena included in the next division, in which the organs usually regarded as voluntary will be shewn to be capable of similar excitement, should convince them that the indications usually regarded as establishing the existence of this property by no means warrant the inferences which have been drawn from them.

The movements we are now to consider are those of which the tendency is, either directly or indirectly, the protection and conservation of the body from external injuries; and for these the voluntary muscles are usually employed, although, in one of the most remarkable instances to which we shall allude—the action of the pupil—the will has not, except in a few extraordinary cases, any controlling power. That the aperture of the pupil is proportional, in a healthy eye, to the degree of light which impinges upon the retina, is a fact known from an early period. That it depends upon the sensibility of the retina has generally been believed, notwithstanding the cases, of no unfrequent occurrence, in which perfect amaurosis is accompanied by an active pupil. Mr. Mayo's experiments appeared to prove that the motion is under the influence of the third pair

* See a curious instance among the Selections from the Foreign Journals, in the present Number, p. 582.

of nerves, excited by a sensation communicated by the optic; and yet cases also occur (of which we have ourselves seen many,) in which complete paralysis of this nerve has been accompanied by an active pupil. The doctrine of excited actions seemed to us fully capable of reconciling these anomalies, and of serving, by its discriminative application, as a means of diagnosis; and we were accordingly well pleased to find that Mr. Grainger had taken a precisely similar view, and expressed it in his usual clear and simple manner.

"The optic nerve, like the common sentient nerves of the skin, contains two orders of fibres,—the true sensiferous, which are connected with the cerebral convolutions by the diverging fibres described by Spurzheim, and the incident fibres attached either to the grey matter of the optic tubercles or of the optic thalami. It is not possible to speak with certainty on this latter point, because the actual distinction in the use of the tubercles and thalami has never been determined by experiment; but, as Flourens remarks that the loss of the former does not destroy the motions of the iris, the incident fibres are probably connected with the latter. Now, those impressions made on the retina which produce vision are, it is certain, transmitted by the true sensiferous fibres to the cerebral convolutions; for, as Flourens has proved, the loss of the hemisphere, the other parts remaining intact, destroys in the animal the power of seeing; whilst it is as certain, from the experiments of Mr. Mayo, that the impressions which excite the actions of the iris are transmitted by those fibres which go either to the optic tubercles or to the thalami." (P. 72.)

"The existence of two orders of conductors existing in the optic nerve, and transmitting impressions either to the brain or to the optic tubercles or thalami, offers also a clue to those apparent anomalies in the state of the pupil which are so often observed in injuries of the brain producing coma, in apoplexy, and in amaurosis. In these affections, in all of which there is total blindness, the pupil is sometimes immoveable, whilst at other times it readily acts under the stimulus of light; and all this occurs as if there were no fixed laws regulating the actions of the iris. But, if a series of observations were instituted to determine these and other phenomena connected with the reflex action of the spinal cord, (and such an enquiry would be of great interest,) it would doubtless be determined that when, in compression of the brain, the iris retains its power, although there is a loss of vision, the cause of compression is confined to the percipient organs, the cerebral hemispheres; and that, when there is both a state of blindness and a fixed pupil, the mischief implicates the optic tubercles and thalami, as well as the hemisphere. The same remarks apply to cases of amaurosis depending on disease of the brain. Of course, in all of these cases, if the trunk of the nerve or retina is injured or diseased, the pupil will be fixed."* (P. 74.)

Here, then, we have another unequivocal instance of the production of reflected action through the spinal cord, without the necessary intervention of sensation.

One of the simplest instances of preservative tendency in movements of this class is afforded by the closure of the eyelid consequent upon the contact of a foreign substance with the tarsi, which Dr. Hall has shewn to occur when the functions of the brain are destroyed. In this and other instances sensation certainly follows the impression when the nervous system is entire; but just as we have shewn that the maintenance of the functions of organic life is remotely dependent upon actions, to the

* In a note Mr. Grainger has, however, complicated his explanation with what we consider the unphilosophical, because unnecessary, hypothesis, that the action of the pupil, through the excito-motor system, is stimulated by the *calorific* rays of the luminous object. We see no reason why light should not produce the requisite impression as well as heat.

excitement of which sensation is a necessary stimulus, so it would appear that, in these cases, its occurrence has a purpose no less obvious. For, as Mr. Grainger very justly remarks, "the accompanying pain is the best precaution that could be devised to prevent injurious effects, as it causes the animal to avoid in future all such sources of mischief." (P. 102.)

Our readers are probably already aware of the fact, first brought prominently forwards by the experiments of Flourens, that many of the higher Vertebrata will, after the removal of the cerebrum, execute most of their natural movements, when these are excited by their appropriate stimuli; and that the power of combining these movements appears to be lost when the cerebellum is also removed. The rabbit made the subject of this experiment by Magendie "cried when a hair of its whisker was plucked, or a strong acid applied to the nose, and endeavoured to remove with its paws any source of irritation, as it would do if it had not been mutilated." These and similar phenomena have been regarded as indicating the persistence of sensibility in the spinal cord. This conclusion has been greatly invalidated, however, by other experiments, which lead to an inference of an opposite character; but, although these experiments are very interesting, and the inferences drawn from them highly probable, we cannot regard them as yielding that satisfactory demonstration which pathological phenomena in the human subject afford. Some of the most striking of these experiments we shall quote from Mr. Grainger, by whom they appear to have been more discerningly executed than the similar ones detailed by Dr. M. Hall.

"The body of a salamander was cut into two pieces, so that the pelvis, with the hind legs and tail attached to it, was entirely separated, and the phenomena of the reflex power were carefully noticed. Some convulsive motions of the legs and tail, as in all other instances, took place. Upon pricking the feet, the limbs were freely moved, and also the tail; and, from the application of one stimulus, the motions were several times repeated, in consequence of the limbs at each movement coming in contact with the table. The effect of applying great heat,—a piece of lighted paper, for instance,—was yet more remarkable; for, upon touching one of the legs or tail, the pelvis and tail were forcibly moved: those parts seemed, in fact, to be writhing under the excess of suffering. In this case, as in every other in which the reflex action is excited, it was found that the extent of motion depended on the intensity of the stimulus; a combination of circumstances than which it is scarcely possible to conceive any more decisive proof that feeling still remained; in fact, without some powerful evidence to the contrary, no other conclusion could be formed.

"In another salamander the spinal cord was simply divided without any further injury being inflicted. Upon pricking the foot and tail, the same motions were produced as in the former experiment; and upon applying a lighted paper violent movements were excited in the tail and legs. All this seemed to indicate the persistence of sensation and volition, and yet it was found that the limbs which could be so violently excited by mechanical irritation and by heat remained motionless under the impulse of the will; so that when the creature made an effort to walk, which it frequently attempted, the hind legs and tail were, as in the case of the rabbit, kitten, &c. dragged along completely paralysed. Now, as the very idea of volition implies a perfect control over any muscle which an animal may wish to stimulate, this utter want of such power is the most decisive proof which could be adduced to shew that in this and similar cases the empire of the will was destroyed by the section of the spinal cord. But it may be thought that, although under these circumstances there was no longer any volition, sensation might still remain; a supposition, however, inconsistent with the fact that, in this same salamander, I found, by proceeding cautiously, so that the animal could not see the approach of the hand, that an entire hind leg could be cut

off with a pair of scissors, without the creature moving or giving any other expression of suffering.* (P. 57.)

In a similar experiment on a frog, it was remarked that the application of heat to one of the extremities, either anterior or posterior, excited a simultaneous movement in both; an effect quite different from that produced by similar treatment in a perfect animal, in which the injured limb only is retracted. The following experiment upon the green frog exhibits a marked correspondence with that of Magendie just alluded to.

“Upon irritating the cloaca in one of these animals which had been decapitated, the most violent motions were excited in the hind legs, and *repeated attempts were made by these limbs to remove the instrument* with which the cloaca was touched. This fact I have since repeatedly seen in the green and common frog, both when the head was removed and when the spinal cord was divided in the back: the same thing may readily be noticed in the common fly and other insects after decapitation. I have also observed that, if after having cut off the head in frogs, fire is applied to the fore part of the trunk, violent motions to remove the source of excitement are made.” (P. 59.)

“In these experiments it was found that when the surface of the body was touched, as in the case of decapitated serpents, the headless trunk of the salamander was constantly turned in the direction of the irritation, and that when both sides of the trunk were simultaneously pinched, the body made violent movements, which, if the head remained attached, would be called struggles. It is likewise particularly worthy of observation, that in frogs there is a marked difference in the degree of excitability in the anterior and posterior limbs after decapitation; the latter, which are almost exclusively employed in swimming, displaying when pricked more forcible movements than the former.” (P. 60.)

It must be recollected that the doctrine of the existence of sensibility in the spinal cord is founded only on vague inference from such experiments as those of Flourens, who was himself led by them to an opposite conclusion; and we think that the inference of its non-existence, founded upon the experiments we have just quoted, is at least as legitimate. We are, however, furnished with positive evidence that motions of this class may be excited without consciousness on the part of the individual, by the experiments which nature performs on the human subject; the effects of disease or injury replacing in these instances the scalpel of the vivisector.

We formerly alluded† to a case of paraplegia in which irritation of the foot produced its retraction, without consciousness or will on the part of the patient. This was at the time a solitary instance; but another case of a still more striking character was soon afterwards published;‡ and the prediction was hazarded that others would be speedily brought to light, when public attention should be called to the question at issue.

* We have heard it more than once enquired why the excessive stimulus of the division of the nerve did not produce the usual sympathetic movements in the case here related. It seems to have been forgotten, however, that, as the leg was cut off, the most definite and constant of these movements could not be shewn. But it may also be remarked, that, as there is a great deal of difference in the tendency of particular parts of the surface to receive and transmit the impressions which give rise to associated actions, (the most *sensible* parts not always being the most *impressible*,) so it appears that impressions made upon the peripheral extremities of the afferent nerves have more tendency to excite them than those made upon the divided extremity of a trunk.

† Vol. III., p. 38.

‡ Edinb. Med. and Surg. Journ., xlviii. p. 33.

This has been verified by the subsequent publication of four cases of a similar nature, of which we shall give the details. (See also p. 519.)

"I am indebted to a most intelligent pupil, Mr. W. F. Barlow, of Writtle, Essex, for the following interesting case:—John Bright, aged nineteen, climbed up a walnut-tree, on the 1st of October, 1836, for the purpose of picking the fruit; and, when he had attained a very considerable height, slipped, and was precipitated to the ground. He was soon afterwards found in a cold and pulseless condition, with his lower extremities numb and motionless. There were obstinate constipation, which was overcome by strong purgatives, and retention of urine, which required the introduction of the catheter. The following was the condition of the patient three months after the accident: The lower half of his body and inferior extremities were entirely devoid of sensation, and they were not in the slightest degree under the influence of the will. Sometimes the patient had cold shiverings; and, whilst the muscles of that part of the body supplied with nervous energy from above the seat of injury, were observed to shake, those deriving their nerves from below that spot were perfectly motionless. Notwithstanding the anæsthesia and the patient's inability to effect a single movement through the medium of volition, when the integuments of the legs were pinched, or more particularly when the sole of the foot was tickled, the extremities were retracted with considerable force. A little cold water dashed upon the surface produced the same effect, though there was no feeling of coldness. One leg was constantly in the flexed position, and, if straightened, immediately recovered it again. When the catheter was introduced, the penis was excited into a state of complete erection; an effect consequent upon the gliding of the instrument along the urethra; at the same time the legs were drawn up and a twitching of their muscles was very obvious.' The spinal marrow was found, post-mortem, to be nearly severed in the neck.

"I am indebted to Dr. Budd for a case of paraplegia, in which the most extraordinary and forcible movements of the limbs took place whenever the bowels were relieved. In a case recently detailed by Sir B. C. Brodie, bart., to the Royal Medical and Chirurgical Society, effects similar to those described by Mr. Barlow took place on passing the catheter; the patient being totally unconscious of the contact of the instrument and of its effects." (*Hall's Memoirs*, pp. 63-4.)

Why such phenomena are not oftener witnessed in old cases of paralysis, the following experiments of Mr. Grainger's afford a satisfactory explanation.

"A portion of the sciatic nerves was removed in a dog, and also in two rabbits: it was found in the dog that the power of exciting the muscles of the leg to contract, by pinching the sciatic nerve, was entirely lost at the expiration of eleven weeks, and, in the rabbits, the same result was observed at the end of five weeks. Thus, it is perceived that in a short time, if the muscles are entirely inactive, as they are in paralytic persons in whom no attempt is made to excite them, they lose all power of being stimulated through the medium of the nerves." (*Grainger*, pp. 96-7.)

By these data, therefore, we regard that as now demonstratively proved of which we formerly spoke as an admissible supposition, "that the changes in the spinal cord, not the sensations which in the natural state accompany them, are the causes of the sympathetic or excito-motory actions."*

III. Although from their greater similarity to voluntary movements, and from their dependence upon sensation for which the brain is required,

* British and Foreign Med. Review; Vol. III., p. 38.—The question has been frequently put to those who uphold the doctrine we are now advocating, *why* sensation should be so constantly associated with these changes, if not essential to produce motion. We might fairly object to any reasoning from final causes in a question of facts; but the objection is very easily answered. In many instances the production of sensations is the stimulus necessary for the excitement of other actions required for the continued maintenance of those in question. (*Edinb. Med. and Surg. Journal*, vol. xlviii.)

(experimental investigation being thus forbidden,) there is greater difficulty in limiting the actions which we have placed in our third class, yet we think that there would be no obstacle to our strictly defining them, if we had those means of analyzing the exciting causes of the motions performed by the lower animals which we possess in the case of man. A characteristic feature in this, as in the other classes of instinctive actions, is the uniformity of their occurrence, especially when contrasted with the variability of those which depend upon intellectual processes. Thus, every species of animals has its own peculiar food, which is selected without hesitation, when its presence is recognized by the excitement of a sensation; various muscular movements, sometimes of a very complex nature, are required for the purpose; and these are performed with a constancy which forbids us to suppose that they are the result of any acts of judgment or will. The movements excited by the sexual instinct are of the same kind; and not dissimilar do we regard those executed for the construction of a habitation, protection of the young, change of climate, &c. Now, it has been said that in all these cases an instinctive propensity excites a desire which occasions the will to act; but we would ask, upon what evidence? It is inferred from the analogy of human actions; but this is not a safe ground of argument, unless the sources of those actions be carefully analyzed; and we think it will appear, from the preceding sketch, that the analogy is really on the side of their involuntary character. In man these propensities are never (except when morbidly excited) the immediate springs of action; they only produce movements by stimulating the intellectual faculties to provide means for their gratification; and in this respect they rank with ordinary emotions. But in man, when reason is undeveloped or dormant, these instinctive propensities manifest themselves in full force; and their action is then similar to that of an emotion predominating over the will, and, as we have shown, independent of it.

We see no reason why the special sensations (which are not concerned in the second class,) should not excite motions as directly as those impressions which appear to act through the spinal system alone; and various facts tend to support such a conjecture. Although, in adult man, the instincts which minister to the supply of the organic functions are entirely superseded by volition, to which they simply act as a stimulus, there are many motions destined to the preservation of the body from danger, which nature has wisely rendered independent of his uncertain and capricious will. The case to which we have formerly alluded* affords us an apt illustration: "the eyelids, although resisting the influence of the will, closed involuntarily on the sudden approach of a body towards the eye, or on the application of a strong light." This motion, therefore, is evidently as direct a result of the visual sensation as is that produced by tickling the edge of the tarsi in a case of anæsthæsia of the mere impres-

p. 38.) In other cases it contributes to enjoyment, (as in suction, ejaculatio seminis, &c.;) affords præmonition of danger, or excitement to supplementary actions, (as in respiration;) gives warning against inconvenience, (as in the excretory functions.) In this last case the sensations are often excited, not to produce the associated actions necessary for the excretion, but actually to make the will set up the antagonizing action of the sphincters. (See p. 510.)

* Vol. IV. p. 500.

sion. The ordinary motions destined to preserve equilibrium are in like manner, as has been well shown by Dr. Symonds,* the result of sensations only, without the influence of volition; although there is no doubt that, in man, they are greatly modified by education.

Amongst the most remarkable operations of this connexion between sensation and motion in the human being, are the actions which result from *imitation* and *habit*. The former is a propensity which is capable, like others, of being morbidly excited and of overcoming the power of volition, so as to lead to the most extravagant actions; but it has a remarkable effect in producing involuntary actions when the mind is directed towards other objects, even without being itself more than naturally developed. In the article referred to, Dr. Symonds's remarks, "Any set of muscles may acquire particular actions and assemblages of actions by passive imitation only; and to such a degree, indeed, that desire is often vainly employed in opposition to this principle. One person yawns, or sighs, or laughs, because another does; a child or susceptible female, if frequently in company with a person who winks, or stammers, or falters in his gait, will fall into similar habits, notwithstanding there may be a variety of inducements for attempting to avoid them."

The influence of habit in reducing to this class actions which were originally of a purely voluntary character is extremely remarkable; and we cannot help wondering that several able metaphysicians should so long have maintained that the will is still concerned in each movement. If it be true, however, as we have already argued, that muscular contractions originally resulting from a volition remotely excited by common sensation, may in time be excited by an impression only, there can surely be no reason for refusing to the special sensations a similar power; and it appears much more philosophical to suppose that a train of habitual actions, once started by the will, should be continued by the stimulus of sensations only, whilst the attention of the mind is stedfastly directed towards other objects, than that the intellect should be in a state of constant vibration between the two processes. It is perfectly true that the will can at once check these actions, but only when directed with that specific purpose; but, if this does not interpose, we are of opinion that the train once started will continue to run on as long as the required stimulus is supplied, and the organs of sensation remain in a direction or condition to receive it. Such actions may be excited by internal as well as external sensations, or by the recall of either by the aid of memory.†

It appears to us not improbable that the locomotive actions of man and the higher animals, considered singly, are of this character. In many of the inferior classes, probably in all below the terrestrial vertebrata, these actions are from the first purely instinctive; and the remarkable perfection and continuance of their performance by insulated segments in the articulated classes would show that they are not in them

* On the Relations between Mind and Muscle. — (West of England Journal, vol. i. p. 169.)

† It will be seen that on this point we adopt the opinions of Hartley, in opposition to those of Dugald Stewart, and others. The *secondarily automatic* actions of the former writer correspond precisely with our *habitual* motions excited by sensation only. For a full statement of the facts and arguments on each side, see Rees's Cyclopædia, Art. *Mental Philosophy*.

dissimilar to the movements of respiration in birds or quadrupeds. In those species, however, of which the young are born in a state incapable of maintaining their own existence, and dependent therefore on their parents, the locomotive apparatus has to undergo an education not only for the adaptation of single movements to particular purposes, but for the combination of them into the complex associated actions of walking, flying, &c. For this combination the existence of the cerebellum appears to be necessary; and it is then impossible to say what degree of sensation may remain. The following experiments, however, show that the part of the foot which habitually touches the ground is peculiarly susceptible of impressions by which propulsive motions are excited.

"In a rabbit about three parts grown, the cord was laid bare in the middle of the back, and a portion of it cut out. The lower part of the body and the hind legs were immediately paralysed, the limbs were laid motionless on the table, and the animal had no power of moving them; a fact which became very striking when the rabbit made an attempt to walk; for then, notwithstanding the fore legs freely acted, and thus dragged along the body, the hind legs were motionless; volition, although in full operation, had no effect upon them. I then, with a fine needle, pricked the skin covering the under part of the heel; when instantly *the toes became extended, the heel was raised and both legs were forcibly thrown backwards*. This experiment was repeated on several other rabbits, in kittens, and in puppies, and constantly with the same result; the combined and successive motions of the toes, the foot, and the leg, were seen; but, in the kitten, the irritation to produce the effect required to be made not on the heel, as in the rabbit, but nearer the toes. In most of these instances it was also found that only the leg belonging to the foot which was irritated was thrown backwards.

"In a similar experiment, performed by Mr. Mayo, it is stated that, when the foot was irritated, the movement of the limb was exactly similar to that which the animal would make if in undisputed possession of its sensation. In order to ascertain the correctness of this statement, I pricked the hind foot in a rabbit, the cord of which was entire, when the animal moved the limb to avoid the irritation; but the motion was totally different from that which occurred in the preceding experiment. Upon dividing the cord, and pricking the under part of the foot, a most violent motion was excited, and both legs were thrown back. Those gentlemen who were present were particularly struck with the difference of the movements in this rabbit before and after the division of the spinal cord. I found, in this and other similar experiments, that when the thin skin of the leg was touched after the section of the cord, a very slight contraction was produced, and that it was only when the under part of the foot, which in progression strikes on the ground was irritated, that the remarkable and combined action of both legs was produced." (*Grainger*, p. 54.)

A very interesting confirmation of this view is afforded by the following remarkable case; and we ourselves have seen a similar one.

"A girl, about fifteen years of age, who was a patient of Mr. Crosse, at the Norfolk and Norwich Hospital, a few years since, was affected with angular curvature of the spine, producing insensibility and paralysis of the lower extremities. On tickling *the soles of her feet*, which as an experiment was often done, the legs were immediately slightly retracted, although the patient said she felt nothing; it was further remarked that, on touching the *other parts of the feet or the legs* in the same manner, no effect was produced." (*Grainger*, p. 94.)

A case has been mentioned by Mr. Travers, in which the os frontis was driven in by a fall, and a considerable portion of the brain extruded at the wound of the scalp; yet the boy, "although utterly deprived of consciousness, made obvious but unavailing efforts to aid the surgeon in getting him into bed, and thrust his arm mechanically into the sleeve of

a clean shirt, after his hand had been placed in the opening of the sleeve."

The following observations of Mr. Grainger's, with which we must take our leave of this part of the subject, we regard as worthy of attention, although we are not yet prepared to coincide with them.

"When, in walking, the foot strikes the ground, if we pay any attention at all to what takes place, we are only conscious of the sensation which is excited; the motions of the limb are overlooked, or it is concluded that they are purely voluntary: and so it is in the case of deglutition; a sensation is produced by the contact of the food, and, until lately, it was thought that the motions consequent upon that contact were voluntary. It is then, I think, most probable that, in progression, an impression is made on the incident nerves of the foot, by which the reflex power of the spinal cord is excited to action, and the combined and required motions are produced. This explanation does not in any degree exclude the idea of due and proper control being exerted over the limbs in walking, flying, swimming, &c., inasmuch as there are proper nerves, the cerebral, going to every voluntary muscle, by which the animal can at will direct, quicken, or give increased vigour to the motions; it is even probable that a special apparatus is provided for the purpose of enabling the animal altogether to prevent or stop the excited actions. It is also evident that these excited actions of the legs must be much more perfect and influential in the lower than in the higher animals, exactly in proportion to the relative development of the spinal cord and brain; so that, whilst in some of the lowest tribes progression is almost entirely a spinal action, in the mammalia, and above all in man, the central influence is greatly exerted. The muscles called voluntary are then probably all susceptible, under different circumstances and in varying degrees, of being stimulated by the brain, through the medium of volition, and by the spinal cord through the means of the reflex power." (*Grainger*, p. 150.)

We have now analyzed the characters of purely instinctive actions, and traced them to the parts of the nervous system on which we regard them as respectively dependent. We are well aware that the extended application of the term *instinct* which we have employed may not suit the ideas of many of our readers: it is, however, one which has been used by several of those who have most successfully investigated the nature of the actions thus included in it, and who think, as we do, that no distinct line of demarcation can be drawn between any of those actions of living beings which are immediately prompted by external stimuli, and in which no purely mental acts are involved.*

Of the functions of the great sympathetic nerve, which Mr. Grainger regards as a collection of systems of ganglia with their respective incident and reflex fibres, by the agency of which are produced the contraction of all the organs which are in a more especial manner supplied by it, we have little to say in addition to the observations we have made in this and in former articles on the influence of the nervous system on the functions of organic life. "If," says Mr. G., "it be admitted that any nervous power at all is required to effect those processes which take place in the fœtus—circulation, secretion, and nutrition,—(and it is almost impossible to arrive at any other conclusion,) it is certain that it is the agency of the great sympathetic, and not that of the cerebro-spinal system which is necessary." Now, we still maintain that it is incumbent on those who uphold the necessity of this nervous power to prove it definitively, since all analogy leads to an opposite conclusion. We regard the capability of

* See Kirby, *Bridgwater Treatise*; ch. xviii. Virey, *Histoire de l'Instinct des Animaux*; Dubois sur l'Instinct, *Mem. de l'Acad. Royale de Médecine*, tom. ii.

separating from the blood a peculiar secretion as a peculiar property inherent in the glandular membrane, as contractility is the inherent property of muscular fibre: and just as the peculiar arrangement of the excitable and contractile tissues in animals requires a nervous system to act as a conductor between them, and to blend their actions, independently of any production of sensation or influence of the mind, so do the complicated and highly specialized organic functions of animals require to be harmonized and kept in sympathy with each other by some mode of communication more direct and certain than that afforded by the circulating system. These functions also require to be placed in relation with the purely animal system, and especially to receive an influence from certain emotional conditions of the mind. This is unquestionably effected by the communications of the sympathetic or visceral with the symmetrical system, which increase in number as we ascend the animal scale.

We think that we have thus found quite enough to be accomplished by this nervous apparatus, which has been so unfortunate in the number and variety of offices with which it has been charged; and we hold that these functions are all that rigorous inferences from observation and experiment can lead us to attribute to it. If we regard the hypothesis of the dependence of the organic functions on nervous influence communicated from the great sympathetic, as wanting in the characters of a legitimate theory, we should express ourselves in still stronger language in reference to Dr. Hall's strange notion of the uses of the ganglia upon the sensory roots of the spinal nerves, which is unsupported by any but the vaguest analogies and is controverted by numerous facts.*

In order to account for the connexion between certain impressions upon the sensory organs and their resultant motions, many physiologists have endeavoured to establish a peculiar anatomical relation between the roots of those nerves which convey the impressions to the central sensorium and those which transmit the stimulus to the muscles. Our readers must be well aware that to establish this relation in the case of the respiratory nerves was one of Sir C. Bell's favorite objects; and that the principle which he thus assumed led him to infer the existence of a particular tract in the spinal cord, from which these nerves, or at least that portion of them subservient to the function, derived their origin. This doctrine was strenuously resisted by those physiologists who were not so far dazzled by the brilliancy of Sir C. Bell's other discoveries as to admit readily whatever he should advance; and it received what we cannot but regard as a very satisfactory refutation from the pen of Dr. Alison, to whose paper on Sympathy we shall presently have occasion to refer more particularly. In fact, as it has been more than once urged, "Sir C. Bell's attention appears to have been so entirely absorbed by those connected with the respiratory function, that he seems to have forgotten that there are other extensive associated and sympathetic movements of the muscles of the body, besides those which he has so beautifully illustrated; for it is obvious that, if a particular tract of the spinal cord is necessary to carry on

* Of these, one will at present suffice. In fishes, no ganglia exist upon the posterior roots of the spinal nerves; yet nutrition of the tissues is performed without them. As the scaly covering of fishes must greatly interfere with general sensibility, we do not consider that the supposition of the connexion of the ganglia with sensation is invalidated by this fact.

the respiratory movements, there ought also to have been a defecatory tract, a urinary tract, and so on, to carry on the other sympathetic movements in which a number of distant muscles are engaged in simultaneous action. If the other associated movements can go on without it being necessary that the nerves supplying the muscles concerned in them should come from particular tracts of the spinal cord, then surely there can be no necessity for this in the case of the respiratory nerves.* We are far, however, from wishing to argue against the principle of referring to structural connexion, wherever we can do so, the connected functions of different nerves. A very large proportion of the associated movements excited by impressions conveyed to the spinal cord are stimulated through the motor nerves arising from the same segment; and this holds good not only with regard to those usually regarded as spinal nerves, but to those which take their origin within the cranium. The supposition that there is an organic or structural connexion of some kind between nerves of apparently distant origin whose functions are associated together, appears to us therefore by no means absurd or even deficient in probability; and knowing as we do how many different functions are performed by the tubuli of any one nervous trunk, and how difficult it is to trace these far into the central organs, we are not surprised that it should have been hitherto difficult or even impossible to trace such connexion by anatomical investigation.†

* Dr. J. Reid, in *Edinburgh Med. and Surg. Journal*, vol. xlix. p. 173.

† We are well aware that the view here expressed is opposed to that which was maintained by Dr. Alison, in his paper on Sympathetic Actions, already referred to, and which this distinguished physiologist still holds. He regards Whytt as having demonstrated that these movements are the result of the excitement of particular *sensations* by which they are stimulated; and endeavours to prove the fallacy of any attempt to explain them by connexion of nervous structure. Now, Whytt's object was to show that all sympathetic actions (in which he includes the motions of the heart and alimentary canal) are produced, not by *direct* nervous or other structural connexions, but by the excitement of the "sentient principle," which is intermediate between the application of the stimulus and the reflexion of it, and for which it was obviously necessary that there should be communication with the *central* organs of the nervous system. How far Whytt implied the existence of *sensation* by the use of this term, we shall presently enquire, (see p. 524;) but it seems to us that the action of stimuli through the *central organs*, rather than by any more *direct* channel of communication, is the main point of his argument. Now, there would seem no reason why particular motions should not be associated with particular *impressions* as well as with particular *sensations*; since it is obvious that, to produce a similar sensation, a similar impression must be made; and a difference of sensation necessarily involves the idea of a difference in the impression. Thus, impression *a* produces

sensation A, with which is associated
motion α ;

and, in like manner,

impression *b* produces
sensation B, with which is associated
motion β .

We cannot see, therefore, why the production of motions may not be regarded as connected with the *impression* as well as with the *sensation*. That sensation always follows the impression in the normal condition of the system, we well know, and can readily account for; but this does not prove that the motion is associated with it; and, in the instance of the action of the pupil, we have abundant evidence, supplied by pathology, that the sensation may be *entirely* abolished, and the sympathetic or associated action yet be excited. It is obvious, therefore, that such views are in no degree opposed to the doctrine of continuity of structure between the afferent and efferent nerves, established through the central organs of the nervous system.

The views which we have already stated regarding the nature of the changes performed by the spinal cord, must have rendered it evident that we regard sensibility, and the capability of exciting voluntary motive action, as confined to the brain. It must be recollected that the sensibility thus attributed to the brain is a very different thing from the (so-called) sensibility of the external organs of the body. When we say that sensibility is located in the brain, we mean that external impressions must be communicated to that organ before the mind can become conscious of them; whilst if we say (as is inaccurately, though frequently done,) that the skin or other tissue is sensible, we mean that it is capable of being impressed by an external agent in such a manner that a sensation is excited in the mind, which is referred by it to the spot where the original impression was, or seemed to be, made.* We can thus readily explain the fact, proved by the experiments of Flourens, Fodera, and others, and by numerous pathological observations, that injury of the substance of the brain itself does not give rise to painful sensations; for it is only in obedience to impressions conveyed through a particular channel that such sensations are excited; and there is no more reason why lacerations or wounds of the substance of the brain should produce pain than that the exposure of the roots of the optic nerve to luminous objects should give rise to visual sensations. A very similar explanation will account for the non-production of motion by stimulants applied to the brain itself, and not directly affecting the medulla oblongata. All motions are excited by changes in the spinal cord, from which the motor nerves really arise: the ordinary office of the brain is to produce some of those changes. We can imitate, by chemical or mechanical stimuli applied to the cord, the effects of the stimulus which it ordinarily receives from the brain or from external impressions; but we cannot imitate those changes in the brain itself which give rise to those of the spinal cord.

We must now bring our review to a close by retracing, as briefly as is consistent with clearness and equity, the successive advances towards what we regard the correct opinions on the points now under discussion. The first reference with which we are acquainted to the doctrine of the transmission and reflexion of an impression through the nervous system appears in Glisson's remarkable work, "*Tractatus de Ventriculo*," (p. 172,) published in 1677; and, though the motions thus occasioned are not clearly expressed, and the central organs of the nervous system, not referred to in their production, it is evident that this discerning writer separated them, on the one hand, from the effects of simple irritability of muscular fibre, (which he was the first to regard as a property of that tissue independent of sensibility,) and from voluntary motion on the other.

We find, however, in the "*Essay on the Vital and other involuntary Motions of Animals*," published by Whytt, in 1751, a much fuller, and in most respects more correct account of these phenomena; and, as the opinions of this sagacious author are too little known to the present generation, and, owing to the misinterpretation of his peculiar phrase-

* This distinction will at once be made apparent by adverting to the *sensations* referred to the extremities of a nerve, when it is on some part of the trunk that the *impression* is really made.

ology, have been little comprehended, we shall detain our readers for a brief space whilst we set them in what we regard as their true light. It is well known that Whytt strongly opposed the doctrine of Glisson and Haller regarding the independent contractility of muscular fibre; and the object of his essay is to show that the vital motions,—that is to say, the muscular contractions immediately concerned in maintaining the organic functions,—are all dependent upon a *stimulus* conveyed from the periphery to the central organs of the nervous system, by which a motive influence is there originated. This motive influence he regarded as due to the *sentient principle*, which he believed to be a part of the soul, and to be diffused through every part of the body. Of the nature of this sentient principle, as elsewhere limited by himself, we shall presently speak. Dr. Whytt describes three kinds of muscular contraction, *natural*, *voluntary*, and *involuntary*. The *natural* contraction he explains to be the “constant contraction of the sphincters, and the tension of such muscles as are balanced by antagonists,” (pp. 12, 13;)* and he points out the dependence of this power upon the nervous system, and at the same time its partial subordination to the will. It is evident that this “natural contraction” precisely corresponds with the “tone” of Dr. M. Hall; and, whilst we willingly yield to the latter physiologist the credit of having limited this operation of nervous power to the spinal cord, we cannot admit his originality in specifying its character.

Of the *evident* contractions he says, (p. 15,) “As often as the influence of the nerves is determined into the muscles, so as to operate more powerfully on them, they are excited into stronger contractions, which are not natural, and therefore may be called violent. This extraordinary determination of the nervous influence may be owing either to the power of the will or to a stimulus.” The former are, of course, *voluntary* actions; and the *involuntary* motions he at first describes as arising from a stimulus applied to the muscles, which immediately excites them to contract themselves. This contraction he attributes to nervous energy; and observes that “the effects of different stimuli depend very much upon the peculiar constitution of the nerves and fibres to which they are applied.” (P. 18.) He subsequently, however, extends the application of this principle, by showing that the stimulus is not always applied to the muscle itself, as in the case of the motions of the iris and those of respiration; and he argues that the sympathy in these cases cannot be explained by the connexion of the nerves themselves, but only by the common terminations in the brain. He maintains that the contractions of the heart and alimentary canal are altogether involuntary, because the stimulus is applied to the muscles themselves; and points out that, where the stimulus is conveyed from a distant part, as in the actions of respiration, defecation, &c., the will has the power of controlling the movements in the inverse proportion of the strength of the stimulus. (pp. 200-1.) The motions of the iris are mentioned as an exception to this principle, because “the action of light upon the very sensible retina affects the mind so strongly that we cannot, by any power of the will, prevent the contraction of the pupil.” (P. 202.) After discussing the motions connected with the organic functions, Dr. W. passes on to those

* Whytt on the Vital and other involuntary Motions. Edinburgh, 1751.

designed for the preservation of the body; and instances the retraction of the leg owing to the fall of a spark or drop of boiling water on the foot, and the contraction of the muscles of the trunk occasioned by tickling the sides, as proofs that the stimulus does not produce motion by "sympathy of nerves and continuity of membranes," but is conveyed to the mind, which, by an involuntary action, endeavours to get rid of a source of disagreeable irritation. He argues also, from these facts, that in no case are the muscular fibres called into action by direct stimulation, as Haller maintained. The last chapter of his work is devoted to the consideration of the motions of animals after death, which he distinctly attributes to the same *sentient principle*, or soul, which produced them during life.

It will be seen, therefore, that Whytt had a very distinct and comprehensive notion of the character of the involuntary muscular actions included in the three classes we have formerly specified; and that, though his erroneous ideas of the contractility of the muscular fibre prevented him from attributing those composing our first division to their right sources, yet that he clearly distinguishes them from such as are excited by a stimulus applied to a distant part. But it will be said by those who are disposed to depreciate the value of his writings, that Whytt absurdly attributed all these motions alike to the operation of a "sentient principle," which he identifies with the soul. This would, however, be giving a very unfair, because incomplete, representation of his opinions; since no one can attentively read the essay referred to without perceiving that the writer uses the term in a far different sense from that which we should now attribute to it.* He clearly separates the *sentient* from the *rational* principle of the soul, and shows that, as far as the latter is concerned, the individual acts voluntarily, or is a free agent. Dr. Whytt's ideas of the universal necessity of nervous agency for muscular contraction, required that this sentient principle should be regarded as existing not only in animals recently dead, but in the separated parts of those which remain alive; and for his ingenious mode of explaining the simultaneous divisibility and unity of the soul, we must refer to his Essay, (pp. 382-3.) By Dr. Alison, and other modern physiologists, Whytt has been regarded as using the term "sentient principle" synonymously with what we now call "sensation." But we think that a candid examination of his essay will show that the term "excitability" more nearly expresses his meaning; for he very plainly shows that the operation of the *sentient principle* does not always imply the mental change which we now understand by *sensation*. The gentle and uniform stimuli to the vital actions he regards as operating without *consciousness* either of the presence of the stimulus or of any mental act excited by it, (pp. 292-303;) and it is evident therefore that his ideas, put into the phraseology of the present day, would be simply expressed by saying that such ordinary and gentle stimuli act by producing *impressions* only, whilst stronger ones, by which unusual motions are excited, produce *sensations* also. How nearly Dr.

* In illustration of this phraseology, we may refer to a curious contemporary treatise on Animal and Vegetable Propagation, published in 1752. The author, Dr. Parsons (like Whytt, a Stahlian,) speaks of the "animating principle" of plants, and of the "animating and moving principle" of animals, as forms of *the soul*.

Whytt thus approached to the doctrines we have been maintaining, we need scarcely stop to point out.

That Whytt was not ignorant of the *independent* functions of the spinal cord, is evident from the following passage: "Wheresoever, in the present argument, I have spoken of nerves being derived from the brain or cerebellum, I desire to be understood as including the spinal marrow, so far as it can be reckoned a continuation of the medullary substance of these parts, or to agree with them in its structure and use; for the spinal marrow does not seem altogether derived from the brain and cerebellum, but probably prepares a fluid itself; whence it is enabled to keep up the vital and other motions for several months in a tortoise, after the head is cut off." (P. 341.) If we couple this statement with Whytt's general proposition, that all the vital and involuntary motions proceed from a stimulus, (p. 325,) we can scarcely fail to recognize the doctrine of the "reflex function."

That our statement of Whytt's doctrines has not been warped by the desire of making them appear as conformable as possible to the present state of opinion on these subjects, we think that no one will deny, who candidly examines the Essay we have alluded to, as well as the preface to his work on Nervous Diseases. But we are enabled to produce stronger testimony in their favour from the writings of his eminent successor Dr. Cullen, who adopted, with some modification, his views on the influence of the nervous system, and whose phraseology is sufficiently like that of more modern writers to leave no doubt as to his meaning. Dr. Cullen's opinions with regard to the agency of the nervous system in producing muscular contraction were intermediate between those of Whytt and Haller, as will be seen from the following passage:— "Though the muscular fibres of the heart be endowed with a certain degree of inherent power, or *vis insita*, they are still, for such as is necessary to the motion of the blood, very constantly dependent on a nervous power sent into them from the brain. At least this is evident, that there are certain powers acting primarily, and perhaps only in the brain, which influence and variously modify the action of the heart. I suppose, therefore, a force very constantly during life exerted in the brain, with respect to the moving fibres of the heart, as well as of every part of the body;"—and he then goes on to adduce, in support of this opinion, various facts illustrative of the influence of the different states of the nervous system upon the heart's action, which, in his opinion, prove the *partial* dependence, at least, of the latter upon the supply of the "Energy of the Brain."

It will readily be seen, therefore, that Dr. Cullen admitted some modification of Whytt's views, in accordance with the proofs adduced by Haller of the independent irritability of muscular fibre; but that he still regarded the influence of the brain as concerned in the production of all muscular contractions. The brain, however, he defines as including "the whole medullary substance within the cranium and vertebral canal." (*Physiology*, p. 102.) Its action he speaks of as being excited by the following causes:—1. By volition, when "we will the motions, as means to an end." 2. By the emotions or passions. 3. By the disposition of

* Thomson's Life of Cullen, p. 296.

human nature to imitation. "This imitation is sometimes involuntary; often without consciousness." 4. By appetites or desires directed to certain external objects, and arising from sensation without any reasoning directed to an end. "These motions are evidently instinctive, and strictly connected with the desire that excited them." 5. By certain propensities or desires to remove an uneasy or painful sensation; the motions thus excited not being directed to any external object, but confined to the body itself. "Very often the stimulus to these propensities (as in sneezing, vomiting, &c.) is irresistible; and, unless the peculiar stimulus is present, the motions cannot be produced by volition." 6. "By certain *internal impressions* arising from the exercise of the functions of the body itself, which produce no *sensation*, nor produce motions of which we are conscious, except when exercised in an unusual manner: such are the motions of the heart and arteries, of the *organs of respiration*, of the stomach and intestines, and perhaps of many other parts." 7. "By various occasional *impressions of external bodies*, and by various occasional states of the system or of its particular parts, which excite motions not only in the parts to which the impressions are immediately applied, but also in distant parts, on which they can operate only by the intervention of the brain. Some of these causes operate with, others without, *sensation or volition*." (*Physiology*, pp. 105-111.) Now, though Dr. Cullen erred with Dr. Whytt in placing the heart's action and the peristaltic motion of the intestines on the same footing with the movements of respiration and other associated muscular contractions, this does not diminish the importance of the fact that he recognized in the nervous system a power of response to *impressions* only, without the necessary excitement of sensation. And as Cullen professedly adopted the opinions of Whytt on this subject, we cannot hesitate in accepting his expression of them as confirmatory of the view we have already given of the doctrines of the latter, and to which we had been led by the careful perusal of his own works alone. We cannot but regard the last passage which we have quoted as containing, under its sixth and seventh heads, all the essentials of the doctrine of the "reflex function," broadly and distinctly stated in language which cannot be questioned; and, had Cullen pushed his analysis further, he would probably have been led, by Whytt's experiments, to the conclusion that the spinal cord is the only part of "the brain" specially connected with this property. We have no record, however, of his having attempted to specialize the functions of the different parts of the central organs of the nervous system: this was accomplished by later physiologists.*

When speaking of *Sensations*, Dr. Cullen divides them into Sensations

* It is a little remarkable that Cullen should have adduced, among other illustrations, a topic which has since given rise to much controversy, and on which the latest and most unexceptionable experiments seem to lead back to the opinion which he expressed. "The action of the stomach," according to Dr. C.† "is not so constant as that of the heart and organs of respiration, but it is pretty constant. . . . That the stomach requires the constant energy of the brain is proved by its immediately becoming paralytic when its nerves are destroyed. This effect was commonly found to occur in the experiments made upon the heart by cutting the par vagum: the appetite was destroyed, the contents of the stomach stagnated, and were variously corrupted."

† Thomson's Life, p. 298.

of Impressions derived from without through the sensory organs; and Sensations of Consciousness, "comprehending a variety of feelings in the mind, of which we are conscious, but which we are not able to trace to the impression of bodies external to the nervous system." Under the latter head he includes not only the consciousness of mental operations, but what are now generally called *internal sensations*, arising out of particular states of body, and connected with the organic functions. With this explanation we shall quote the following remarkable passage, which seems to us to comprehend what is essential in the opinions we have expressed on habitually excited actions, and at the same time to correspond in fact, though not in appearance, with the statements of Whytt on the same subject—"Actions which at first produce a sensation of consciousness, as being accompanied with volition, come by repetition to be performed *without any sensation*; or they produce it only when they are performed with uneasiness, pain, or unusual force. When voluntary actions are seldom repeated, the mind is conscious, as often as they occur, of its volition producing them; but if they are very frequently repeated, the mind will at length nearly, or altogether, lose the consciousness of the exercise of its volition; and a motion of this kind will now be excited by external agents, or by any state of the body that formerly occasioned a volition of the mind, without any consciousness of the will being concerned in producing the motion, or at least without any subsequent recollection of the will having been exercised." (*Life*, &c. p. 321.)

The next writer to whom we shall refer is John Hunter, whose Croonian Lecture on Muscular Motion, delivered to the Royal Society in 1776, but just published for the first time, contains many valuable observations relating to our present subject. We do not, of course, refer to these as disparaging the merit of more recent authors, who had no direct means of becoming acquainted with them; but rather as illustrating the gradual progress of opinion on the influence and action of the nervous system: it is right, however, that we should state that many of Hunter's views were expressly included in the writings of his successor in the Lectureship, Sir G. Blane, who, as we learn from good authority, was present at these lectures, and much indebted to them. A considerable part of this Lecture is occupied with a parallel between the irritability of vegetable and animal structures, to which we have already alluded; and Hunter seems by this analogy to have been led to espouse strongly the Hallerian doctrine of the independent contractility of muscular fibre. He therefore avoided the error of Whytt by attributing to this source, and not to nervous agency, the actions of the heart and alimentary canal.*

"The great variety of causes of muscular motion, (says Hunter,) make it almost inexplicable; they may be said to be three—the will, passions of the mind, and external stimuli. Those actions arising from the will and the mind appear to be the most simple, because they are totally unintelligible; but those arising from external stimuli are either voluntary or involuntary, for a muscle that acts by command of the will at one time is also capable of being thrown into action by a particular state of mind or external stimulus." (P. 214.).... "The actions of the body that are both voluntary and involuntary are some of the most beautiful circumstances in the machine.

* Hunter's Works; London, 1837, vol. iv. p. 211.

I believe the muscles of respiration are the only perfect instances of it. Fresh air was necessary for our existence, and it was therefore necessary that it should be regulated by some other principle than that of the will; for it is necessary when we sleep and when we *will* the contrary. Therefore our will has its limits of power over the involuntary actions, and the involuntary also have their limits over the actions of the will; each therefore can go only a certain length in opposition to the other." (P. 217.)*

The following summary of Hunter's opinions on associated actions we derive from the chapter on Sympathy, in his *Lectures on Surgery*. We cannot help thinking that the ideas contained in the last sentence, if fully developed and clothed in modern phraseology, would be found to coincide closely with those which we have expressed in the present article. For our parts, we rejoice to have been anticipated by such a man.

"I attempted to show that the more simple actions arose independently of sensation or of the actions of the nerves; that the nerves, from their specific actions, only become the cause of many actions, but are not the principle of those actions; that, from their termination in the brain, they produce sensations there, from which is formed mind, and that they also give rise to the will, and form the basis of reasoning. I showed that the mind becomes the cause of many involuntary actions in the body, as reason becomes the cause of the voluntary; and that thus the actions of life, of the nerves of the mind, and of the will, arise from impressions being made on each so as to affect their principles." (Vol. i. p. 317.)

It is evident that Hunter had very distinct and accurate ideas of the dependence of the different classes of muscular motions upon the nervous and sensorial powers. He did not attempt, however, to analyze the changes in the nervous system on which these motions are dependent. This was partially done by Sir Gilbert Blane, who took up the subject not long afterwards, with the express intention of extending the results obtained by Hunter; and from his Croonian Lecture on muscular motion, read to the Royal Society in 1788, we shall quote two passages which show how completely he had anticipated various doctrines which have since been propounded as original. After dwelling upon the effects of stimuli applied to the muscular fibre itself, in producing contractions independent of nervous agency, as in the heart and alimentary canal, he proceeds to consider the actions excited by external stimuli, which act remotely, "as in the various instances of sympathy, and in the case of those instincts which nature has implanted for the purpose of self-preservation in brutes, and in the early part of human life." Of the latter he says,† "There is a connexion established between the impression of certain external bodies and the action of certain muscles, analogous to what has already been noticed with regard to the internal motions excited in vessels by the peculiar stimulus of their fluids, nature having instituted certain habitudes between outward stimuli and the moving powers, whereby natural propensities are established equally necessary to the support of life as the internal functions. Thus, in a new born animal, the first contact of the external air excites the act of respiration, and the contact of the nipple excites the act of sucking; both of which actions are absolutely necessary to the maintenance of life, and require the nice cooperation of a great number of muscles,

* Hunter's Works; London, 1837, vol. iv. p. 211.

† Select Dissertations, p. 261.

prior to all experience. Actions of this kind are called instinctive, and differ from voluntary motions in this respect, that the latter are the result of memory and experience, whereas the former are the immediate effect of external impressions, in consequence of an established law of nature, and *independent of consciousness*." He afterwards remarks that the respiratory movements are the only actions of this class that are constantly performed; and that these, whilst unattended with consciousness in their ordinary exercise, are occasionally under the control of the will. We need scarcely point out the complete correspondence of these views with the opinions which we have already expressed on the subject of instinctive actions.

Sir G. Blane then goes on to investigate how far these movements are dependent on the brain and nerves in animals possessed of these organs :

"There are facts which show that instinctive actions, even in animals endowed with brain and nerves, *do not depend on sensation*. I took a live kitten, a few days old, and divided the spinal marrow, by cutting it across at the neck. The hind paws being then irritated by pricking them, and by touching them with a hot wire, the muscles belonging to the posterior extremities were thrown into contraction, so as to produce the motion of shrinking from the injury. The same effects were perceived in another kitten, after the head was entirely separated from the body. In repeating this experiment, I found that when the spinal marrow was cut through, between the lumbar vertebræ and os sacrum, the posterior extremities lost their irritability, but the part below it, the tail, retained it. It might, therefore, be said that the spinal marrow below the division served as a sensorium; but it may be answered that when the head is cut off its irritability remains, as appears by the motion of the ears when pricked or touched with a hot wire; and as the extremities are also irritable, it will not be said that consciousness and sensation exist in two separated portions of the same body. Nor can it be admitted that sensibility and consciousness may remain in the head after separation; for if mere compression of the carotid arteries abolishes sensation and thought, by interrupting the circulation in the brain, how much more must the superior violence of decapitation have this effect."

Without considering the reasoning in the latter part of this quotation as altogether conclusive, we may point out that Sir G. Blane deduced from his experiments the very same inference which Dr. M. Hall labours to establish; and, as far as the *experiments* of the latter gentleman are concerned, with the same validity; since, as we have already remarked, no *certain* proofs of the absence of sensation can be derived from any source but the human subject. After alluding to the motions of anencephalous monsters as supporting his argument, Sir G. Blane concludes—"These facts show clearly that instinctive, or rather automatic, motions may be excited without the intervention of the sensorium commune, and therefore *without sensation or consciousness*."

Legallois made a great step in the investigation, by demonstrating the connexion of the sympathetic and excited actions with the spinal cord alone. His work, entitled "*Experiences sur le Principe de la Vie*," was published in 1812, and consists of Memoirs which had been at different times presented to the Institute and to the Faculty of Medicine in Paris. His great object was to prove that the "principle of life" resides in the spinal cord; that the action of the heart, the respiratory movements, and the contractions of the muscles of the trunk are directly dependent upon it alone; and that it is consequently entitled to rank as a distinct and most important organ, instead of being regarded as a mere bundle of

nerves, which seems to have been the estimate formed of it by many of the author's countrymen. By his experiments on the respiratory movements, Legallois showed that the brain itself is not essential to their regular performance; since every part of the cranial contents, except the medulla oblongata, might be removed without interrupting them.* He also proved by experiment that the "life" of every part of the trunk is dependent upon the integrity of its nervous connexions with the spinal cord; and that the integrity of that portion only of the latter organ with which the nerves are connected, is essential to the manifestations of life.† In what these manifestations consist he has explained at the commencement of his work, where he speaks of sensation and motion as the indications of the presence of a living principle. In his zeal to establish the important character of the spinal cord as a distinct organ, Legallois undoubtedly went too far, for we find him maintaining‡ that, when the spinal cord is divided, two centres of volition and sensation are produced: but the results upon which this conclusion was founded are precisely those upon which Blane was led to the inference that motions may be excited through the nervous system without the intervention of sensation; for he found that when the spinal cord of a rabbit was divided below the last dorsal vertebræ, the posterior extremities were agitated when the tail or one of the feet was compressed. In regarding the "principle of sensation and motion" as residing in the spinal cord, and attributing to the brain the function of determining and regulating the actions produced through the medium of the former, we regard Legallois as having greatly erred; but we must not overlook the high value of his researches, both as to the facts which he developed, and the direction of the regard of physiologists towards the unquestionably distinct powers of the spinal cord.

We think it not improbable that many persons who have devoted much attention to *mental* phenomena alone may have arrived at conclusions similar to those which physiologists were attaining by experimental research. No one is likely to arrive at accurate conclusions on subjects like the present who does not add to observation and experiment more or less of acquaintance with the powers and operations of the mind; and it will frequently occur that the results of the two distinct kinds of investigation, physiological and psychological, will confirm or check one another. We can point to one author, at least, on the latter department, whose conclusions will, we think, interest those of our readers who agree with us in the advantage of an extended system of enquiry on topics like the present. The writer of the article *Mental Philosophy*§ in Rees's *Cyclopædia*, (published in 1814,) inferred from the consideration of psychological phenomena only, that many actions, both those constantly occurring in the system, and those at first voluntary but subsequently habitual, might be excited through the *sensorial organs* without *sensation* being necessarily produced. He carefully distinguished the *sensorial changes* through which sensation is produced in the mind, from the sensation itself; and argues at some length to prove that in many instances motions are excited by these sensorial changes produced in obedience to an external impression, the mind being unconscious of them either through its direc-

* Op. cit. p. 38. † Ib. p. 34. ‡ Ib. p. 61.

§ The Rev. Dr. L. Carpenter.

tion to other objects, or its state of torpor, as in sleep or apoplexy. If this writer had been acquainted with the experiments of Whytt and Legallois, and with the fact that all nerves are primarily connected with the spinal cord, he would probably have perceived that for the *sensorial changes* to which he alludes this organ alone is required.

None of our readers can be ignorant that, about fifteen years ago, a great advance was made in nervous physiology by the experimental researches of Bell and Mayo in this country, and those of Magendie, Flourens, Fodera, and others on the continent. The results of Magendie's experiments confirmed and extended the views which had been stated by Bell many years before; and in the same manner, the researches of Flourens were little more than a repetition of those of Rolando, also of older date, but little known beyond the country where they were published. The anatomical distinctness of the motor and sensory fibres in the nervous trunks necessarily led to the inference which had been previously supported on other grounds, that the automatic and sympathetic muscular movements can only be produced by the transmission of the external impression to the central organs, and the propagation to the circumference of a motive stimulus. The researches of Rolando, Flourens, Magendie, and other vivisectioners, to which we have already alluded, limited the parts of the nervous system necessary to the performance of these instinctive actions to the spinal cord and its cranial prolongations; and the general results set forth by Flourens appear to us to contain nearly all that Dr. M. Hall can be said to have demonstrated. Flourens clearly separated the *excitability* of the nervous fibre, (namely, that power of receiving impressions and of giving rise to functional changes which Bichât included under the general term of *sensibility*,) from true sensibility and voluntary action.* The former he considered as appertaining to the spinal cord and nerves, regarding the nervous trunks as the *excitors* of sensation on one side and of motion on the other, and the spinal cord as the organ by which the *irritations are generalized*, and by which all those sympathetic actions are performed which result from a *transmission of irritation*. He distinctly states that it is in the brain alone that the consciousness of these changes is produced, but that this organ is not concerned in their performance. Not being aware of the distinctness of the sensory and motor fibrils of the nerves, he could not so accurately refer to the transmission of stimuli from the circumference and their reflexion from the centre, as physiologists have subsequently been enabled to do; but we maintain that the knowledge of the facts which he brought forward, when refined and specialised by the splendid discovery of Bell, could not lead to any other conclusion. It is easy to explain the want of attention which the inferences of Flourens himself received in this country. The conclusions drawn from his experiments by Cuvier and his adjuncts in their Report to the Academy, as to the permanence or extinction of sensibility, were precisely opposite to those of their author; for the phenomena which led Flourens to deny the sensibility of the spinal cord, and to point out its excitability (more properly *impressibility*) as a distinct faculty, were regarded by them as proving the persistence of sensibility in that organ,

* Recherches Expérimentales sur le Système Nerveux. Paris, 1824.

and the independence of the cerebral hemispheres regarding this function, except so far as the special sensations are concerned. Founded upon this Report is the statement of Dr. Alison (*Physiology*, p. 131,) that "it is now satisfactorily ascertained that no part of the brain, higher than the corpora quadrigemina, nor of the cerebellum, is essentially concerned in sensation;" or in other words, "that we are to attribute to the spinal cord, and the nerves arising from it, all the physical conditions that are necessary in order that sensation may be felt, and that voluntary efforts may excite muscular contraction." These opinions are now, we think, shown to be invalid by the experiments and pathological observations which we have detailed; and we cannot longer refuse to Flourens the credit of having made a most decided advance in this branch of nervous physiology.

In Dr. Alison's paper on Sympathy* published in 1826, will be found a very full statement of the views of Whytt as modified and rendered more precise by the discoveries of Bell; the object of this profound and sagacious writer was to show the fallacy of the explanation of the respiratory motions propounded by the latter; and to prove that none of the associated actions can be explained by structural connexions between the origins of the nerves. This essay may be looked upon as presenting a fair view of the questions it embraces by one who refused to admit the inferences of Flourens; and we find it distinctly propounded that the sympathetic actions in general are excited by sensations whose influence is *reflected* downwards to parts frequently distant from those on which the stimulating impression is made. Although the writer speaks of the *brain* as connected with them, he was evidently aware that only that portion of the encephalon is essentially concerned which is now usually regarded as the cranial prolongation of the spinal cord.†

We have shown, then, that at this period (about twelve years ago,) the doctrine of the excitement of associated or sympathetic movements by external stimuli acting through the nervous circle of which the spinal cord (to the exclusion of the brain proper) is the centre, was very well understood; that it was also known from the experiments of Legallois that any single division of the spinal cord would act as the centre to the portions of the body connected with it by nervous trunks; and that Dr. Carpenter and Flourens had maintained (the one on psychological, the other on physiological, grounds,) that for this excitement *sensation* is not essential, but rather that series of changes which are antecedent to sensation and which, in the usual state of the system, give rise to it.

We are now, therefore, advanced sufficiently far in our survey to enter upon the claims of the authors of the present time; and we shall briefly consider those which Mr. Mayo has set forth in the pamphlet before us, of which the first portion alone is connected with the subject under discussion. The following are his reasons for thus presenting himself to the public.

"I find that in my former writings in which I have probably too much studied conciseness, I have failed in stating my opinions upon the points referred to so clearly as to prevent their being misunderstood. I have therefore determined to restate them

* Trans. of Edinb. Medico-Chirurgical Society, vol. iii.

† See British and Foreign Med. Review, Vol. III. p. 580.

in a more conspicuous manner and apart from the enquiries with which in my former works they are mixed up. I may mention that I should probably have hesitated to intrude a recapitulation of the following views of these subjects upon the physiological reader, but that I believe them to be susceptible of various practical applications and to form the basis of that true theory of the nervous system which several sensible men in my profession have recently told me they anticipate will be discovered before long." (Pp. 3-4.)

Mr. Mayo attributes to Magendie the honour of the discovery, that the part of the medulla oblongata, corresponding with the root of the fifth nerve, is essential to the perception of all sensations but those of sight, and then gives us his own "larger view" and "juster conclusion," in the following words:

"I believe the segment of the medulla oblongata in which the fifth nerves arise, (or, as I have here more closely phrased it the root of those nerves,) to exert an influence not only *downwards* along the spinal chord, but *upwards*, likewise, towards the brain. I believe that the participation of every part of the nervous system in consciousness depends upon its continuity by nervous substance with that segment. I believe that the organs in which thought and reflection are seated, namely, the cerebrum and cerebellum, are as much dependent for the continuance of their functions on their continuity with the upper part of the medulla oblongata, as are those of the organs of sensation and volition which exist in the spinal chord." (P. 13.)

A little further on, Mr. Mayo adds the following as the "logical deduction" from the facts he has adduced, and others parallel to them, "that the spinal chord has no part in consciousness, and that the cerebrum and cerebellum are equally excluded from their part in consciousness, when they are separated from the medulla oblongata," (p. 16.) We confess that we cannot understand how Mr. Mayo can reconcile the *existence of organs of sensation and volition in the spinal chord*, with its non-possession of *consciousness*. We do not imagine that the spinal cord is possessed of sensibility, or that an animal is conscious of any impressions confined to it; nor, on the other hand, do we believe that although sensibility is a property of cerebral matter, it can be called into exercise, and a sensation produced, by any other means than an impression propagated along the nervous trunks. The humours of the eye will form an image upon a piece of paper as well as upon the retina, but no vision is produced unless the latter be perfect; and the retina cannot conduct to the sensorium any visual impression except that formed by the organ appropriated to it.

Mr. Mayo claims for himself the first enunciation (in his *Anatomical and Physiological Commentaries*, Part ii., 1823,) of the principle that "an influence may be propagated from the sentient nerves of a part to their correspondent nerves of motion, through the intervention of that part alone of the central organs of the nervous system to which they are mutually attached." But we have seen that the same principle, making allowance for the then prevailing ignorance as to the anatomical distinctness of the motor and sensory fibrils, had been distinctly laid down by Legallois many years before. He also claims the announcement in the same publication, of the idea of the "reflex function," which he allows to Dr. M. Hall the credit of having "followed out with great diligence." We leave the adjustment of this claim, and also the appreciation of its importance to the readers who have attentively followed us in the preceding investigation.

In an addendum to the last edition of his Physiology, Mr. Mayo speaks of having "carefully distinguished" the influence directly "propagated from the sentient nerves of a part to their correspondent nerves of motion," from the "agency of sensation and volition." Now, after careful examination, we can nowhere find that he has gone further than the hypothesis which he advanced in his Anatomical Commentaries, viz. that the connexion between *sentient* surfaces and the action of voluntary muscles may produce motion independently of the *will*; and that the influence independent of will, which occasionally throws voluntary muscles into action, may possibly regulate the unconscious movements connected with the emotions, &c. This hypothesis may have been new to Mr. Mayo's own mind, yet we cannot perceive anything in it which had not been previously advanced; and there certainly is nothing in his statement of it implying that he considered *sensation* as well as *volition* to be unconcerned in the movements in question. Even this limited view, however, he subsequently relinquishes; and returns to his former opinion, that all instinctive actions, and even those usually termed automatic, are effected by a voluntary impulse. Thus, with regard to the movements of the pharyngeal muscles in deglutition, he "prefers to think" that "by an instinctive law, a voluntary impulse is issued along" the motor nerves in obedience to the *sensation* produced in the *medulla oblongata*; rather than to suppose that volition regulates some movements, and a different principle others, (pp. 21-23.) Certainly Mr. Mayo's notion of volition must be rather more comprehensive than that usually entertained.

It is a little singular that Mr. Mayo should quote Whytt as a supporter of the doctrine that instinctive actions (in which he includes automatic also) are voluntary; for though Whytt attributes them to the *sentient principle*, he carefully distinguishes this from the *rational* principle of the soul, by which alone, according to him, voluntary movements are excited. In the passage which Mr. M. has quoted from the Essay on Vital and other *Involuntary* Motions, as "ringing like true metal," the discerning reader will perceive that Whytt introduces the case of certain undoubtedly voluntary motions, which by habit are performed without a conscious effort of the will, as illustrating, by way of analogy only, the unconscious action of the sentient principle of the mind in producing the movements connected with the organic functions. This error on Mr. Mayo's part, appears to have arisen from an imperfect examination of the whole of Whytt's essay, and the want of due allowance for his peculiar phraseology of which *his own* explanation should be taken. Mr. M.'s present opinion is thus expressed: "Each segment of the double chord, with the nerves arising from it, contains the entire mechanism of sensation, instinct, and volition, although these endowments in the otherwise perfect state of the segment are not manifested unless it be in continuity with the medulla oblongata." (P. 27.) Allowing for the obscurity of this passage—the obvious import of which seems to us to be that in the ordinary state of the spinal cord, the medulla oblongata is the channel through which the endowments of "sensation, instinct, and volition" are manifested, and that when disconnected with it the distinct segments do not manifest them at all, but yet are in possession of them,—we cannot see any essential

difference between the idea which Mr. Mayo probably intends to convey, and the inferences drawn by Legallois nearly thirty years ago, to which we have already alluded.

In thus freely criticising the views and opinions, and claims of Mr. Mayo, we are influenced only by one motive, the love of truth; and if we are proved to be in error, it will give us the utmost pleasure to confess it, and, as far as in us lies, to repair the wrong. None can be more willing than ourselves to allow to him whatever credit is justly due to him. He has undoubtedly contributed much, both by his experimental and anatomical researches, to the establishment of results of the highest importance in the physiology of the nervous system; and would he be content to rest his claims to distinction upon his universally acknowledged merits, we think that he would attain a place in general estimation to which such publications as the present are not likely to elevate him.

We shall next advert to the paper of Professor Müller on the "Reflexion of Motions after Perception,"* in which he amplifies the views which he had previously expressed on this subject in his *Handbuch der Physiologie*. As far as these are concerned we must still maintain the appreciation which we have formerly expressed,† that they contain no opinion which had not been long entertained in this country, being, in fact, only the doctrines of Whytt, with the modifications necessarily resulting from the discoveries of Legallois, Bell, Flourens, &c. ingrafted upon them. That this learned physiologist should have himself regarded them as novel we can only understand by supposing that the opinions of Tiedemann; respecting the dependence of associated movements on the sympathetic nerve were then prevalent in Germany. Now Dr. Alison had, many years previously to Professor Müller, laid down very clearly the distinction between the *organic sympathies*, which he refers to the *great sympathetic*, and the *sympathetic movements*, which he justly regarded as dependent upon the cerebro-spinal axis. The experimental researches of Brâchet, too, led to the same conclusion; and indeed this physiologist made out a distinct case of *reflex action without sensation*, in maintaining that the motions of the paretics of the stomach depend upon an impression conveyed to the medulla oblongata by the par vagum, and transmitted down again in the form of a motive impulse. Although not according with Dr. M. Hall in several points, Professor Müller seems disposed to admit that *sensation* is not an essential link in the chain of reflected actions; for he observes‡ that "the reflected motions on stimuli of the skin, which take place after the removal of the brain, do not contain any proof that the stimulus excited true sensation in the spinal marrow; it is rather the centripetal conduction of the nervous principle which commonly takes place in sensations, but which here is no longer sensation, because it is no longer conducted to the brain, the organ of consciousness. During health, also, numerous reflected motions result from stimuli of the skin, which do not come as true sensations to the consciousness, but still may excite violent impressions on the spinal marrow." He also argues in

* Philosophical Magazine, vol. x.

† British and Foreign Medical Review, vol. iii. p. 581.

‡ Op. cit. p. 188.

favour of an opinion which we have taken some pains to uphold, that involuntary movements may arise from sensations (particularly those of a special nature,) as directly as those characterized as reflex or excitatory by Dr. Hall from impressions only.

To those who have followed us through the preceding retrospect, we think it will appear that the following opinions had been maintained by different authors previously to the publication of the researches of Dr. Hall and Professor Müller : 1. That the sympathetic or associated movements are involuntarily excited by a stimulus conveyed to the central parts of the cerebro-spinal nervous system along one portion of the nervous trunks, and centrifugally transmitted by another. 2. That for the reflexion of this stimulus, the brain is not necessary, but the spinal cord and its nerves alone. 3. That any segment of the spinal cord may act independently of the rest, so far as the parts of the body are concerned, with which it possesses nervous communications. 4. That sensation is not an essential link in the chain of processes, this mental condition being a result of those organic changes which are excited by the external impression, and which propagate its influence to the muscles.

We do not mean to assert, however, that any one physiologist maintained all these doctrines, in the form in which we have expressed them; although Flourens undoubtedly approached so near to them, that, if he had been acquainted with the previous investigations of Bell, we cannot but think that he would have been led to see their bearing upon his own conclusions. Now, it appears to us that it is in the harmonious combination of these doctrines into a uniform system, and the application of it to the explanation of many phenomena which were not formerly regarded as explicable on such principles, that Dr. Hall's great merit consists, and not to anything particularly novel in the opinions themselves. The claims to *discovery* on a question like the present are very difficult to adjust, if all the vague thoughts which have preceded them respecting the subject they involve are allowed to be brought in evidence of previous right;—thoughts, too, which would perhaps have been forgotten, if the clear statements of the writer whose claims to originality are opposed had not revived them. Admitting that this is in some degree the case with regard to Dr. Hall, we still think that the clear statements of the previous authors we have quoted will, if fairly examined, bear out the opinion we have now given. We have been pleased to find that so able and intelligent a writer as Mr. Owen has come to precisely the same conclusions with ourselves as to the validity of Dr. Hall's opinions and the justice of his claims.

“The essential character of the actions of the brain, whether as a recipient or transmitter of impressions, is consciousness of the action: but this property of consciousness is not possessed by the spinal cord; probably not by the medulla oblongata. Whenever, therefore, an impression received by an excitable nervous fibre is transmitted through the unconscious part of the central axis to the exciting fibre of muscular motion, the latter phenomenon is unaccompanied with consciousness or sensation. That the spinal cord, or any segment of it, possesses the power of transmitting an impression from an excitable to an exciting nervous fibre, has been known and admitted as a fundamental fact in physiology since the experiments of Whytt, Blane, and Mayo. To the latter physiologist we are more especially indebted for the most decisive experiments in proof, and the clearest enunciation, of this property of the central

nervous axis. Recently the automatic animal motions resulting from this property have been grouped together more *extensively than had before been done, and the morbid phenomena ably traced out by Dr. M. Hall.*"*

Dr. Hall claims, however, much more than this: he thinks that he has succeeded in demonstrating the existence of a "System of Excito-motory Nerves, *physiologically* distinct from the nerves of sensation and voluntary motion." It is a little remarkable, however, that those writers who were disposed to accord with his views in other respects, have, in almost every instance, expressed their conviction that this supposition is unphilosophical, because unnecessary; and that there is no reason to suppose that the very same fibrils may not conduct those impressions to the spinal cord, which, acting through it alone give rise directly to motive influence, or, propagated to the brain, cause sensations to be formed in the mind. This was the opinion of Professor Müller,† Mr. Carpenter,‡ Dr. J. Reid,§ of Mr. Owen,|| and others. We shall quote the remarks of the last, as giving, in our opinion, a very fair statement of the question.

"There is not a single phenomenon of automatic motion in parts supplied by spinal nerves which may not be accounted for on the demonstrated property of the central axes to transmit impressions from the excitable to the exciting nerves, at any part where they are connected to it, independently of the rest; and I am at a loss to understand why impressions, so received by the spinal cord, should not also be transmitted by the brain, (its continuity with that organ being uninterrupted,) without the necessity of supposing a class of nervous fibres for conveying, in this case, impressions to the spinal cord, distinct from a second class which are supposed to transmit to the motive fibres those impressions which are not afterwards propagated to the brain."

Mr. Owen shews himself, however, quite willing to modify his opinions on the subject, if called upon to do so by new facts; for he says, at the same time, "It remains to be seen whether anatomy will establish the existence of these four classes of nervous fibres, which, so far as I can understand Dr. Hall's hypothesis of the reflex function, are called in to account for the voluntary and automatic muscular motions."

This question we cannot regard as by any means decided by the investigations of Mr. Grainger. Regarding the anatomy of the spinal cord this gentleman has not, as we have shewn, arrived at any peculiarly novel results; and, without calling in question a single one of his facts, we must take leave to withhold our assent to the theory erected upon them. Not because any direct and certain evidence can be produced against it, however, do we regard it as unstable, but because we cannot discern either solidity in its foundation, or firmness and connexion in the superstructure. It is to be remembered that the doctrine of the relative functions of the grey and white matter is by no means entitled to rank as a physiological certainty; and, however ingenious may be the deductions drawn from it, they must be looked upon with great suspicion, unless they meet with substantial support elsewhere. We may take this opportunity of expressing our high appreciation of the general character of Mr. Grainger's work, from which it will be evident that we have drawn much valuable

* Hunter's Works, vol. iv. p. 202, note.

† Op. cit.

‡ Edinburgh Med. and Surg. Journal, vol. xlviii. p. 43.

§ Ditto, vol. xlix.

|| Loc. cit.

information; and, although we differ from him in several important physiological views, and in the estimation we attach to the labours of Dr. Hall, we willingly bear testimony to the very candid and discriminating spirit which generally pervades his book.

We have but a few words more to say of Dr. Hall. His want of accurate acquaintance with the opinions of older physiologists, to which we have already alluded, has led him to regard many of his doctrines as novel which are very far from being so. To some of his pathological explanations we have alluded in a former article,* and have shewn that they differ only in words from those commonly given. With regard to the function of respiration, again, the comprehension of which within his excito-motor system he regards as a great advance in the enquiry, he seems quite unaware that, before the time of Bell, it was universally considered as on a par with other associated movements. Whytt states this as clearly as possible, and describes the action of the stimulus originating in the lungs, and transmitted to the muscles through the sensations produced in the brain, (or rather, to use his own language, through the excitement of the *sentient principle*, not always implying *sensation*.) The great pains which Bell took to set it in a different light produced a corresponding effect on the minds of many physiologists, particularly in England; but his *respiratory system* was never received in an unqualified manner in any but English schools; and, as we have already seen, the question was early placed on its true footing by Dr. Alison. The introduction of the function of respiration by Dr. M. Hall, therefore, into his excito-motor system, was merely to restore it to its true affinity with those other associated movements from which (following the doctrine of Bell) he had at first unjustly separated it.

With one more observation we shall conclude our references to Dr. Hall's doctrines and claims. He imagines that to him is due the establishment of the fact that the nervous influence may operate in directions not only incident and reflex, but also direct and retrograde, or, in other words, downwards and upwards, in regard to the spinal marrow. Now, this is saying nothing more than that a stimulus affecting one part of the body may produce motions in other parts whose nerves arise either above or below the insertion of those which convey the impression to the spinal cord. This was a fact known to every one long before Dr. Hall's time; and, although in the view of those who regarded the spinal cord as a mere bundle of cerebral nerves it might be a matter of astonishment that such a retrograde action could occur, no physiologist who considered it an independent organ would have more difficulty in accounting for the fact that a stimulus conveyed to the lumbar region might excite the motor nerves of the cervical portion, or that a stimulus incident on the dorsal region might excite both cervical and lumbar motor nerves, (in the first of which cases the action would be *retrograde*, according to Dr. Hall, in the second both *retrograde* and *direct*,) than that a stimulus affecting the cervical portion of the cord should be conveyed (*directly*, according to Dr. Hall,) to the motor nerves of the back or loins.†

* Vol. III. p. 39.

† A passage in one of Dr. M. Hall's recent Lectures (*Lancet*, February 17, p. 729,) induces us to refer to one topic, which we should not otherwise have thought it neces-

In taking our leave of Dr. Hall, we would assure him and his partisans that we have been actuated by no motive, in the examination into his claims which we have thought it our duty to make, save an honest desire to arrive at the truth, and to set their investigations on their proper footing; and we would remark, that we are quite sure that these claims would have received more attention in various quarters had they been more moderate and discriminative.

ART. XIV.

Changes produced in the Nervous System by Civilization, considered according to the Evidence of Physiology and the Philosophy of History.

By ROBERT VERITY, M.D. &c.—London, 1837. 8vo. pp. 79.

ONE of the first things that struck us, on taking up this production, was the disparity of dimension between the subject and the volume in which it is treated. The subject is one of great interest and of vast extent; it would, indeed, afford many years of occupation to the most powerful and comprehensive mind, aided by a wide range of historical, physical, and metaphysical learning; yet all that Dr. Verity has to say upon it is comprised within seventy-nine pages. In this small space, however, he contrived to pour out such a flood of words, that his reasoning, if such it can be called, is nearly drowned. If there were many such books, we should stand in need of a literary humane society for the recovery of submerged ideas.

The general scope of our author's argument is to demonstrate that the progress of civilization has been attended with a proportionate improvement in the organization of man, and especially in that of the nervous tissues; the cerebral functions acquiring increased power and activity as the moral and intellectual nature gains the ascendant over the animal.

sary to discuss. Dr. H. continues to take great credit to himself for having identified the new "motive power," which he regards himself as having been the first to demonstrate, with the *vis nervosa* of Haller; and for having rectified an important misstatement on the part of Haller and Professor Müller as to its propagation along the nerves. Now the fact appears to us to be, that Haller employed the term *vis nervosa* in precisely the sense which we and others have given to the term "motive influence," designating by it that organic change in the nervous fibre which is *originated* in the central organs of the nervous system, and which, by its *centrifugal* propagation, excites contraction of the muscular fibre. This motive influence may be produced either by an act of the will or more directly by a stimulus conveyed to the cerebro-spinal axis through the centripetal or afferent nerves; which stimulus, if further conveyed to the cerebrum, excites sensation. We regard Haller and Müller as perfectly correct, therefore, in stating that the *vis nervosa*, or *motor power*, acts only in the direction of the branches of the nerves. It is the stimulus by which this is excited in the central organs that is transmitted in a contrary direction. But, further, Dr. Hall is evidently wrong in the identification he has attempted; for, according to him, the excito-motor system is anatomically distinct from the cerebral or volitional, and therefore the motor power which acts through it cannot, according to his views, be the same as that derived from the brain, which organ is specially referred to by Haller as the originator of the *vis nervosa*. Dr. Hall might as well say that "motive power" is transmitted along the sensory nerves when an action is the result of the production of a *sensation* in the brain, as that it is transmitted along the afferent spinal nerves; for these only convey the impression by the stimulus of which to the spinal cord the change is produced in the efferent or motor nerves that constitutes motive power.

In order to make good this position, Dr. Verity takes a rapid survey of the partial and transient civilization of the great nations of antiquity; of the dark period that ensued, till the gradual commingling of races and of manners gave rise to more permanent principles of improvement; and of the influence of the Crusades, of chivalry, and other causes, which are generally acknowledged to have contributed to the formation of the present state of society. In his view of the character of these ages, and the operation of these causes, Dr. Verity does not appear to differ from the majority of philosophical historians, and for this very reason we think his observations entirely superfluous; the rather that his professed object is not to give a history of civilization, but to shew that civilization has produced a certain effect on the physical constitution of man. What is still more unfortunate, when he comes directly to the point of proof, he contents himself with telling us that the thing *must* have been so!

"As no human manifestation whatever can be allowed to take place without also its material conditions in the body, the physiologist knows well that the nervous system must have felt the influence of the general impulse and movement in the same full and equal degree as the historical evidence extends, and that the corresponding parts of the great nervous centres, from having been directly excited by this newly-created and additional activity for many generations, must have taken up, by successive accumulations of nutrition, an amplified form and size, permanently fixed in the organization, transmissible from parent to child, and forming, in short, an important step towards an improved character of type. The appearance of every new element of activity in the history of a race must ever be, to the physiologist, evidence of something added or modified in the nervous system. There is a rigid concatenation between the two circumstances." (P. 29.)

If physiologists know all this well, which we admit to be the case, to what purpose is the argument with which our author has been previously occupied? The truth is, he has been vainly seeking, in the history of past ages, for the evidences of a physiological fact which is amply demonstrated in the ascending scale of cerebral organization, as exhibited by modern anatomy, but on which history throws no light, because it is not the province of history to record the dissection of brains.

There are some other physical changes alleged by our author to result from civilization, concerning which we are very sceptical. Thus, we are told that "the bony structure, losing all excess of bulkiness and porosity, becomes denser, more compact, and more finely grained; gaining in strength and specific gravity what it loses in softness and clumsiness of size." (P. 59.) We should have liked a few examples of this sponginess of uncivilized bones; in the absence of which, we will give an example to the contrary that just now occurs to us. When the skeleton of Robert Bruce was discovered in 1819, it was examined by Professor Monro, who observes, "it was remarkable that even the very thin bones of the orbit were quite entire, as also all the processes of the bones at the base of the skull; even the pterygoid processes of the sphenoid bone and the palate bone."* The bones of the Scottish hero, then, must have been exceedingly well compacted, or their more fragile parts would not have lasted so long; yet he died in the comparatively rude era of 1350. We must further confess that the examinations we have made on various occasions of ancient bones, as compared with modern ones, and the bones of sa-

* Monro's Elements of Anatomy, vol. i. p. 21.

vages, as compared with those of civilized men, have not led us to the conclusion that their intimate texture is at all influenced by the degrees of civilization.

Dr. Verity, moreover, informs us that, "besides the amplified volume and enhanced temperament of the cerebral masses," which result from civilization, "the different structures of the body become interpenetrated with a more copious interlacement of nervous webbing, whereby all the complicated mechanism of animal and organic life is made to perform its various functions with more energy, more breadth, and more endurance." (P. 60.) This passage contains a most outrageous assumption. We admit that the sensibility of several of the textures is increased by circumstances connected with civilization; but we have yet to learn that this depends on an increased quantity of nervous matter interwoven with those textures; nor is there on record a single anatomical fact to bear out Dr. Verity's assertion.

Attendant upon all the progressive stages of civilization, great changes took place in diet, and the general hygienic circumstances, which, arising from increased moral refinement, reacted in their turn upon morals, through the medium of an improved physical organization. So says our author, though with greater copiousness of diction than we approve of or have room for. This, no doubt, is true to a certain extent: it is not to be forgotten, however, that gastronomy is the daughter of civilization.

Besides the matters above commented on, we find a proposal for a continued series of observations on the development of certain parts of the encephalon, in connexion with the social progress of man; a thing much to be desired, and to which phrenology, when placed on a more philosophical basis than it stands at present, will probably conduce. We find also certain precepts on the adaptation of temperaments and constitutions in marriage, which, we fear, can never be reduced to practice; and many observations about things in general, with which we do not think it needful to trouble the reader.

In the preface, Dr. Verity announces his intention of publishing, at some future period, "some views on the principles of medicine, considered in relation to the modified type of temperament produced by the increasing proportion of the nervous element in the organization of individuals who are fair representatives of the high civilization of modern times." To these views the present work is intended as an introduction; but, unless the author can bring himself to much severer habits of thinking, and a much denser style of writing, we would by all means dissuade him from the labour he contemplates.

Without at all knowing how the fact may be, we should strongly conjecture that Dr. Verity had read too many of those shallow and bombastic productions which encumber the fine field of German literature; for, although this book is written in the English language, it strikes us as essentially German in its style; and truly it yields not in feebleness and verbosity to the most inane productions of the German school.

PART SECOND.

Bibliographical Notices.

ART. I.—*Researches into the Physical History of Mankind.* By JAMES COWLES PRICHARD, M.D. F.R.S. &c. &c. *Third Edition.* Vol. II.; containing *Researches into the Physical Ethnography of the African Races.*—London, 1837. 8vo. pp. 373.

THE first volume of this work has been already briefly noticed by us: the second, now before us, contains a continuation of the enquiry into the relation of the human races to each other, as exemplified in those of all parts of the vast continent of Africa. In the previous portion of the work Dr. Prichard endeavoured to shew “that no remarkable instance of variety in organization exists among the human races, to which a parallel may not be found in many of the inferior tribes;” and also, “that all human races coincide in regard to many particulars, in which tribes of animals, when specifically distinct, are always found to differ.” He proceeds in this volume to investigate the nature of the organic diversities in mankind in a different way, and to enquire how far the characters of particular tribes enquired into have been permanent, or in what respects subject to variations. The question here to be decided is, whether the distinguishing characters of the human races have been constant and undeviating within the period of time to which historical testimony extends. That they have been thus constant and undeviating is, Dr. Prichard observes, a very prevalent opinion, and is strongly opposed to the admission of the unity of the species of mankind; and, to decide this important point, he enters on an investigation of the physical history of particular races of men or families of nations.

Dr. Prichard considers it altogether hypothetical to divide mankind, as has been so often done, into a few classes or groups resembling each other in physical character, and to assume that such groups constitute races or lineages, of which the members are more nearly allied than to tribes of different physical peculiarities. Avoiding all attempts to distribute mankind into departments on principles purely conjectural, he sets forth his intention to be to “proceed in a geographical arrangement, to examine the phenomena which present themselves in the population of different regions of the world.” In this manner he is of opinion that an opportunity is also afforded of more correctly marking the effects of physical agencies in the development of varieties of breeds, or in the origination of new or diversified races. But many circumstances, he remarks, must be taken into such an investigation beyond the relative distance of different climates from the poles or the equator. There are conditions to be taken into the consideration which often differ much in the same latitudes. Dr. Prichard, therefore, endeavours to ascertain “what are the most remarkable fea-

tures in the physical geography of each region, and what relations the origin and development of varieties in families or tribes may bear to all these local conditions."

The reader who feels desirous of information on this most interesting subject cannot peruse with too much attention this most important and difficult part of Dr. Prichard's subject, relating to the races which constitute the population of Africa. He will there find many remarkable and some unexpected particulars, calculated, we think, not only to throw much light upon the enquiry which they principally illustrate, but to enlarge our conceptions of the capacities and destinies of some portions of mankind, of which the inferiority has been asserted on insufficient foundation, and acted upon without kindness or humanity. He will also, we think, see abundant proofs that the characteristic qualities of human races are *not* permanent and undeviating. Of the various facts accumulated in this volume, and by which the very learned and able author establishes these points, no analysis or abridgment could give the reader an adequate idea. The whole work must be carefully gone through, in order to appreciate the perfection of the powerful chain of reasoning which binds observations so extended and so various into satisfactory proofs of the doctrines propounded.

ART. II.—*A concise Treatise on Operative Surgery; describing the Methods adopted by the English, Continental, and American Surgeons: selected for the use of junior Practitioners and Students. Illustrated by twelve Plates.* By W. P. Cocks, Surgeon.—London, 1837. 8vo. pp. 375.

MR. COCKS'S work is confessedly a selection. The plan of it is to give an account of the mode of performing operations, without any mention of the symptoms or circumstances which render such operations necessary. To this plan we should have very decided objections, even if it were ably and skilfully effected. To detail the mere manual part of surgery,—to string together receipts for the mode of doing operations, without first giving the student and young practitioner, for whom the work is "selected," some guide as to when he ought to operate, and when he ought not, appears to us extremely injudicious. The omission, too, of the slightest hint as to the probable failure or success of the different operations is very objectionable. The plan of this work, then, we think, is decidedly bad; and we are sorry that a sense of justice to our readers compels us to add, that the manner in which it is executed is slight, superficial, careless, and not unfrequently erroneous. Mr. Cocks states in the preface, that "the operator, referring to the index, puts his finger at once upon the best recorded method of accomplishing his design; and, with the text before him, he can act promptly and efficiently." We refrain from formally ridiculing the idea of Mr. Cocks's work being placed upon a reading-desk, or in the hands of an assistant, in order that the operator's knife should follow the direction of the text; but we must observe, that "the finger" can *not* "be put at once" upon the "best recorded method;" for the simplest of all possible reasons, because the selector first describes one way of operating; and then "another me-

thod," and "another method," and sometimes even another; but no mention is ever made of which *is* the best.

We do not think it incumbent upon us to enter into a detailed critical examination of the work: if we did, we should find no difficulty whatever in producing examples to shew that Mr. Cocks sometimes mentions well-known facts in such loose and indefinite language as to lead the reader to no positive conclusion; and that at others he describes operations which are now nearly exploded, omitting any mention of the really "best recorded method," but selecting for the student that which is but rarely practised. Proofs of the carelessness with which the work has been sent from the press are by no means rare. For example, at page 233, we are referred in the text to *Plate 8th* as illustrative of hernia, but *Plate 8th* illustrates other subjects. Again, at page 306, the reader is referred to *Plate 11th* for the "surgical means of suppressing hemorrhage," but this plate depicts the appearance of hernia. Mr. Cocks seems to have an insuperable objection, too, to the ordinary mode of spelling the names of the different authorities he quotes: thus, we have "La Dran," "Baron Heurtelop," "Caviale," &c. It may appear hypercritical to mention these mistakes, and we certainly should not have done so if the work, in general, had possessed any redeeming merits. It has often been said, and truly too, that a medical practitioner may fail completely in a court of justice, in giving clear and satisfactory evidence upon a subject with which he is well acquainted, and upon which he would *act* skilfully and efficiently. The same observation applies to written statements, which may be loosely worded or injudiciously compiled, and yet the writer may be a very good and safe practitioner: and this may very probably be the case with Mr. Cocks.

ART. III.—*The Question concerning the Sensibility, Intelligence, and Instinctive Actions of Insects.* By DAVID BADHAM, M.D.; one of the Radcliffe Travelling Fellows of the University of Oxford, &c.—*Paris*, 1837. 8vo. pp. 54.

THE ingenious author of this pamphlet has put forward much acute, but (as it appears to us) somewhat sophistical reasoning in defence of a position which will probably startle most of our readers,—that the insect tribes are not endowed with sensibility, still less with anything approaching to intelligence. Our own views on this subject have been partly expressed in a former article, and remain unchanged by the perusal of this essay: it contains, however, much clever argument and lively criticism upon the opinion of others, which will amuse, at least, if it does not convince. That a large proportion of the actions of the articulated tribes result from the stimulus of external *impressions* only, or, in the language of Dr. M. Hall, are *excito-motor*, we have no doubt; but this is only saying that they are purely *instinctive*, according to the definition we have given of the latter term. Dr. B.'s great error consists, as it appears to us, in maintaining that *sensations* can only act through the intellect and will: now, we have already endeavoured to show that this is by no means the case, and that there is a class of actions (of which the purely *emotional* movements may be taken as a specimen,) as necessarily con-

nected with particular sensations as others are upon impressions only. We see no reason to believe that the *mental* emotion, passion, or propensity always intervenes, in the lower animals, between the sensation and the action: and, in fact, we are disposed to regard it as rather designed to awaken the reasoning faculties, and to exist therefore in proportion to them. But we feel no doubt that insects possess the senses of sight, hearing, &c. in great perfection, and that these senses minister to their instinctive actions. Nor can we refuse to them some degree of intelligence, which, we think, is proved by the power of adaptation to novel circumstances evidently possessed by many, and still more by the educability which some manifest. We have had the opportunity of noticing the last in the case of bees, whose *memory* for the entrance to a hive beneath a window, to which there were many similar, evidently improved during the first few days of their being placed in their new situation. We have an additional argument for the existence of sensation in insects in the fact that impressions in the special organs of the Vertebrata do not appear to operate like those which act through the organs of common sensation.

ART. IV.—*On Warming and Ventilating; with Directions for making and using the Thermometer-Stove, or Self-regulating Fire, and other new Apparatus.* By NEILL ARNOTT, M.D., Physician Extraordinary to the Queen.—London, 1838. 8vo. pp. 138.

THIS is an admirable book; and, although not (strictly speaking) a medical one, it ought to be in the hands of every member of the profession. The warming and ventilating of chambers and houses are so intimately connected with the prevention and cure of diseases, that every physician and surgeon ought to understand them thoroughly; or, at least, the principles on which these most important measures rest: and we think it is not going too far to say that no book, before the publication of Dr. Arnott's, contained the exposition of these principles in such a clear and simple form as rendered the understanding of them easy to every one. But, besides the much clearer exposition of what was already known, Dr. Arnott's book contains a great deal of most important information which is entirely new. It is, indeed, like the "*Elements of Physics*," by the same author, literally full of the most interesting knowledge and the most ingenious contrivances, all planned and directly destined for the improvement of the arts of life, and the amelioration of man's condition as a social and civilized being.

The invention of greatest interest and value is the *thermometer-stove*, named in the title-page; an invention which appears to us calculated to produce a total change in the mode of warming houses, and with a saving of expense in the article of fuel, (to say nothing of its probable benefits in improving health,) which will cause it to be hereafter regarded as of national importance. We will not attempt here to describe the stove: we will only say that it may be manufactured, from Dr. Arnott's description, by any experienced white-smith, at a small expense, and that it has been proved to have the power of keeping up an incessant and unvarying degree of temperature in a room, at the cost in fuel of threepence, or, we believe we may say, three half-pence, in twenty-four hours!

A general expression of the qualities of the thermometer-stove is, that it possesses all the advantages of steam or hot-water warming, with many peculiar to itself. The following are the chief of these advantages:—1, Economy of fuel; 2, uniformity of temperature; 3, perpetual operation; 4, no smoke; 5, no dust; 6, no danger to persons or property; 7, obedience to command; 8, trifling original expense; 9, little trouble attending it; 10, moveable from room to room; 11, graceful form; 12, no sweeping boys, &c. Of all these advantages, No. 3—its perpetual operation, or being always alight,—is that which captivates us most; perhaps, because it comes most home to our business and bosom.

“Its importance is perceived by reflecting on the dependence of persons generally, in winter, on the regularity of servants or others, by being obliged, in the mornings, to wait in bed, until fires are lighted and rooms are put in order. The early riser, student, or man of business, with his ever-burning stove and alarum clock, is independent of the seasons. In England, many persons acquire the habit of lying abed in winter, as above explained, and then continue it in summer, and become eventually sluggards through life, from the accident of our imperfect domestic economy.” (P. 49.)

We cannot conclude without expressing our sense of the almost unexampled liberality of Dr. Arnott, in refusing to take out a patent for this admirable invention; a liberality which has only one parallel, which he himself afforded when he made the same sacrifice in the case of his other transcendent gift to afflicted humanity, the water-bed. The extent of the sacrifice in the present case appears to us literally immense; as we cannot but believe that the thermometer-stove will be used by thousands and tens of thousands, if, indeed, it do not eventually become an article of universal use. It is in instances like these that we are proud to see evidence that that combination of science, dignity, and humanity, which we would fain regard as inherent in the true medical character, still lives among us, to put to shame the ignorance, and quackery, and selfishness which we see too often striving to degrade it.

ART. V.—*A Treatise on Ruptures*. By W. LAWRENCE, F.R.S.; Surgeon Extraordinary to the Queen; Surgeon to St. Bartholomew's Hospital, &c. &c. *The Fifth Edition, revised, corrected, and considerably enlarged*.—London, 1838. 8vo. pp. 632.

THE work of Mr. Lawrence, of which the present is a much improved edition, has been so long and so favorably known to the profession, that it would be quite superfluous to give any detailed analysis of its contents, or even to speak very formally of its merits. We have always regarded it as a model of what a surgical treatise should be,—accurate and comprehensive in its facts; clear and simple in arrangement and in style; decided yet not dogmatical in its judgments and opinions on controverted points of practice. The treatise is now so much enlarged that it probably contains everything of importance which is to be found in other authors who have written on the subject; so that it is equally valuable as a work of reference in the study or as a manual in daily practice. Its very excellence renders it less fitting for the surgical student, whose attention should not be distracted by a multiplicity of statements, whether of facts or opinions; but to the young practitioner, whose experience yet needs

the support of authority in the difficult emergencies which every one has, sooner or later, to encounter, it must be invaluable. Indeed, we may safely say that no practical surgeon's library is complete which does not contain this classical and standard work.

It will be observed that the author of this work and the author of that noticed in the preceding article, are sharers in the honorary medical appointments recently made by the Queen. It is most gratifying to us, as public journalists, to observe that these honours seem, on the present occasion, to have been bestowed with more reference to personal merit, and with less regard to mere courtly favour, than during any former reign. With, perhaps, one or two exceptions, all the physicians and surgeons on the Queen's medical staff are the very men whom the general voice of the profession would have named as most deserving this distinction. With whomsoever the great merit rests of having advised these appointments, we hail the fact as a proof of the sway of better motives than have generally prevailed at courts. When the selection is made on such grounds, these courtly titles are really honours; and, if the same principle is allowed to operate, in place of being contemned, as they have often been by men of science, they will come to be coveted and valued as marks of professional merit and as objects of honorable ambition.

ART. VI.—*The Connexion of Natural and Divine Truth; or, the Study of the Inductive Philosophy considered as subservient to Theology.*

By the Rev. BADEN POWELL, M.A. F.R.S. F.G.S., Savilian Professor of Geometry in the University of Oxford.—London, 1838. 8vo. pp. 313.

WE notice this very interesting and excellent work in connexion with some of the topics discussed in our first article, which was in print before the publication of this volume. The views entertained by the author respecting the study of the value of *final causes* in physiology correspond precisely with those which we have advocated, (see p. 338,) although expressed in different language; our aim having been to demonstrate the injurious tendency of relying upon them in the prosecution of physiology as a science; whilst Professor Powell's object is to show their invalidity as a ground of theological argument, compared with that which is afforded by the study of general laws established independently of them.

"Let us guard," he says, "against the manifest error of taking for granted the thing to be proved. If in physiological, as in any other enquiry, we set out by *assuming* design, intention, or, in a word, a *moral cause*, then it is clear that any *inference* we may wish to draw in support of the truth of natural theology is vitiated and nullified. Considered, therefore, as taken in the correct order of reasoning, the caution expressed by Geoffroy St. Hilaire appears to me strictly just, and the censure which has been cast upon it undeserved. If he did *set out* by 'ascribing intention to the Deity,' he *could not* arrive at any *proof* of such intention. . . . To assign to animals 'a part to play,' before we have traced their analogies, is surely premature in the order of just reasoning, if by that expression anything more be meant than the mere guiding conjecture which the structure of their organs may suggest." (P. 131.)

After adverting to the inferences which may be deduced from the study of individual instances of design on the one hand, and from general laws on the other, Professor Powell continues:

"Either way, then, in the study of nature, there is an equally clear manifestation of that infinite intelligence, which, after the inductive examination of the laws or adjustments in either case, we are directly led to acknowledge as the irresistible conclusion. And, as this is the case with either species of investigation singly, so will it be more preeminently true when both are pursued jointly, as they assuredly may be, without the smallest detriment to each other or confusion of first principles, provided only we keep strictly to the simplest rule of all just reasoning, and do not confound our final conclusion with our first assumption. The highest philosophy is most disposed to cherish a readiness to perceive and admit the fair indications of design and intelligence, in whatever form they may present themselves; and there is a wider expansion given to our views when we thus include the contemplation of order and symmetry (even though we perceive not their end or object) in our notion of design. In such considerations we may find the loftiest exercise of truly philosophical reflections; we shall realize the highest aim of scientific speculation." (P. 136.)

In illustration of these views, Professor Powell adverts to the doctrines of Vegetable Morphology, to which we adverted for the same purpose; and thus closes his argument:

"The conclusion appears to me irresistible, that symmetry of arrangement is as decided a proof of design as adjustment of mechanism; beauty and harmony as clear indications of mind as combination of mechanical action." (P. 140.)

We cannot but rejoice that this subject has been placed in so correct a position by one whose profound acquaintance with the physical sciences, logical correctness of argument, and fearless advocacy of what he deems the truth, entitle his opinions to the calm and impartial consideration of those whose antiquated prejudices they oppose.

ART. VII.—*Anatomical, Pathological, and Therapeutic Researches upon the Disease known under the Name of Gastro-Enterite; Putrid, Adynamic, Ataxic, or Typhoid Fever, &c.; compared with the most common Acute Disease.* By P. C. A. LOUIS, M.D. &c. *Translated from the French, by H. J. BOWDITCH, M.D.—Boston, 1836. Two Vols. 8vo. pp. 395, 462.*

THESE are two remarkably handsome volumes printed at Boston, and, if they meet with a satisfactory sale in America, it will shew that there is much zeal for pathological information amongst our transatlantic brethren. The original work of M. Louis is an application of his numerical method on a large scale, and alone is sufficient to hand his name to posterity as a most careful, indefatigable, and judicious observer of facts; but it is the reverse of popular: it requires diligent study, and will remain a standard book of reference. It differs from the generality of French medical works in being dry and unattractive; for, in general, our neighbours manage to impart some of their liveliness to their serious labours: it also differs from too many in one object which M. Louis tells us he had in view, "not to write one single useless phrase." It is full of minutely reported cases and rigorous deductions. M. Louis says that, between 1822 and 1827, he collected 138 cases of typhus fever, fifty of which were fatal. He analyzed all; and, in order to ascertain which of the lesions found in the patients who died were peculiar to typhus, he compared them with the alterations found as consequences of other acute diseases in eighty-three subjects, whose histories he learned. He did the

same with the symptoms of those affected with typhus fever, or with any other acute affection, which terminated either favorably or fatally; so that he has analyzed the diseased changes of the viscera of 133 subjects, and the symptoms of nearly 900. When it is recollected that this labour was undertaken for the investigation of fever only, that M. Louis has taken equal pains with phthisis, and still continues his exertions as a clinical teacher, it will excite no surprise to those who are not acquainted with M. Louis to learn that, in accurate diagnosis, he has no rival. The motto M. Louis prefixes to his work, from Rousseau's "*Emile*," is given, we presume, as an authority for the numerical method; but it contains, we think, the fallacy on which the numerical method, when carried out to treatment, exists. "*Je sais que la vérité est dans les choses, et non dans mon esprit qui les juge, et que moins je mets du mien dans les jugemens que j'en porte, plus je suis sûr d'approcher de la vérité.*" (*Emile*.) It may be literally true that the truth resides in the facts themselves, and not in the mind of the persons judging them; but, when Rousseau adds, that the less he introduces what is merely his own into the judgments he pronounces, the more he approaches the truth, he altogether overlooks the fact that it is by his mind that he judges, and that the rectitude of the decision greatly depends on his own mental acuteness; for the correctness of any deduction from probable evidence, which requires what we call judgment, depends materially on the correct powers of judging which the individual possesses who investigates. For instance, Harvey stated that he discovered the circulation of the blood by reflecting on the use of the valves: now, the existence of the valves had been known long before his time, but it wanted the reflective powers of a Harvey to discover the great truth which these valves suggested. Probably Rousseau himself merely intended to say that, in judging correctly, we should free ourselves from prejudices.

The translation, wherever we have made the comparison, is faithful, but it is too literal: it reads like a translation, which no translation ought to do. Dr. Bowditch's dedication to the memory of his friend, James Jackson, jun., and his enthusiasm for his teacher, give us a very favorable impression of himself; and we trust that he may be able, at some future time, to complete the design he announces of giving some account of typhus fever as it appears in Boston, which he thinks is identical with the typhoid fever of Paris: with great consistency, however, he does not fix his opinion in this point until he can prove it numerically.

ART. VIII.—*De Physiologia Tenotomiæ Experimentis illustrata. Commentatio Chirurgica, qua Ordini Medicorum gratioso Acad. Georg. Aug. &c. &c. gratulatur* FRED. AUG. AB AMMON, M.D., Reg. Saxon. Archiater, &c. &c.—*Dresdæ*, 1837. 4to. pp. 24.

On the Physiology of the Division of Tendons in Surgical Operations, illustrated by Experiments. By F. A. VON AMMON, M.D., Chief Physician to the King of Saxony.—*Dresden*, 1837. 4to. pp. 24; with a Plate.

THIS short treatise is a gratulatory address presented to the university of Göttingen, the 17th September, 1837, on the centenary of its insti-

tution. The distinguished author justly considers that the investigation of this subject is one of much interest and importance at the present time, more especially as the particular mode of practice referred to must still be regarded as in its infancy, and has hitherto been very imperfectly examined in a physiological point of view. His claim to the right of discussing it is founded on his having repeatedly performed the operation on man, and, in an experimental way, on animals.

After some introductory matter respecting the nomenclature and synonymy, &c., the author proceeds to give the history of the operation. Like many other medicinal and chirurgical modes of treatment, the operation for the division of tendons, or, as the author, following some French writers, terms it, *Tenotomia*, or *Tenotomy*, (from the Greek words *τενω* and *τομη*), has, since its first introduction, undergone many vicissitudes of fame and favour; falling for a time into discredit and disuse, and then being restored with all the pomp of a new discovery. According to Von Ammon, the operation appears to have been first performed (on the tendon of the mastoid muscle) by Roger Roonhuysen, in the latter part of the seventeenth century. He was followed by Meekren, Tulp, Blasius, Ten Haaf, and, much more recently, by Thilenius, Michaelis, and Sartorius: the two latter of whom divided the tendo Achillis for the cure of clubfoot. Some fifteen years afterwards, Delpech taught and practised the operation; and subsequently Dupuy adopted it, but professedly as the imitator of the Dutch surgeons, not of his own countrymen. Lafosse, in France, introduced the operation into veterinary practice about the end of the last century; and it seems to have been subsequently followed in that country, with good success, in the cure of contractions of the tendons in the feet of horses. Ten years after Delpech, Stromeyer, of Hanover, once more set about curing clubfoot by the division of the tendo Achillis, and undertook a set of experiments on the result of similar operations on animals, in conjunction with Dr. Gunther, veterinary professor in the same city. In consequence of this new impulse given to the practice, it was speedily followed by Dieffenbach, Holscher, Elster, Bünger, Leonhard, Zeis, Ulrich, Von Ammon, &c. in Germany; by Bouvier, Roux, Cazenave, &c. in France; and, we may add, by Dr. Little, Mr. Whipple, and many other surgeons, in England. The general result of these operations has been most satisfactory; and the operation may now be considered as too well established to be again allowed to fall into disuse.

The author attributes the slow and uncertain progress of the operation of *tenotomy* to various circumstances; to the general opinion of the difficulty with which tendons were healed when ruptured; the apprehension of bad consequences, such as tetanus and spasm from incised wounds of them; prejudices probably derived from the confounding by the ancients, both in name and in fact, of the nerves with the tendons. It was only by the results of actual practice, and by the experimental investigation of the whole subject, that these false notions and fears could be dissipated.

The experiments made of late years by Duval have thrown much light on the natural processes in cases of this kind; and it is but justice to this author to state that the conclusions published by him coincide perfectly with those deduced by Professor von Ammon from his experiments.

These, in which he was assisted by Dr. Prinz, the veterinary professor, and by Dr. Zeis, of Dresden, were performed on six horses and three rabbits. In the horses, the tendon of the flexor profundus of one of the fore feet, and in the rabbits the tendo Achillis, was divided: the effects resulting from the wounds being carefully observed, the animals were killed at different periods after the operation, and the state of the wounded parts closely examined. The local reaction and inflammatory fever varied much in the different cases. The following are the general results observed on dissection:—1. Twenty-four hours after the operation, the whole wound was found filled with coagulated blood, or there was a coagulum on the extremities of the divided tendon, without any traces of plastic or coagulable lymph, or of new vessels. 2. In one instance, in the horse, some traces of plastic lymph were found on the second day. 3. After the fourth day, the lymph presented small, conical elevations on the cut ends of the tendon, (which were still surrounded with coagulated blood,) so as partially to lessen the distance between the divided parts. 4. After the seventh day, these elevations assumed the appearance of threadlike elongations uniting the divided tendon, and the coagulum appeared intermixed with plastic lymph. 5. After a month the wound was perfectly cicatrized, and the new production resembled tendon in its structure, although still distinguishable from the original structure at the point of union: there was no inflammation in the neighbouring parts. 6. After the thirty-sixth day (the longest period of observation), the new substance, in the horse's foot, had more resemblance to cellular tissue, but was firm, of a blueish-red colour; the tendon was somewhat elongated. All the foregoing results were obtained on the horse. In two of the rabbits, examined after six days, the results are not clearly detailed; in the third, the tendon, after the twelfth day, was found united by a slender, hard, and very compact substance, almost like the original tissue, and adherent to the neighbouring parts. The author regards the new-formed substance not as actual tendon, being distinguished from it by its inferior smoothness and lustre and greater density, as well as by its colour, which at first is reddish, and subsequently blueish; but considers it as having precisely the same function as the primary tendinous tissue. He believes that the new substance requires about fourteen days for its formation. It is important to add, that Dr. Hartwig, who gives a brief analysis of Professor von Ammon's dissertation in the *Medicinische Zeitung* for January 17th, informs us that he has performed the same experiment, on the ligamentum nuchæ, and flexor tendon of the foot in upwards of twenty horses, and, for the most part, with very similar results. We regard the profession as much indebted to Professor von Ammon for this short treatise. Although it contains nothing very new, it places the facts of the case in a clear light, and gives a more connected view of the history and whole bearings of the operation than we before possessed.

PART THIRD.

Selections from the Foreign Journals.

ANATOMY AND PHYSIOLOGY.

On the Structure of the Iris in Birds, and the mechanism by which it is moved.

By Dr. AUGUST KROHN, of Petersburg.

DR. KROHN considers the iris of birds as composed of four principal coats; 1st the anterior layer of pigment; 2d, the fibrous layer; 3d, a delicate membrane serving as support to the fibrous coat; and 4th, the posterior layer of pigment or the uvea.

1. On the anterior pigment depends the great variety of shades of colour of the iris. Of the light tints the white and yellow occur most frequently, but the dark also occur in every shade from light brown to the deepest black. In a few cases, as in the white goose, the anterior pigment is wanting, and the iris then assumes a blue colour caused by the dark uvea being seen through the colourless iris. The yellow colour of the iris in owls and in the common fowl is owing to the presence of a yellowish oil which is contained in small vesicles, and is not, at least in some owls, at all dependent on the molecules of the pigment. In other cases it appears as if the yellow colour were owing to a combination of the oil with a yellowish pigment. Dr. Krohn is doubtful whether an extremely fine membrane coats the anterior pigment, to preserve it from the action of the aqueous humour.

2. The following results, regarding the nature of the fibrous coat, were obtained from an examination of the iris in—(*Birds of Prey:*) *Strix Bubo*, *Str. nisoria*, *Falco ossifragus*; (*Climbing Birds:*) *Psittacus erithacus*; (*Singing Birds:*) *Panus ater*, *Fringilla domestica*, *Fr. spinus*, *Fr. enucleator*, *Loxia pyrrhula*; (*Gallinaceous Birds:*) *Phasianus Gallus*, *Tetrao Bonasia*, *Meleagrus Gallopavo*, *Coturnia dactylisonus*, *Columba oenas*; (*Swimming Birds:*) *Anas anser*, *Anas Boschas* (domestic).

The whole surface of the iris, from the ciliary border to the pupil, is covered with a large number of fibres running concentrically with the pupil, but requiring the microscope to be accurately distinguished. They are particularly distinct towards the ciliary border, and then appear to be composed of several layers heaped on each other; some of them deviate in a slight degree from the true circular course, and by crossing with others produce in some places an interweaving of fibres. Towards the pupil the course of the fibres is more parallel, and their thickness diminishes progressively as they approach that aperture. Below the microscope these fibres present all the characters of muscular fibres; they appear as transversely marked cylinders, are easily torn, both in a longitudinal and transverse direction, and their transverse lines become more distinct by maceration in alcohol, and thus the fibre in some measure acquires a resemblance to the trachea of insects. It is impossible to determine, owing to the small field of vision of the microscope, whether the single fibres form entire circles, but at any rate it is certain that they belong to the smallest fibres of the muscular tissue; in diameter the largest of them correspond most nearly with the fibres of the heart, but their breadth seems to vary with the class and species of the bird. It is remarkable that in owls the iris is not, as in other birds, in immediate contact with the ciliary processes, but is connected to them solely by the vessels which stretch through the aqueous humour from the choroid to the iris. In order to examine the muscular fibres both layers of pigment must be removed by means of a hair pencil and frequent agitation of the iris in water; a

small piece should then be excised, and subdivided beneath the field of the microscope by small needles.

3dly and 4thly. The thin delicate membranes posterior to the muscular layer, and the posterior layer of pigment present nothing remarkable. The latter, like the anterior layer is probably covered by a thin delicate coat, to preserve it from being washed off by the aqueous humour.

Dr. Krohn has likewise devoted two or three pages to a description of the blood-vessels and nerves of the iris, with reference particularly to some peculiarities in their distribution in the owls; but as his remarks are scarcely susceptible of abridgment we are forced to pass them over.

The reader will scarcely doubt, says Dr. Krohn, that the movements of the iris and its frequent oscillation depend upon the expansion and contraction of the circular fibres of the muscular layer; they contract to diminish the aperture of the iris, and the dilatation of the pupil is owing simply to their relaxation. In this respect the iris corresponds with the other sphincter muscles, but there is this difference, that the contraction or dilatation of the pupil varies rapidly owing to the frequent change of the force, and the peculiar nature of the stimulus. A circular muscle, a sphincter pupillæ, is therefore sufficient to account for all the changes in diameter in the pupil of living animals; and with this view the experiments of Mayo seem to correspond. According to this physiologist, the division of the N. oculomotorius within the skull of living pigeons was followed by immovability and dilatation of the pupil, or in other words by paralysis of the sphincter pupillæ; whilst mere irritation of the nerve produced a contracted pupil, in the same way as mechanical irritation of a nerve supplying any muscle is followed by contraction of that muscle.

Dr. Krohn next proceeds to speak of the circle of muscular fibres, first described by Crampton, which lie on the inner surface of the osseous plates. These fibres arise from the osseous circle, and are inserted, according to Crampton, into the inner surface of the cornea; according to Mannoïr into the ciliary border of the iris; and according to Dr. Krohn, into the anterior part of the upper wall of the canal of Fontana. Examined with the microscope they present the same appearances as the fibres of the iris; and are identical in structure. According to Crampton they serve to flatten the cornea and to adjust the eye for seeing distinctly at great distances; but Dr. Krohn does not acquiesce in this use of the muscle, although he confesses he cannot offer any probable conjecture as to its action. According to Mannoïr's view it would act as a dilator of the pupil; but it is very improbable that the fibres are inserted into the iris.

The preceding observations refer only to the eyes of birds; the examination of the eyes of the mammalia requires greater care and is more difficult: Dr. Krohn has, however, found in the human iris a layer of fibres running concentric with the pupil, but he is not yet prepared to speak decidedly as to their nature.

Müller's Archiv. Jahrgang, 1837. Heft iii.-iv.

On the Nature of the Degeneration of the Kidney in Bright's Disease.

By Dr. GLUGE.

In this disease the cortical substance of the kidney is paler, and the tubular substance redder than in the healthy state, and the former acquires a greater development at the expense of the latter. The cortical substance has lost its smoothness and has become granular, and this granular appearance is also seen on the surface of the kidney after removal of the tunica propria. Is this granular appearance owing to the deposition of a foreign substance? In order to ascertain this point a drop of fluid from the cortical substance was examined with the microscope and found to contain a large quantity of albumen, a large number of peculiar globules, detected by Dr. Gluge in inflammatory secretions and termed by him inflammatory globules, and a few globules of pus. Fluid pressed from the tubular substance likewise contained inflammatory globules, but in infinitely smaller quantity. On examining a thin segment of the cortical substance with the microscope these globules were found deposited in immense numbers between the urinary

ducts; they were likewise found in the medullary substance but in much smaller quantity. Dr. Gluge therefore concludes that the degeneration of the kidney in Bright's disease consists in inflammation of the cortical substance of the kidney, which extends, though in a comparatively slight degree, to the tubular substance.

Wochenschrift für die gesammte Heilkunde. Jahrgang, 1837. No. 39.

On the Existence of the Polystoma Sanguicola in the Human Blood.

By Dr. DELLE CHIAJE.

CASE I. Was of a man, æt. twenty-five, subject for some time to hæmoptysis. His temperament was sanguine, his parents were robust, but he had been suckled by a nurse affected with syphilis. He led a dissipated life, had had syphilis and itch. The hæmoptysis occurred first in an alarming manner, after he had taken a bath in the sea, during the heat of summer. Expectoration continued notwithstanding the means which were employed. On one occasion the patient was seized with an attack of hæmoptysis in the presence of his physician, Dr. Gallo. On examining the blood half an hour afterwards, Dr. Gallo found some small flattened worms, resembling little leeches, which were attached to the sides of the basin. The parents assured him that they had observed similar animals every time that an attack of hæmoptysis had occurred. The patient became phthisical, and Dr. Gallo had no further opportunity of observing the matters expectorated.

CASE II. M. Tolinea observed a similar case of hæmoptysis, in an individual who had been previously affected with syphilis and scabies. Three of the *Polystoma sanguicola* were found in the blood expectorated. From this period, the patient was given into the charge of Dr. Delle Chiaje, but here also the disease rapidly passed into phthisis, and the author was disappointed in his endeavours to preserve alive these remarkable entozoa.

The author regards the two preceding cases together with others of a more ancient date, (e. g. that of Borelli in his *Obs. Cent. iii. Obs. iv.*,) as irrefragable proofs of the existence of the polystoma, although it has been denied by the majority of naturalists. Dr. Delle Chiaje endeavours to dissipate all doubt on the subject by supposing that the polystoma does not exist in the blood itself; according to his view, it is lodged in the pulmonary parenchyma in the same manner as the diastoma hepaticum occupies the parenchyma of the liver, the tetrastoma, the papillæ of the kidneys, and the polystoma pinguicola, the ovaries. It is not until after they have perforated the walls of vessels that these animals enter the circulation.

The author thus describes the polystoma. It resembles at first sight, a drop of coagulated blood, flattened like the pips of a gourd, but of a deeper red colour than that of the blood which surrounds it. When touched, this seeming drop of blood moves distinctly. The animal has two extremities, the one pointed, the other rounded; in its contracted state its length is three lines, and its breadth two lines. When it stretches itself, its length is ten lines and its breadth three lines. Its structure, which is almost annular, may then be distinguished. It executes its movements by a double extension, in the same manner as leeches. The animal reacts against the least impression, and passes immediately from rest to a motion more and more quickened, according as it is found swimming in a fluid or attached to the sides of a vessel.

Osservatore Medico di Napoli.

On the existence of the Ovum at a very early period in the Female Economy, and on the consequent necessity of admitting a new period in the History of Human Development. By Dr. C. G. CARUS, of Dresden.

By the labours of preceding physiologists the axiom had been established, "that man and the mammalia derive their origin from an ovum existing in the ovary previous to impregnation," but there were wanting data to establish at what period of life these ova first become developed in the ovaries of man and of the mammalia. The investigation of this point forms the subject of the paper before us.

Dr. Carus with a little practice was soon able to pick out the Graafian vesicles from the ovaries of very young calves, and to submit them to examination beneath the field of the microscope. The extracted vesicle was carefully laid open with a cataract needle, and a little attention then sufficed to distinguish the ovum, surrounded by the discus proligerus, swimming about in the fluid which escaped. The chorion, the yolk, the germinal vesicle with the cicatricula were all remarkably distinct.

The opportunities of examining the ovaries of female infants were more difficult to be obtained; but in the spring of 1837, the opportunity of examining the ovary of an infant four days old presented itself. In this case it was impossible to detect a Graafian vesicle already filled with fluid and containing an ovum, but on submitting thin segments of the ovary, subjected to moderate pressure according to the method of Purkinje and Valentin, to examination, there was no difficulty in perceiving ova of various sizes, characterized by the yolk and germinal vesicle, but closely enclosed by the Graafian vesicle and substance of the ovary.

The examination of the ovary of an infant, one and a half years of age, showed considerable differences. In this case a number of the Graafian vesicles had expanded to the breadth of a quarter or even half a line. The child had been affected with rhiachitis, and the circulation had in consequence been obstructed, and blood even been effused into some of the vesicles. In others the ova were found in a semi-dissolved state; but in one of the largest the ovum was found quite entire and distinct, and in another there was found an imperfectly developed ovum, in which the clear circle of albumen, between the membrane of the yolk and the chorion, and part of the yolk were discernible. Dr. Carus here calls attention to the pathological conditions of the ova, as above alluded to, as throwing light upon the origin and cause of dropsy and degeneration of the ovaries, and as affording an explanation of one of the causes of sterility.

In examining the ovaries of a girl, æt. four years and a half, who had died of pneumonia, both ovaries were found to contain one perfectly developed Graafian vesicle six-eighths of a line in diameter, each of which contained an ovum of the one-twelfth of a Vienna line in diameter, with yolk, germinal vesicle, and cicatricula all perfectly distinct, swimming in a clear albuminous fluid. Besides these large vesicles both ovaries contained a number of smaller ova from one-sixtieth to one-fourteenth of a Vienna line in diameter.

From these observations Dr. Carus concludes: 1stly, that the ova, the germs of future individuals, are already formed before the birth of the female, so that in the last period of pregnancy three generations exist in the body of one individual; 2dly, that shortly after the birth of the female, at least before the end of the first year, several of the Graafian vesicles become developed to such a degree around the ova, as to present an appearance not materially different from their condition at the period of puberty, and consequently, it is rendered probable that the only obstacle to impregnation at an early age lies in the structure of the external parts. 3dly, that the matured human ovum, when it has in some measure been isolated from the maternal parts by the development of the Graafian vesicle and its fluid contents, may remain in a state of latent life for an indefinite number of years till roused into activity by the stimulus of impregnation. 4thly, that a new period, that of the latent life of the ovum, must henceforward be adopted in the history of the development of man.

Dr. Carus concludes his paper with some comparative remarks on the latent life of the human ovum, and the ova of plants and insects. It is well known that the seeds of plants may retain their latent life for thousands of years, but this quality is essentially different from the latent life of the human ovum, as in the latter there has been no fructification, and because the ova of plants before fructification are incapable of latent life. The eggs of the articulated animals are also incapable of remaining for any length of time in a state of latent life, although after fructification has taken place, but before there is any appearance of the embryo, they may remain, often for a long period, in a latent state. The same kind of latent life is apparent also in the impregnated eggs of the higher amphibia and of birds, when

the egg has reached a certain stage of its development. The long latent life of the human ovum Dr. Carus considers as one of the consequences of the high destiny of man, as affording time for important but imperceptible changes in the structure of the germ.

Müller's Archiv. Jahrgang, 1837. Heft iv.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Notice of the Influenza which prevailed in Lisbon during the month of February, 1837. By Dr. LIMA LEITAO.

THE labour of comparing the character and progress of epidemics in climates very different from our own with what we observe of their essential nature and mode of transmission at home is one of considerable interest, and may, we consider, prove instrumental in illuminating this one of many obscure departments of our art. Dr. Leitao's memoir bears on the face of it the impress of faithfulness and candour, and is a very detailed and ample account of the disease; but in our eyes its value consists chiefly in the differences we discover not only in the character of the epidemic in Portugal and in Britain but in the period of its appearance, the weather with which it was associated, and certain circumstances connected with its diffusion in the respective countries. The influenza was milder in its character in Portugal than here, evinced by the fact of its not being fatal in the former situation. Besides this difference, we find, according to our author, that gastric symptoms, when they occurred in Portugal, were the results of treatment, emetics, strong purgatives, or stimulating diaphoretics, having been administered, and having given origin to them, whereas we can assert from our own abundant observation that in this country, gastric symptoms, liable certainly to be aggravated by improper treatment, constituted an important portion of the majority of cases observed in the epidemic—in many instances, indeed, a more important portion than the affection of the mucous membrane of the air-passages.

The winter in Lisbon was not unusually severe, but the early days of January were very cold, so much so that on the 2d of that month snow fell so abundantly as to give the Portuguese capital the appearance of a city in the north of Europe, and astonished the inhabitants very much, nothing similar being remembered. But from the 10th of February to the end of the same month, the period during which the epidemic appeared, the sun was very warm though the wind was high and cold. The disease, it will be observed, manifested itself fully six weeks later than in this country. We at least saw cases towards the close of December, 1836, whilst the first examples that fell under the observation of Dr. Leitao were on the 20th of February. On the first appearance of the epidemic in England the weather was so severe, as to be very generally supposed the cause of the disease, but in Portugal the severe weather engendered no epidemic, but the influenza there, like our own of 1833, appeared and spread under a warm sun. The author informs us that it was rumoured that some English ships, recently arrived from England and lying in the Tagus in the beginning of February, had influenza on board, and to this circumstance, he is evidently disposed to ascribe its diffusion through the city. It originated, he says, under the same circumstances at Bilbao in the north of Spain, there being English ships there. He regards the cause of the disease atmospheric, and thinks that fractional portions of the morbid atmosphere may be conveyed in ships and become the cause of contamination to an atmosphere hitherto pure. We remember that the late Sir David Barry propounded a similar theory clothed in very mystic terms respecting the transmission of cholera. It appears to us at most only a circumlocutory expression for a mode of contagion very well exemplified in the case of puerperal fever, where a medical man conveys from bedside to bedside a disease from which he is himself exempt. Dr. Leitao thinks that influenza in its diffusion through Portugal manifested a preference for the courses of rivers; and states that ascending the Tagus *especially by the right bank*, it reached Abrantes and Villa Velha. This preference for the right bank of this

great inlet to the interior of central Portugal must certainly be referrible to some other principle than a tendency to move in the course of a river, and, as we happen to know that the halting places of the barges navigating the Tagus are chiefly on its right bank, supposing the fact to be correctly stated by the author, we cannot explain it otherwise than by considering human intercourse to have been the means of diffusing the disease. This statement of Dr. Leिताo bears more the aspect of contagion than anything that fell under our observation in this country.

Jornal da Sociedade das Sciencias Med. de Lisboa. Jan. 1837.

The Influenza in Copenhagen in the winter of 1836-7. By Dr. OTTO, Professor in the University of Copenhagen.

THE first cases took place in December, 1836, simultaneously with the occurrence of the epidemic at St. Petersburg, Stockholm, Berlin, &c., but a year later than in most other parts of Europe. Former visitations of the disease had taken place in spring. The influenza was ushered in, and accompanied in its progress, by the same thick foggy weather which had introduced the measles the year before; it spread with astonishing rapidity; family after family was attacked in rapid succession, and when the epidemic was at its height, in the middle of January, upwards of 30,000 persons were ill at one time. Towards the end of January the disease began to decline, but continued through the month of February, and even at the beginning of March some slight cases occurred. The symptoms were those of catarrh with or without fever, but invariably accompanied by extreme depression of strength, the marked attendant of every former epidemic; a dry cough, with or without pain in the chest, hoarseness, great thirst, loss of appetite, furred tongue, nausea, bitter taste in the mouth, and headach were present in a greater or less degree. When the patients attempted to follow their usual avocations the symptoms were aggravated, and the disease often lasted as long as a fortnight, whilst rest and careful management removed every symptom but weakness in four or five days. Perfect health and strength, however, were rarely restored under five or six weeks. Exposure to cold soon brought back the disease in an aggravated form. The prognosis was nearly always favorable, for the epidemic was distinguished from those of London, Glasgow, &c., by being in itself entirely free from danger. Persons affected with cough or diseases of the lungs, and old men suffered most, and children least. Dr. Otto concludes his memoir by stating his firm conviction that the influenza at Copenhagen was contagious, and that when one member of a family was attacked the others were sure to suffer in succession.

Zeitschrift für die gesammte Medicin. B. v., Heft ii. Hamburg, June 1837.

Method of treating Intermittent Fevers in the Infirmary of Clinical Medicine of the Surgical School of Lisbon. By LIMA LEITAO, M.D., Professor.

[AGUE being of very frequent occurrence among the labourers in the flooded or marshy grounds bordering on the Tagus, Dr. Leिताo has written a sensible article explanatory of the varieties observed in the disease, and of the treatment applicable to each variety. It appearing to us to be a very judicious account of the abdominal complications of recent intermittents, and to be faithfully deduced from ample observation, we transfer it in an abridged form to our pages. Such of our readers as are not, like the author, disciples of Broussais, will probably regard the local affections of the abdominal organs as concomitants not as causes. We shall, however, retain the original divisions and designations.]

The following divisions comprise the varieties observed: 1st, intermittents proceeding from gastro-duodenal phlogosis; 2d, those arising from inflammation of the liver, of the spleen, or both conjointly; 3d, such as proceed from phlogosis embracing simultaneously, wholly or in part, the gastro-duodenal mucous lining, the liver, and the spleen; 4th, intermittents not arising from inflammation.

1. The symptoms of the cases referred to the first division are thus described. If during the intermission, the following phenomena are observed—redness of the

margin of the tongue, with a white or slightly yellow coating on its surface; more or less thirst; an obscure feeling of pain or weight in the epigastrium, even when pressure is not applied; a sense of heat in the urethra and rectum in passing urine and feces; nausea, or vomiting of mucous or bilious matter; a pulse without being decidedly febrile, yet not that of health; then, according to the author, there exists inflammation of the mucous membrane of the stomach, duodenum, or both. This form of the disease Dr. L. has observed exclusively in persons of the sanguineous, or bilio-sanguineous temperament; of youthful and adult age; of a constitution not yet broken-down, and in first attacks of ague. He does not remember to have observed it in the quartans of Portugal; but only in quotidian, and double and single tertians. For its cure he recommends, repose in bed; a diet of light broth; beverage lightly acid and edulcorated (*agridoce*) or mucilaginous, according to the taste of the patient, taken tepid; and emollient *enemata*. After the second paroxysm, or after twenty-four hours' repose in the hospital, whatsoever number of paroxysms besides the second may have occurred prior to the patient's admission there, leeches are applied to the epigastrium, followed by poultices. The number of leeches is proportioned to the age of the patient, twenty-four being the mean number. Dr. L. thinks the paroxysm the most suitable period for their application. Should the symptoms above described have disappeared in the apyrexia next ensuing, but should the paroxysm follow with the same or nearly the same intensity, a grain of the sulphate of quinine is given every three hours, every two hours or every hour (according to the type of the disease) during the intermission. After each dose of the medicine Dr. L. gives some mild mucilaginous or sugared beverage, and very little other sustenance is taken. It is important that the apyrexia be perfect, and that the indications of local affection have ceased before administering the sulphate of quinine, otherwise there is risk of converting the disease into the remittent or continued form.*

Should the inflammation resist the first application of leeches, they must be repeated a second or even a third time, till it is removed. If the paroxysm does not recur after the application of the leeches, or if it be much diminished in intensity and later in coming on, no sulphate of quinine is administered; and, in the latter case, it is observed, that after one or two fits more and more slight, the disease ceases. The author thinks that convalescence is more speedy and relapse less liable to take place, in these, as it were, spontaneous recoveries, than where the sulphate is employed.

2. In the second division, comprising cases in which the liver, the spleen, or both conjointly are affected, the author recognizes the phlogosis of the liver (should there not be enlargement) by obtuse pain, heat, and tension, increased by pressure; yellow tinge of the face and eye; yellowish furred tongue; bilious vomiting and dejections, &c. When the liver is enlarged, the local symptoms are referred to the left lobe. If the spleen suffers, it presents analogous local symptoms: both organs are often simultaneously affected. The remedies of this form of the disease are the same as those of the preceding, with this difference that general bloodletting is found more serviceable than leeches, or, at least, should precede their employment. Two bleedings of eight ounces each are generally sufficient. Dr. L. has seen intermittents of this division and of all types, yield to depletion alone by general followed by local bloodletting; and this successful result from depletion solely has been more manifest in the diseases of this than the preceding division.

3. The third division, comprising intermittents connected with inflammation of the gastro-duodenal lining, and of the liver and spleen conjointly, is marked by a combination of the symptoms of each of the preceding diseases. The malignant intermittents, observed by the author in eastern Africa, belonged to this class. The treatment consists of the methods employed for the other two divisions combined, that is, bleeding, general and local, excepting in nervous temperaments, when he has recourse only to the latter. He thinks this kind of case very suitable for the endermic method of employing sulphate of quinine. In the malignant

* This admonition we think of great importance, having seen very serious evil result from the neglect of such caution.—REV.

intermittents of eastern Africa, he derived much advantage from frictions of tincture of bark, and from sprinkling blistered surfaces with powdered bark and camphor.

4. The fourth division, consisting of cases unattended with local inflammation, he treats as he does those of the preceding, except that bleeding is omitted.

The author subsequently gives a practical commentary on the 59th aphorism of the 4th section of Hippocrates, "tertiana exacta in septem circuitibus ad summum judicatur." Having tried its truth, he found the patient, solely from the influence of low diet and repose, escape the seventh paroxysm in some cases and the eighth in others. The examples in which this fortunate result took place belonged principally to his fourth division; but a proportion of them to his first, or that comprising the complications with gastro-duodenal inflammation. These spontaneous recoveries, wheresoever they occur, Dr. Leitaõ regards as the most favorable, the general health being the least disturbed, convalescence most prompt, and relapse very rare.

[We recommend these "spontaneous cures" of ague, to the attention of Mr. Parkin, (See British and Foreign Review, No. V. p. 199,) as throwing some light on the result of his treatment of the same disease in Spain, by means of saline draughts.]

*Jornal da Sociedade das Sciencias Medicas de Lisboa.
Mez de Fevereiro, e de Abril, 1836.*

Observations on the Plague. By Dr. BULARD.

DR. BULARD has enjoyed opportunities of studying the fatal malady in several of the cities of Egypt, of Asia Minor, and Turkey; and the following remarks are abridged from some abstracts of his views communicated to the *Medicinische Zeitung*, by Dr. Vetter. Dr. Bulard is now in Egypt.

After mature consideration, Dr. Bulard adopts the hypothesis that the plague depends upon alteration of the fluids. When a vein is opened, the blood escapes freely, but it is thicker, and its colour is darker, than in the healthy condition. After standing for some time, it acquires a violet hue, and forms an imperfect coagulum, covered with a deep reddish coloured serum. Frequently, however, no coagulum is formed: the blood in that case becomes of a livid hue, has a very perceptible odour, and some oily drops may be observed upon its surface. After death, the blood in the arteries and veins is found equally dark-coloured; and in some of the large trunks the oily drops are also occasionally discovered. These appearances, which the blood presents after death, are not the result of post-mortem changes; for the period which elapsed between death and the examination of the body was insufficient to produce this by decomposition. The first link in the chain of disease is in the lymphatic system: a diseased lymph is poured into the blood, and gives rise to all the fearful phenomena of plague, by acting as a poison in a manner analogous to the action of pus in phlebitis, according to theory of Dance. The alterations in the tissues of the body are such as might be expected from alteration of the fluids. The spleen, in ninety-five cases of a hundred, is more or less diseased; the stomach, in about twenty cases of a hundred, presents diseased appearances; but these do not appear to stand in any necessary relation to plague. The liver is occasionally slightly altered in colour and density; the lungs and bronchial mucous membrane are sometimes inflamed, but are as often healthy; and the only constant, and apparently the most material, lesion which occurs is venous congestion of all the tissues of the body,—of the brain, lungs, liver, spleen, heart, kidneys, &c.

The disease may be divided into two periods: the symptoms of the first are an increased development, in a greater or less degree, of the lymphatic glands, with softening or putrid degeneration of their substance, and venous congestion of the great vessels. The second period is characterized by a putrid state of the lymphatic glands, with extravasation of blood; general softening of the tissues; a decided state of general congestion; external and internal petechiæ; ulceration of the gastric mucous membrane; ecchymoses, and carbuncles.

Resting upon these views, various methods of treatment were assayed; but the result was seldom satisfactory. At first, patients who presented the characteristic symptoms of the first period, (in general, from about six to twelve hours after the commencement of the disease,) were treated with stimulant and volatile remedies; such as muriate and acetate of ammonia, volatile ammonia, iodine, chlorine, nitric acid, and the preparations of alcohol and æther; but these remedies produced no perceptible effect upon the nervous system, although they sometimes recalled the secretions of the skin and kidneys, and produced a momentary increase of muscular strength. The patients thus treated generally died in from twenty-four to forty-eight hours. Cases of the same description were next treated by emetics: these produced vomiting and perspiration, but had no beneficial effect upon the course of the disease. Narcotics, although in opposition to theory, were next tried with a like result. Sulphate of quinine, calomel, pills of mercurial ointment, combined with external mercurial frictions, had no effect in altering the character of the disease.

The best mode of practice seemed to be that which was limited to assisting nature, and such a method was therefore ultimately adopted. The feeling of cold was combated by additional blankets and the exhibition of warm drinks; the buboes were brought to a head by poultices, and laid open as soon as fluctuation was distinct; carbuncles, when present, were punctured, or, when they did not appear within a certain time, were artificially produced; and, in short, the footsteps of nature were followed as closely as possible.

Numerous experiments have been instituted to determine precisely the nature of the contagious principle of plague, but still all doubt upon the subject has not been removed. It has, indeed, been fully proved that immediate contact suffices to spread the disease; but, notwithstanding the experiments of the commission of Cairo, it still remains doubtful whether mediate contact possesses a like power. The experiments of the commission were performed in the plague-hospital; and hence the doubt has arisen whether the disease, in their experiments, did not arise from atmospheric or other causes, independent of the operation of mediate contact.

Experiment 1. On the 15th of May, 1835, Dr. Bulard stripped himself in the wards of the plague-hospital, and put on the still-warm shirt of a patient ill of plague, and continued to wear it for forty-eight hours, without adopting any precautionary measures to prevent absorption. No evil consequences followed.

Exp. 2. On the 17th Zilkedjè, at eight o'clock in the evening, the condemned criminal, Ibrahim Hassan, æt. 18, was dressed in the shirt, jacket, and trousers, and put into the still warm bed of a patient ill of plague: on the evening of the 21st symptoms of plague appeared, and he died in the night of the 25th.

Exp. 3. At eight o'clock of the evening of the 7th Zilkedjè, the condemned criminal, Mohamed-Ebn-Ali, was dressed and put to bed in a similar fashion. He continued in his usual health till the morning of the 23d, when the disease appeared, and on the 26th he was convalescent.

The inoculation of dogs with the pus of buboes, with blood from the veins and heart, or with the serum of carbuncles, produced no consequences; neither had the introduction of the serum of carbuncles into the subcutaneous cellular tissues of dogs and asses any effect which could be ascribed to an animal poison. Dogs which devoured these pathological productions likewise continued healthy.

Exp. 4. On the afternoon of the 18th Zilkedjè, the condemned criminal, Hassan, was inoculated in four places in the right arm with blood from the cephalic vein of a plague patient. He was seized with plague on the 20th, and was convalescent on the 25th.

Exp. 5. On the 22d Zilkedjè, the condemned criminal, Mahomed Halil, a plethoric robust man, was inoculated with the blood of a plague patient; and again on the 30th, after an interval of eight days. The punctures, owing to mechanical irritation, became slightly inflamed, but no general symptoms followed.

The same man was afterwards inoculated with pus from a bubo; and Mohamed-Ebn-Ali, who had now recovered from the effects of the former experiment upon him, was inoculated with serum from a carbuncle: the disease did not appear in

either case. Clot-Bey now inoculated himself in the axilla and groin, with a like negative result.

In those cases mentioned above, where the disease appeared to be produced by mediate contact, it remained doubtful whether this was really the case, owing to the circumstances in which the experiments were performed; and the same causes invalidated the conclusions which might have been drawn from those cases in which the inoculation took effect.

From the fatal nature of the disease, it should be combated chiefly by prophylactic measures; and these Dr. Bulard considers under six heads: 1, the preparations of iodine; 2, the preparations of mercury; 3, frictions with oil; 4, inoculation with the matter of buboes and carbuncles; 5, inoculation with vaccine matter; 6, epispastics. Nothing as yet is known as to the preservative power of the two first; popular opinion speaks for the third. It has been asserted that small-pox acts as a preservative; but this still remains a doubtful question, although in the Levant the voice of the majority has pronounced it as such. Epispastics are rarely found on the bodies of those who have died of plague; and it is said that, of forty thousand cases which died in Constantinople, there was not one body on which was found an open issue; and Dr. Bulard affirms that, of fifteen hundred cases admitted into the hospital of Cairo, on one only an issue was found. From these facts it has been asserted that individuals with issues enjoy almost total immunity from the attacks of the disease.

Medicinische Zeitung, 1837. Nos. 40, 41, 42.

On the Medicinal Powers of the Caïnea. By Dr. ROBREDO.

BARON LANGDORF, Russian consul-general at Rio Janeiro, first announced, a few years ago, the wonderful virtues of this plant in Europe. It is the *Chiococca racemosa* of Linnæus, and of the natural family *Rubiaceæ* of Jussieu, and is very abundant in the Antilles, the Floridas, and many parts of Brazil. Its root is composed of a delicate bark, which has a bitter and slightly acrid and astringent taste, and, when broken, offering a resinous aspect; and a woody centre, which does not partake of these properties. Messrs. Pelletier and Caventou have found that it contains a bitter principle, to which they have given the name of Caïneic acid, an oily matter of a green colour, another of a yellow colour, and a coloured viscous substance. From the experiments of Messrs. Caventou and François, it appears that the root, and especially the caïneic acid, are powerfully tonic; that they increase the secretion of urine in a very remarkable manner, and that they are slightly purgative.

Three cases are related by Dr. Robredo in confirmation of these opinions. In the first case, a bilious diarrhœa had reduced the strength of the patient, a female, extremely; the suppression of this diarrhœa being always followed by thirst, scanty urine, extensive œdema, and excessive prostration. A recurrence of the diarrhœa removed these symptoms, but exhausted rapidly the strength of the patient. The employment of caïnea was begun, when the dropsical symptoms were extreme, and the urine scanty and sedimentous. A decoction of a drachm of the root in a pint of water was administered daily. The second day of its employment ten pints of clear urine were discharged, the stomach performing its functions regularly; and in five days a cure was effected, which the author thinks is radical, since, at the period of his writing, three months had elapsed without any disturbance of the patient's health, and her strength was quite restored.

In the second case, amenorrhœa had been treated by copious bloodletting, and anasarca was the consequence. The author having ascertained, by examination, that there was no disease of the uterus, administered caïnea. The ultimate effect was more slowly produced; fifteen days having been required for a cure, and the dose of the medicine (probably from deficient susceptibility of the patient,) having required increasing to a drachm and a half daily.

In the third case, that of a boy eight years old, there was extreme marasmus, œdema, costive bowels, and enlargement of the spleen; the whole the result of an

ague of three months' duration. All the symptoms, the enlargement of the spleen excepted, were removed in eight days, by means of a decoction of half a drachm of the root daily; and, by appropriate remedies, the spleen was afterwards reduced to its natural condition.

Jornal de la Academia de Medicina de Megico. Octubre, 1836.

Notice of the Bafureira of Cape de Verd. By Dr. B. A. GOMES.

THIS plant is indigenous at Cape Verd, and is employed by the inhabitants of the Cape Verd Islands and the coast of Africa to increase the secretion of milk. The writer has been assured by persons whom he thinks worthy of credit, that not only is it efficacious for this purpose in women who have been recently confined, but that it produces the secretion in virgins and persons of advanced life; so that infants have been nursed for a long time by females who, from their age and other circumstances, could not have furnished the secretion under the influence of any natural stimulant. Its mode of employment is by means of poultices, made of the green leaves, applied to the mammæ; or a strong decoction, with which the same parts and external organs of generation are washed. Sometimes such a decoction is taken internally, conjointly with its external application. The plant belongs to the family *Euphorbiaceæ* and the genus *Ricinus*. The writer is doubtful whether it is a variety of the *Ricinus communis*, or a distinct species. It has been raised in the garden of the Marine Hospital at Lisbon, from seeds sent from Cape Verd.

Jornal da Sociedade das Sciencias Medicas de Lisboa. Novembro, 1836.

Researches on the Febrifuge Properties of the Chloride of the Oxide of Sodium.
By Dr. GOUZÉE.

THE high price of the sulphate of quinine renders it very desirable that some other remedy, more attainable by all classes of society, should be discovered for the effectual treatment of intermittent fevers. In a memoir presented to the Academy of Sciences by Dr. Lalesque, in 1835, the chloride of soda was recommended as possessing medicinal properties as active as those of the sulphate of quinine, and as being fitted to serve as its substitute in all cases in which the quinine is indicated in periodic fevers. Dr. Gouzée gave, at first, but little credit to the value attached to the chloride, until it was further confirmed by the experience of a friend; but, to establish the efficacy of a new febrifuge remedy, various conditions are indispensable. It is not only necessary that the situation generally imparts to intermittent fevers that tenacity which prevents their yielding to all medicines: attention must also be paid to the particular case in which the experiment is made, to the season, to the medical constitution; which circumstances will alone, in certain circumstances, contribute powerfully to recovery. Thus, for example, it has just happened that at Antwerp, where the most favorable circumstances might appear to be combined for experimenting on this chloride, a number of fever patients have become spontaneously convalescent, shortly after their entrance into the hospital; having suddenly passed from a life of activity, and an abundant but not well selected diet, to circumstances of an opposite character. It is also very important to determine the quality of the medicine employed, as well as its exact mode of administration. In these matters too much exactness cannot be observed. The chloride of soda employed by Dr. Gouzée was recently prepared, marking 12 of the areometer, and decolorizing at least eight parts of the sulphate of indigo. The ordinary prescription has been half a drachm of the chloride in four ounces of distilled water. The patients have so taken this dose that the last quantity of it should be swallowed shortly before the occurrence of the paroxysm which it was wished to overcome; and, in order to isolate the patients as much as possible from all opposing influences, a very light diet only was allowed, and confinement either to the bed or chamber was enjoined.

Several cases are recorded, illustrative of the febrifuge action of the chloride of the oxide of sodium, and from them the following inferences appear to be fairly deducible:

1. The chloride of soda actually possesses febrifuge properties. 2. It is far from producing the certain and energetic effects of the sulphate of quinine. 3. It cannot, therefore, replace the sulphate of quinine in every case in which that salt is indicated in intermittent fevers; and it would be imprudent to hazard its employment in pernicious intermittents. 4. It is not irritant. 5. It may be had recourse to in recent intermittent fevers, disposed to yield, in individuals who are easily impressed, in women, in children; and it may, in general, be employed in all cases where there does not exist any danger. 6. The diminution of the intensity of the paroxysms during its use augurs favorably, but does not always announce an approaching cure. 7. It appears to exercise a favorable influence over engorgements of the spleen. 8. It remains a subject for further enquiry whether its dose and mode of administration may not be advantageously modified; if it may not be associated with other substances capable of rendering its action more energetic; if, lastly, in continuing its use, the frequency of relapse may not be diminished.

Revue Médicale française et étrangère. Février, 1836.

On Obliteration of the large Arteries. By M. LEGROUX.

At the sitting of the Academy of Medicine, on the 21st February, M. Legroux related the case of a lady, aged twenty-nine, who, three months previously, was seized with acute rheumatism. There was uneasiness in the præcordial region, an irregular pulse, and a loud bellows' sound, and these symptoms were attributed to endocarditis. Antiphlogistic treatment was employed. At the end of a month she appeared to be getting well, when all at once violent pains came on in the legs and feet, with coldness of the extremities. In a fortnight the arterial pulsations had completely ceased in the lower limbs. Eight days before death, the same symptoms were observed in the left arm, and the pulsations ceased there also. The pains continued to the last; no gangrene took place.

On inspection, the cavities of the heart were found to contain old and adherent fibrinous clots; the lining membrane of the auricles had lost its polish and transparency; the right subclavian, the termination of the aorta, the common and external iliaes, completely obliterated by old and adherent clots; the right iliac dilated at the level of the hypogastric to the volume of a nut, and containing a fibrinous, cystiform coagulum; three of the lumbar arteries were closed by clots, which projected into the aorta like the heads of nails.

M. Legroux thinks, 1st, that this disease was, in the first instance, rheumatic endocarditis; 2d, that, in consequence of this inflammation, coagula were formed in the cavities of the heart; 3d, that the obliteration of the arteries was commenced by the expulsion of coagula from the heart into the vessels. This opinion he founds on numerous observations, which prove the very frequent coincidence of coagula closing the arteries and old clots in the heart: he, however, admits that local obliteration may be caused by arteritis. 4th. That the obliteration, at first imperfect, was completed by successive additions of coagulum; the structure of the clots indicating that they had been formed at various times. 5th. That the adherence of the coagula to the walls of the artery was the result of inflammation caused by the presence of the clot as a foreign body. The inner membrane of the artery was only notably altered at points where the obliteration had existed for some time; that of the subclavian, which was recently closed, was only slightly injected.

Bulletin de l'Académie. 15th March, 1837.

On a particular Kind of Swelling of the Tonsils, Uvula, and soft Palate.

By Dr. RÖSCH.

In obstructions of the portal system, in deficient activity of the mucous membrane of the intestinal canal, and particularly in diseased states depending on sluggishness of the intestinal secretions, the tonsils, uvula, and velum swell from time to time, still remaining soft: they appear to be traversed by small blue ves-

sels, and to have acquired a blueish dark-red colour; the swallowing is somewhat impeded, and the voice not unfrequently acquires a hoarse tone. This condition may be apparent one day, and lost on the following; but it frequently continues many weeks, and disappears only on recovery. In general it keeps pace with the improvement or deterioration of the general complaint, so that, e. g., on the re-establishment of the action of the bowels by means of laxatives, it disappears. The author considers such a condition of the throat always as a symptom of obstruction, and also as a symptom of chronic gastritis, in cases where the patients are already very much debilitated, where there is an evident predominance of the venous system, and where there is a tendency of the blood, however small, to be evacuated by the intestinal canal. An analogous state of parts is seen in the blue discoloration of the lower lip in hydrothorax, hemorrhoids, &c., and which is indicative of obstruction.

Jahrbücher der in-und-ausländischen gesammten Medicin. No. 2. Heft 2. 1837.

Case of Chronic Hydrocephalus, cured by accidental Paracentesis.

By Dr. HÖFLING, of Heinfeld.

THE patient, a boy, æt. 5, presented the symptoms of chronic hydrocephalus in a marked degree. The temporal and frontal regions of the head were, in particular, largely developed, and projected far over the diminutive face. The general health, however, still continued good, and there was no particular ailment. Whilst in this state, the boy received a severe blow on the forehead from the hoof of a cow, which produced momentary stupefaction. Examination shewed that the frontal bone, already extremely thin, had been broken by the stroke, and that a considerable quantity of water had already escaped, and still continued to flow from the wound. The water continued to come away in a steady flow during the eight following days, when the wound closed. Two years have now elapsed since this accident, and the boy is in the enjoyment of good health. The head is still large when compared with the face, but its proportion to the body is nearly natural.

Wochenschrift für die gesammte Heilkunde. 1837. No. 41.

Practical Observations on the Treatment of Diseases with Milk and Whey.

By Dr. LEVISEUR.

DR. LEVISEUR's experience of the utility of milk and whey is confined to a few diseases: on the different value of different kinds of milk, he has not made any experiments, that of the cow being the milk which he almost always employed. We shall notice his remarks on the utility of milk in some chronic diseases; and of its utility, as a remedy of more value than any other with which he is acquainted, in these diseases, he speaks almost enthusiastically. His patients were children and young people under thirty years of age. The common character of all the diseases was emaciation. 1. Children in their second year, not long after their removal from the mother's breast, and before the end of teething, sometimes become the subjects of diarrhœa, which, from neglect or mismanagement, acquires great obstinacy, assumes the character of lentergy, is attended with increasing emaciation, and terminates life, with or without dropsy. They are emaciated and debilitated, the skin hanging in loose folds about them; they swallow what is offered them to eat, and void it, per anum, almost immediately, mixed with a peculiarly fetid mucus. The collapsed and soft belly particularly distinguishes this condition from the so-called atrophy of children. This is a condition in which Dr. L. has especially employed the milk-system, and with constant success; provided always that no *febricula hydrencephalica*, no deep-seated organic lesion, or no dropsy exist, and that the treatment is pursued strictly according to the prescribed rule.

Mode of Treatment. Medicine is to be totally excluded. If a good nurse can be procured and the child will submit to it, it may be put again to the breast; but this cannot often be managed. Cow's or goat's milk, somewhat diluted with water but not sugared, either just milked or boiled, must be given to the child *without*

limit: let it drink as much as it may. It is taken with great appetite, and in such abundance as to swell up the belly; the lookers-on being apt to say that the child must burst. Sleep generally follows; the urine flows in considerable quantity, and the relaxed evacuations from the bowels appear to consist of nothing but coagulated milk. Nothing whatever but the milk is to be given to the child. It may be repeated in the same manner, morning, noon, and evening. If the child has been indulged with various kinds of food previous to this treatment, it may require firmness on the part of the parent and nurse to refuse that for which it will cry; but in two or three days, the child will contentedly see others eat, itself being perfectly contented with its milk. In the course of from eight to fourteen days, the improvement becomes evident, the diarrhoea diminishes in frequency, &c. Before eight weeks have elapsed, the lientery has long ago ceased, the condition of the child is wonderfully improved, and in general several teeth will have protruded themselves. If at this time some other diet is had recourse to, its chief ingredient should be milk, and this until the first dentition is ended. Errors in diet and cold readily bring back diarrhoea. If the use of baths can be associated with the foregoing treatment, the child's improvement will be the more rapid. Great cleanliness and frequent change of clothes, careful ventilation of the child's chamber, and the wearing of a piece of fine flannel over the abdomen, are to be attended to.

2. In the chronic disorders of *adults*, in which Dr. L. employs milk and whey, the chief indication appears to be to stop the rapid emaciation. In most instances the origin of the disease was acute, and dated back months or even years, and its course was not clearly given. In general, the emaciation was attended with a slight febrile attack after the meal at noon or evening, and accompanied with more or less morning sweatings. In no case was suppuration of any viscus certain; in the majority of cases the certainty was quite the contrary. In a few cases, there was thirst after the morning perspirations, which ceased in the evening. Except in two cases there was no real hectic. The disposition was disturbed and particularly irritable, the sleep mostly interrupted, there was either anorexia or a sensation of pressure and obstruction in the stomach and bowels after taking food, or that which was eaten was again vomited. The bowels were constipated, and in females, previously regular in their menstrual function, this failed. An annoying dry cough occurred in some towards evening. Childbearing, menorrhagia, ague, and other imperfectly described female diseases, hypochondria and hysteria were among the diseases to which the origin of these complaints was ascribed. Many had to this time neglected themselves, others had adopted all sorts of tonic and stimulant treatment, all without benefit, or even harm. All these patients adopted an exclusively milk or whey treatment, with one exception, where, on account of the great feeling of hunger, something more was permitted.

General Remarks on the Treatment itself. (1.) Patients in the condition above described, subjected as they have been to various systems of treatment, will willingly submit to the milk-treatment, but it requires a discreet management on the part of the physician to ensure their observing it during eight or ten weeks, which will alone ensure a successful issue. (2.) In the choice between milk and whey, the degree of rapidity with which emaciation took place, the amount of appetite, the violence of the thirst, and the existence or non-existence of vomiting, were Dr. L.'s guides. Where the consumption was rapid, and where, even although emaciation was slow, there was little appetite and less thirst, and where the patient vomited what he had eaten, milk was always selected on account of its greater amount of nutritive matter. But if, when the emaciation advanced slowly, the appetite and thirst were very considerable, whey was preferred, of which the patients sometimes consumed three quarts in the twenty-four hours, a quantity which could not easily be digested of milk. When there were no particular grounds for preference, the choice of milk or whey was left to the patient himself. (3.) Often as Dr. L. has thus employed milk and whey, he has not in the circumstances mentioned met with one contra-indication, nor in any individual case has he encountered any obstacle to the uninterrupted continuance of the treatment, i. e. in the employment of an exclusively milk and whey treatment. Herein his experience

leads him to different conclusions from those who speak of indications and counter-indications in the use of milk. (4.) With regard to the duration of the treatment, the author says that he did not prescribe a gradual transition to other diet, until he was fully convinced of the convalescence of his patient, that this rarely occurred before the expiration of eight weeks, and that in some cases it required a period of between four and five months.

[We have always been of opinion that the extremely active, or, as it is called, *heroic* treatment of chronic diseases, so prevalent in this country, is most injurious, and often fatal; and we have looked forward to much indirect benefit from the prevalence of the homœopathic doctrines, in demonstrating practically to the sectators of the *medicina proturbatrix*, that it is wiser and safer to *do nothing* when no rational indication presents itself, than to hazard the consequences of the blind application of potent drugs to a delicate or diseased system. In this point of view we regard the observations of Dr. Leveiseur as important. Our own experience, now of many years, enables us to testify to the beneficial results of an analogous though not exactly similar mode of treatment in cases of the kind related; and we most confidently can promise our younger friends more success in numerous instances from adopting Dr. L.'s dietetic practice, than from the calomel and black-dose system which has so long disgraced British medicine.]

Wochenschrift für die gesammte Heilkunde. Nos. 25, 26. 1837.

On [Supposed] Inflammation of the Pancreas, observed during an Epidemic Parotitis. By Dr. CONTI, of S. Natoglia.

IN October, 1835, the cold weather set in very suddenly and severely, so that the sides of the mountains and the vallies beneath were covered with snow; and from this time until May 1836, an epidemic parotitis made its appearance, that attacked a considerable number of persons of every age and sex. Besides the usual train of symptoms attendant upon the inflammation of the parotid glands, many of the males suffered from a similar affection of the testicles, while, in the females, the mammæ participated in the disease.

In the greater part, however, (above two hundred persons) no sooner was a feeling of uneasiness felt in the parotids, than a sharp oppressive pain came on in the epigastrium, attended with severe dyspnœa; attempts to vomit, with sour belchings and a flow of limpid water from the stomach; and followed by decided vomiting of a yellow frothy matter succeeded by a whitish fluid with a saline taste. At the same time the stomach was exquisitely tender to the touch, respiration was painful and laborious, and the patients complained of a sensation of weight and pressure, as though by a large stone, over the epigastric region, which was remarkably increased by swallowing even the smallest quantity of fluid. The vomitings were not copious but frequent, amounting to fifteen or twenty times in the course of the day. Some patients had hiccough; and the beating of the cæliac and aorta were perceptible to the eye. In some the epigastric pain extended itself over the whole abdomen, with a burning and pricking sensation of a rapid and fugacious character. An ordinary diarrhœa was soon followed by the discharge of a yellow frothy matter, and this by a white fluid equally frothy, like the white of egg beaten up, and emitting no fœcal odour. The patients who had diarrhœa complained less of the pain in the epigastrium; and although they suffered from the acid eructations and flow of limpid fluid, their attempts to vomit were ineffectual. The diarrhœa, like the vomiting, lasted six or seven days.

It is remarkable that the patients who suffered from this form of disease had no affection of the testicles or mammæ, while those who laboured under inflammation of those organs felt no uneasiness in the epigastrium.

The disease, when properly treated, did not last more than a fortnight; but, neglected or badly treated, it was often prolonged to six or nine weeks.

[The preceding narrative is curious, although the author adduces no positive proof of the seat of the disease being the pancreas, or of its inflammatory nature.]

Bullettino delle Sc. Med. Jan. 1837.

New Method for the Cure of Stammering. By Dr. B. VOISIN.

DR. VOISIN being afflicted with an impediment in his speech left no method untried, from the pebbles of Demosthenes to the methods of MM. Leich and Malbouche, for the purpose of removing it. The systems of M. Leich and his imitators instruct the stammerer to carry the tongue high, the point against the superior dental border of the incisors, so as to detach this organ as little as possible from the palate; and to observe a sort of measure in speaking. Dr. V. does not accord with these directions, and proposes another which he considers more simple and efficacious.

Chance first led him to the discovery of the method he recommends. He was reading a paper before a society, and wishing to do so with some energy, he happened to look in a mirror which was opposite him, and perceived that he rested the border of his right hand upon his chin, in a manner so as to depress the inferior maxilla and hold the mouth half open. The idea immediately suggested itself that this instinctive and mechanical movement might contribute to his reading more promptly and easily. In fact, upon ceasing the pressure, the difficulty of expression was quickly reproduced; but upon replacing his hand the freeness of articulation immediately returned. Endeavouring to give an account of this, he observed: 1st. That the mouth was kept half open, the distance between the teeth being a line or a line and a half. 2d. That the tongue, abandoned to itself, in the state of repose placed itself against the inferior dental border, whilst during pronunciation it is projected forwards and upwards, but is withdrawn almost immediately behind the alveolar arch. 3d. That a medium pressure is necessary upon the chin; this should be sufficiently strong to resist the muscles which move the inferior maxilla, without impeding its movement of elevation, so strong as to prevent perfect approximation. To produce this pressure and, at the same time, make it excusable, it is necessary to use a certain delicate art, so that the manœuvre may not appear forced, but on the contrary almost natural. This pressure should be made with the external border of the right or left hand indiscriminately, the thumb applied upon the chin and the fingers free. Since he has made the discovery he finds he frequently takes the position without thinking of it, and has observed the same in other individuals afflicted with impediment of speech. This habit does not appear to be peculiar to stammerers, since it is frequently assumed by timid persons when speaking in public. Dr. V. has only had an opportunity of trying it in two individuals, but the effect surpassed his expectations.

Bulletin de l'Académie Royale de Médecine. September, 1837.

SURGERY.

On the Treatment of Aneurism by Ice. By M. REYNAUD, Surgeon-in-chief of the Naval Hospital at Toulon.

A SEAMAN, aged thirty-four, became affected in the year 1833 with pains in the right groin and thigh, which were followed by the appearance of a hard, circumscribed tumour in the upper part of the thigh, which soon increased in size. When brought to Toulon the following was the report of this case. The right lower extremity is much enlarged, the circumference of the upper part of the thigh being thirty inches, whilst on the opposite side it is only fifteen. The base of the tumour extends from the anterior superior spine of the ileum to the linea alba. Its apex, wide and flattened, reaches to an inch and a half below the crural arch. In the centre there is a fluctuating point of a bluish tinge, where the skin appears so thin as to threaten an approaching rupture. The tumour is divided into two parts by the fallopian ligament, which forms a groove in the middle. Pressure does not sensibly diminish its volume. Pulsations are with difficulty perceived in the tumour; they are isochronous with the action of the heart. No pulsations are felt in the course of the femoral, the popliteal, and anterior tibial arteries. The whole limb is œdematous. The leg, semiflexed on the thigh, is incapable of motion.

From this examination it appeared evident that the common iliac was diseased as well as the external iliac and the femoral. It became therefore a question whether the operation of tying the aorta should be had recourse to; but on considering the unfortunate result of all those cases in which that operation has been hitherto employed, it was resolved to treat the case by the application of ice. The patient was accordingly put on very low diet, and a bladder containing pounded ice was constantly applied to the tumour. In four days (20th February, 1834,) the circumference of the limb diminished four inches; the pulsations were scarcely perceived, even by the ear, and the temperature of the limb, previously low, began to rise. On the 6th March, the circumference was again diminished by an inch, and the integuments were losing their bluish colour. After eight months' treatment no pulsation was felt in the tumour, and the limb was moved with facility. Compression was now applied by means of a bandage, and frictions of hydriodate of potash were employed to the engorged cellular tissue. At the end of eleven months the limb was reduced to nearly its natural size, but no pulsations were yet felt in the ham or leg. The patient was allowed to rise and walk a little on crutches, but the swelling and pain being somewhat increased, he was again restricted to his couch. After this, the improvement continued, but more slowly. The application of ice was continued unceasingly for two years, at the end of which time the tumour was hard and flattened, and the thigh had returned to its natural state. He was now allowed to walk. Pulsations were now felt in the lower third of the femoral artery and in the anterior tibial. In short, the aneurism was cured, and the patient was shortly afterwards discharged from the hospital, scarcely requiring the support of a stick in walking. During this long course of treatment the diet was light, the drink acidulated barley-water, and no medicine was administered.

Gazette Médicale. September, 1837.

On Dislocations of the Shoulder-joint. By M. VELPEAU.

MUCH as has of late years been written on the subject of luxations of the humerus, M. Velpeau considers that, both as regards their diagnosis and mode of reduction, much remains to be done. The paper which we are about to analyze contains principally the result of his own experience on the subject, in which he has endeavoured, though not to settle all doubtful points, to contribute something to their explanation.

The question as to whether the humerus can or cannot be dislocated upwards or downwards, he regards as one merely of words; the careless use of terms having been the origin of misapprehension. There are only two principal directions in which the humerus can be dislocated,—the *antero-internal* and *postero-external*, as it respects the glenoid cavity; i. e. on the side of the axilla and on that of the sub-spinous fossa.

I. The *postero-external, sub-acromial, sub-spino*, is now admitted beyond a doubt.

II. With regard to the subject of *incomplete* luxations, M. Velpeau considers that there is the same confusion as that above referred to. If, by an incomplete luxation, is meant that the cartilaginous surface of the head of the humerus has but half escaped from the glenoid cavity, its existence must doubtless be denied; but if by incomplete it is to be understood that the head of the humerus is detained by some portion of its anatomical neck upon the border of the glenoid cavity, the possibility, the existence, and even the facility of such a dislocation should not be denied. This should not be regarded as a complete luxation; because in it there is manifestly escape of the cartilaginous hemisphere and of a portion of the bony circumference which separates it from the surgical neck of the bone, whilst, in the incomplete form, the osseo-fibrous structure remains partially within the capsule. Of incomplete luxation, so understood, M. Velpeau believes that he has met with three cases; two outwards and one inwards. Very many surgeons have admitted incomplete luxations towards the axilla as of possible occurrence. M. Velpeau has related a case, with its dissection, illustrative of this form of accident. Concerning

these cases, however, although in this instance it is contrary to the author's opinion, there is the doubt whether the situation in which the bone was found after death was not acquired subsequent to a complete dislocation of the head of the bone.

III. The *antero-internal* or *axillary* luxations are thus divided by M. Velpeau: (1) sub-pectoral; (2) sub-scapular; (3) sub-clavicular. These forms, he adds, have a mechanism, a seat, and a mode of reduction, which it is often of advantage not to confound.

(1) The *sub-pectoral* is that form in which the head of the humerus is situated in the hollow of the axilla, between the sub-scapular and pectoral muscles. This is the luxation downwards of surgeons. (2) In the *sub-scapular*, the head of the bone, lodged in the axillary fossa of the scapula, is separated from the hollow of the axilla by the sub-scapular muscle. (3) In the *sub-clavicular*, the head of the humerus, situated near the root of the coracoid process, is, as it were, bounded beneath by the superior part of the sub-scapular muscle. The sub-pectoral dislocation will chiefly happen when, at the moment of displacement, the arm is elevated above the scapula beyond a right angle. The capsule is then torn beneath and inwards, so that the head of the humerus glides over the lower border, or through the least elevated fibres of the sub-scapular muscle, to place itself beneath the larger pectoral, in the hollow of the axilla. If the elbow is somewhat less elevated, or almost in the same plane with the shoulder, when the displacement, either by a direct or by an indirect cause, takes place, the sub-scapular luxation must be expected. As to the sub-clavicular luxation, it is scarcely possible but as a consequence of force, exercised upon the elbow or hand from below upwards and from without inwards, or, indeed, after violence or blows effected upon the upper third of the humerus in the same direction, the elbow being confined by some other force. These three kinds of dislocation present certain differential signs, which, first of all established in the living subject, and afterwards by direct experiment upon the corpse, have appeared to vary but in a very trifling degree; thus—

(1) <i>Sub-pectoral.</i>	(2) <i>Sub-scapular.</i>	(3) <i>Sub-clavicular.</i>
Pectoralis major more or less raised; sub-clavicular hollow preserved.	Pectoralis major scarcely raised; sub-clavicular hollow almost effaced.	Pectoralis major thickened without being elevated; relief in the situation of the sub-clavicular hollow.
Acromion very prominent; depression of deltoid considerable.	Acromion less prominent, and deltoid less stretched.	Acromial projection and depression of the deltoid more evident behind than before.
Head of humerus almost naked beneath the skin in axilla, which it fills; easily felt through pectoralis major from before backwards.	Head of humerus separated from skin in axilla by a thick layer of tissues; can be but with difficulty traced through the pectoralis major: the fingers can be passed between it and the inner parietes of the thorax.	Head of humerus at the top of the axilla, and difficult to feel; appreciable in front through the elevated portions of the deltoid and pectoral muscles.
Voluntary and communicated movements tolerably easy. Elbow separated from thorax, and carried somewhat backwards.	Movements of limb limited and somewhat difficult, producing a species of crepitation. Elbow separated from thorax, and carried somewhat forwards.	Movements painful, and almost impossible to effect; occasionally crepitation.
Arm from two to twelve lines longer than that of the opposite side; inferior angle of scapula raised considerably backwards, separated from the vertebral column.	Arm a few lines longer or shorter, or of the same length as that of the opposite side. Whole border of scapula elevated backwards.	Arm slightly bending backwards, but scarcely separated outwards from the trunk; shorter than that of the opposite side; posterior border of scapula more projecting superiorly than below.

The above must rather be regarded as general rules than as an absolute diag-

nosis. The three species of luxations above described may differ by such trifling circumstances as to render an absolute definition impossible. The elevation of the inferior and superior angle, of the whole posterior border of the scapula, and its distance more or less from the vertebræ, appear to deserve a degree of attention which they have not yet received. The species of crepitation alluded to in the second and third forms of luxation is not found in the sub-pectoral. Occasionally the sound is very loud. In order fully to appreciate it, the shoulder should be grasped by the hand resting upon the acromion, whilst the limb is quickly rotated by the other hand. This symptom should be very carefully studied, as it may give rise to the suspicion of fracture which does not exist; but, instead of the fine and sonorous crepitation of bone, it is rather a deep muffled crackling of the cartilages.

The change in the *length* of the arm is a subject of much importance, considering what has been lately advanced on the subject. M. Malgaigne has maintained that it is always longer when dislocated, and that it cannot be shorter; and M. Sedillot, that the want of such elongation would be inexplicable. Velpeau was at first disposed to coincide in these opinions. Its value, if true, as a means of distinguishing dislocation from fractures of the neck of the humerus would be very great, because in the latter the arm is almost always shortened: but he did not find that it was in accordance with fact, and others, to whom he communicated his experience, were equally disappointed. A pupil, who performed twenty experiments upon the dead body, with the intention of ascertaining the truth, came, indeed, to a contrary conclusion; i. e. that, in luxations of the humerus, the arm is almost always shortened. M. Velpeau believes that he has not only ascertained that the arm is not always lengthened, but the conditions to which either its lengthening or shortening is ascribable. Having luxated the humerus in the dead body, he has assured himself that (1) there is elongation of some lines, from half an inch to an inch, but never to two inches, as has been inadvertently said, in the sub-pectoral luxations; (2) the length is almost the same, from two to three lines, more or less, than that of the healthy side, in the subscapular luxations; (3) there is a shortening, varying from a few lines to an inch, in the sub-clavicular dislocations. Applying these results to clinical practice, he has found them hitherto universally confirmed.

M. Sedillot, renouncing the idea that elongation was necessary, stated, after having seen some of Velpeau's cases, that the dislocated arm ought always to be shortened when it is held in a horizontal position; i. e. at a right angle with the body, whilst the measurement is being made. The head of the humerus, having slid towards the subscapular fossa, should in this case be placed upon a plane posterior to that of the glenoid cavity, and thus render the shortening of the limb necessary. But a condition apparently so true in theory is contradicted by experience. A case is related of a sub-scapular luxation: when measured whilst parallel to the trunk, the luxated arm was of the same length as its fellow; elevated to a right angle with the trunk, it was found to be about four lines longer. The measurement was repeated many times, and in all possible manners, but always with the same result. Hoping that this case might be justly regarded as exceptional, the enquiry was repeated in other cases, but with no different effect. In six individuals who allowed their arms to be measured in this position, the limb was found to be lengthened; or, at least, not to present any shortening. In one of these, the lengthening was nearly an inch. This fact was attested before very many witnesses. It is difficult to explain. Unless it be admitted that, in elevating the arm, the acromion and the glenoid cavity are depressed backwards, so as to increase by one or two inches the space which naturally separates the anterior extremity of the spine of the scapula from the epicondyle, the author does not see how to explain the elongation.

The application of the foregoing observations to practice is especially important, as it concerns a choice of the various methods of reduction which have been so greatly commended. It is from not having paid sufficient attention to the distinctions above established, that practitioners are so little agreed as to the relative or absolute value of each mode of reduction. By excluding all methods but one, it is

impossible to obtain any very exact inferences; for the method of reduction best suited to one kind of luxation may be, of all, the least proper for any other. Regarding the subject thus, M. Velpeau concludes that (1) the vertical extension, the limb being raised to the side of the head, is best accommodated to the sub-pectoral luxations; (2) the horizontal extension succeeds best in the sub-scapular luxations; (3) in the sub-clavicular, oblique extension downwards, and then horizontally, is the best suited to the object in view. In the majority of simple cases, each one of these methods may suffice; and such is the reason that they have all been adopted. The elevation of the arm, which, in the sub-pectoral luxation, brings, without effort, the head of the humerus beneath the border of the sub-scapular muscle, towards the anterior border of the scapula, and which then allows it to enter into the glenoid cavity, does not present the same advantages as it regards the other luxations. This movement, which the head of the humerus suffers, favouring, for example, the folding or rolling up of some of the fibres of the sub-scapular muscle and capsule between the border of the glenoid cavity and the anatomical neck of the humerus would, in the sub-scapular and sub-clavicular luxations, be but an impediment to the end at which the surgeon aims; but the extension at first somewhat oblique, and afterwards horizontal, enable the end of the bone to find with facility the aperture in the capsule, if the luxations be of the other forms. Conformably with these principles, M. Velpeau employs a certain mode of reduction in a certain species of dislocation; but, even in sub-pectoral luxations, the horizontal extension succeeds almost as well as the vertical; and, although the latter method may be preserved in practice, it will be probably but as exceptional.

In the latter part of his paper, M. Velpeau makes some observations on direct luxations of the humerus. A case is given, in which it was certain that, when the dislocation occurred, the arm was applied to the side of the thorax, and that it took place in consequence of direct pressure upon the shoulder. Great contusion and enormous ecchymosis beneath the acromion, together with the absence of all injury of the elbow or hand, would have sufficed to prove the mode in which the dislocation occurred, even if the account of the individual himself had not been conclusive. This is a rare case, and valuable as evidence against those who deny the possibility of dislocation occurring in such a manner; particularly as, from the swelling and species of crepitation accompanying it, together with the facility with which blows upon the shoulder fracture the neck of the humerus, the opinion would generally have preponderated in favour of such a fracture, rather than of a dislocation the possibility of which is scarcely credited.

Archives générales de Médecine. Tome ii. Juillet, 1837.

On sudden Dilatation, a New Method employed by PROFESSOR LALLEMAND in destroying Strictures of the Urethra.

THE method employed by M. Lallemand is opposed to the general practice in similar cases; but if his account of its success be correct, it strongly recommends itself to the notice of surgeons.

After having well determined the existence of one or more strictures in the urethra; after having above all things ascertained that no acute inflammatory condition exists, the patient is prepared for the treatment by some general or local baths, by a mild regimen and cooling drinks. The first gum elastic sound is then introduced. It is rare that with perseverance and care, No. 3 cannot be passed; but rather than over-fatigue the patient it is preferable to pass a sound of smaller size or a piece of catgut. When the sound has passed the obstruction, it must be fixed in the usual manner, rest must be enjoined and abstinence. The cooling drinks should be continued. If the sound has been introduced with facility, the patient may be again visited in two hours' time, and by gently drawing the instrument backwards and forwards it may be ascertained whether it is free or the contrary in the urethra. In the former case, it must be withdrawn, and a sound of the next size introduced: in the latter, it must be allowed to remain for at least an hour. The sounds must be afterwards changed every two hours, or even every

hour; taking care however that no sound is removed, until it ceases to be retained tightly by the canal of the urethra. If it is found that the instruments pass easily, instead of passing regularly through the series of sounds, one may be here and there omitted; thus, Nos. 5 and 7, or 7 and 9 may be employed, omitting Nos. 6 and 8. In the course of two or three days, and sometimes in the course of twenty-four hours, the surgeon is enabled, by these means, to introduce No. 14, and to dissipate the engorgements which frequently diminish the urethra in the proportion of three-fourths of its caliber. As soon as the last sound is withdrawn, the canal returns to its ordinary diameter; but it is an essential part of the treatment that it should be submitted to the forcible distension produced by No. 14, otherwise the stricture would not be destroyed. The sounds when removed are discovered to be covered with mucosities. This is an exudation both necessary and useful. But if the presence of the sounds should cause too great a degree of irritation, this may require local depletion, &c.

When, after some instruments have been introduced, considerable difficulty is found in reaching the bladder, and particularly when the disease is of ancient date, it is possible that in addition to the submucous engorgement, there exist actual callosities. In this case, the patient should be allowed to rest for a day or two, and cauterization should be then had recourse to. This may be done in the ordinary manner, and it is rare that one cauterization will not be sufficient to perfect the cure, already very advanced by compression. A few days after the cauterization, some small eschars are evacuated from the urethra together with the urine. The only precaution required then is, that, during two or three weeks, an elastic bougie of the size No. 8 or 9 should be introduced every night into the bladder. This precaution, to which indeed the patient should submit whether there has been necessity to employ caustic or no, suffices to preserve afterwards the ordinary diameter of the canal.

Dilatation thus employed possesses the advantage over the other means of being equally efficacious, and less subject to accidents; and, besides, its greatest merit is to shorten the treatment very materially, and to facilitate, when it is necessary, the employment of cauterization. Patients also submit to it with greater willingness; for it is well known that the abuse of cauterization has lately given rise to considerable prejudice. It is now more than three years that Professor Lallemand has employed with success this new method, distinguished by the name of *sudden dilatation*, and many to whom it has been communicated have found reason to congratulate themselves on being acquainted with it.

Gazette Médicale. No. 26 *Juin*, 1836.

On a new Method of Treating the Hectic Fever which follows the Operation for Empyema. By M. FUSTER.

M. RECAMIER, considering the hectic fever which follows the operation of empyema to be caused by an alteration of the pus consequent on its mixture with the external air, has proposed to inject into the pleural cavity, immediately after the operation, a certain quantity of water, at the temperature of 28 or 30° R. (about 98° F.) to fill the void which would otherwise be occupied by atmospheric air. He has not found pain or inflammatory reaction to result from the presence of the water, which must be injected in less quantity in proportion as the compressed lung regains its normal dimension. The use of injections is contraindicated when the pleura is in an irritable state, or when tubercles exist in the lungs. "In fact," says M. Fuster, "this practice is only applicable when the pleura and lungs have returned to a healthy state, and we have only to treat the fluid effusion, the result of an old affection." If these conditions are necessary to the success of the practice, it will be, indeed, rarely applicable; for we never find the pleura in a healthy state when purulent effusion exists, and the patient is much more frequently destroyed by the disease of the serous membrane than by the existence of pus in the cavity. It is said that M. Recamier has treated many cases successfully by this method; but, as no numerical details are given as to the proportions of successes

and failures, we know not what degree of confidence may be placed in the statement. We have, since the year 1832, made trial of the method of M. Recamier, not in cases of empyema, but for the hectic fever which is consecutive on large suppurations of the extremities, and among others for deep abscess of the thigh; but, notwithstanding the precautions used, we have not been able to prevent the bad effects of absorption, which have proved fatal more or less quickly.

La Presse Médicale. 1837.

Fracture of the Ribs by Contre-coup. By Dr. MONDIERE.

THE author relates this case as unique. A robust man, aged forty-five, fell from a height of about forty feet, upon some ground recently dug. When he fell, his hands were crossed over the chest, the legs flexed on the thighs, and the thighs on the pelvis; the head also was inclined upon the thorax; so that he fell flat upon the right side, the only part which came in contact with the ground. He lost almost wholly the power of motion of his lower limbs; the sensibility of which was, nevertheless, very much increased. The fourth, fifth, and sixth ribs of the left side were fractured transversely, at a very slight distance from their cartilages. The integuments covering the broken ribs were not in the slightest degree bruised; and the manner in which he had fallen having been ascertained from others besides himself, no other inference was admissible than that the fracture was a result of *contre-coup*. On that part of the right side on which he had fallen was an extensive ecchymosis, exactly opposite the seat of the fractures. The patient perfectly recovered.

Archives générales de Médecine. Tome ii. *Juillet*, 1837.

On the Treatment of Wounds by the Affusion of Cold Water. By M. GODIN.

IN the British and Foreign Medical Review, for January, 1837, we noticed the observations of M. Josse, of Amiens, on the treatment of many diseases by the affusion of cold water. The author of this paper, a pupil at the Hôpital Neckar, relates several cases of lacerated wounds treated in this manner successfully by M. Berard. The views of M. Berard as to the therapeutical effects of the cold affusion are thus explained:—After relating a case of very extensive lacerated wound, which cicatrized rapidly, M. Godin continues,

“I think that this favorable result is in part to be attributed to the continuance of a very elevated temperature during the month of July and part of August. It may perhaps appear strange that I should attribute an advantageous effect to *heat* after having established that of *cold* applied by means of irrigation, but we must here make an important distinction. *Cold* can be of no advantage in promoting the cicatrization of wounds, and it is not with this view that M. Berard employs it locally in the system of continuous irrigation. Every contused wound must suppurate; suppuration cannot be established without a reaction of more or less intensity; and it is in order to restrain this reaction within moderate bounds, to prevent the extension of inflammation to neighbouring parts, that the affusion is to be employed. It becomes useless when suppuration is well established.”

Archives gén. de Méd. *Mars*, 1837.

Method of Promoting a Flow of Blood after Venesection. By Dr. BURDACH, of Triebel.

IT is not unfrequently the case, when venesection has been performed, that the vein is found empty, and that no blood can be drawn. Under these circumstances compression and friction are often of no avail, and the only sure means of exciting the venous circulation is to press the vessels of both arms, and by that means to cause a sympathetic excitement of the corresponding venous trunks of both sides: thus a flow of blood is rapidly effected, and it may be drawn to any extent. When a vein has been opened without effect, the other arm should also be bandaged above the elbow, and a little tighter than if it was intended to perform venesection in it: the consequence will be, that its veins will swell from above downwards to the

fingers' ends. Very shortly the veins of the other arm will fill in the same manner. As soon as the state of congestion is indicated by the feeling of deadness in the fingers' ends, the bandage should be loosened to such an extent that it may be nearly removed; then the blood should be gently directed by the pressure of the thumb to the orifice made by the lancet, from which, in this way, a plentiful and continuous stream may be made to flow, to be regulated by the compression or relaxation of the bandage. It is essential to the success of this experiment that the compression, which is not merely mechanical, and which acts in a peculiar manner upon the venous trunks, should be properly regulated, increased or diminished. This practice will also be found very useful in bleeding those women in whom, from their corpulency, the superficial veins of the arm are found with much difficulty, and furnish but little blood. Only in these cases more time is required; seeing that the very small veins have not only to fill but to expand; whilst, in those above mentioned, the caliber of the vessels is sufficiently large, and they are only empty from derangement of the small circulation, or from a primary, morbid inactivity of the peripheral venous system. This method of promoting the abstraction of blood may be often practised, with a good result, on the feet; but here its success is not so certain, probably because the circulation is not so active, and therefore to obstruct it not so efficacious as in the arm.

Graefe und v. Walther's Journal. B. 22.

On the Use of Soot in Diseases of the Bladder. By M. ANDRÉ GIBRIN.

THIS gentleman has transmitted to the Academy of Medicine six cases of chronic inflammation of the bladder, in which soot was beneficially employed. In two of these, which proved fatal, and in which ulceration of the fundus existed, the symptoms were alleviated: the others were cured. The symptoms were occasional retention of urine, pain at the hypogastrium, frequent desire to make water, a turbid and fetid state of the secretion, which was mixed with mucus and sometimes with blood, and deposited a viscous sediment which adhered to the sides of the vessel. Having tried various modes of treatment without success, M. Giboin determined to have recourse to the decoction of soot, from the external application of which he had derived great advantages; but he does not state how, or in what diseases, he had employed it.

He took from the chimney two ounces of compact soot, broke it up, washed it, and boiled it in a pound of water. The decoction was filtered through paper, and injected into the bladder twice a day. The good effect followed so closely the administration of the remedy, that it was impossible to be mistaken as to the cause. The pain ceased, and the patient obtained sleep, to which he had been for some time a stranger. The urine gradually became clear, and recovered its natural appearance. The two patients in whom ulceration of the bladder existed obtained much relief from the injection, and, on examination, the ulcerations were found to have lost that livid appearance which they usually present.

Bulletin de l'Académie. 15 Mars, 1837.

On the Use of Chloride of Lime in Wounds attended with much Pain.

By Dr. CHOPIN.

IN wounds produced by contusion, laceration, or by the explosion of gunpowder, where there is much pain, speedy and certain relief, says Dr. C., is produced by chloride of lime. That this relief is not the effect of cold or any other cause than the chloride in solution, the author is convinced by many experiments. Charpie, moistened with the same solution, has been also found a useful application in relieving the pains which sometimes follow delivery, which depend on small excrescences in the vagina. That such is frequently the case, Dr. C. is convinced from repeated examination. Excoriated breasts are most efficiently treated by the use of the same external application.

Jahrbücher der in-und-ausländischen gesammten Medicin. Heft i. 1837.

Treatment of incised Wounds by Means of the Membranes of the Egg. By Dr. HEUSSNER.

Dr. H. commends a popular remedy, not generally known, as a means of keeping the edges of wounds in contact. The parents of a child, who had cut its lip considerably by a fall, were unwilling that sutures should be employed, and the constant escape of saliva from the wound prevented the adhesion of the ordinary plaster. The mother of the child broke an egg, and took from it the thin membrane which lines the shell. This was placed obliquely across the wound in several strips. They adhered well, quickly dried, and united so closely to one another, that the borders of the wound came into close contact, preventing the escape of saliva, and favouring the healing process, which was complete in a few days.

Wochenschrift für die gesammte Heilkunde. No. 25. 1837.

MIDWIFERY.

Report of the Berlin Lying-in Hospital, from October 1, 1829, to December 31, 1835. By Professor BUSCH.

[THE original Report, of which the following is a condensed analysis, occupies nearly two whole Numbers of the valuable Journal in which it appears.]

This institution is attached to the university, being expressly intended by the Prussian government for clinical instruction; and has recently, it appears, been erected in a more convenient part of the town, being close behind the building of the university itself. The patients are divided into two classes,—those who are delivered in the hospital, and those who are attended at their own houses. Prof. Busch is the successor of the celebrated Elias von Siebold, and has carefully recorded everything which has been worthy of interest, both in a statistical as well as practical point of view. We have only to complain that so much time has been allowed to elapse before publishing a Report, which ought, at the longest, to have been every two years. The quantity of matter collected would not have been so bulky. In the present instance it is much too unwieldy for insertion in a Journal, and would have appeared infinitely better in a separate work, like that of Dr. Collins. The following is a condensed summary of the general view which Prof. Busch has given of his practice for the above period, and our readers will derive interest in comparing the results with those of Dr. Collins, as given in our Third Number.

Total number of labours	2056	Boys	1061
Single births	2035	Girls	1000
Twin births	21	Sex not ascertained	16
Total number of children and fetuses,	2077	Women delivered alive	2054
Mature and premature children	45	after death	2
Abortions	32	Primiparæ	992
Multiparæ	1064	Forceps cases	178
Patients recovered	2016	Extraction	55
Patients died	38	Turning effected by altering the patient's position	4
Children born alive	1913	Turning with the head foremost	4
dead	132	nates foremost	2
Children which died during the first three weeks	92	feet foremost	57
Presentations of the head	1911	Accouchement forcé	5
face	18	Artificial premature delivery	3
nates	47	Perforation	6
knees	2	Embryotomy	2
feet	28	Cæsarean operation during life	1
Total number of unnatural presentations	54	after death	2
Presentations of the shoulder	43	Reposition of the prolapsed cord	3
back, breast, sides, &c.	11	Artificial extraction of the placenta	47
Presentation of the arm and head	86	Incision of the perineum	3
Labours without assistance	1711		

Among the remarkable phenomena occurring *during* pregnancy, we may notice that of menstruation. Prof. Busch observes that, in several cases, menstruation appeared once, twice, and even three times, after conception. The patients had all borne children before, and the circumstance appeared to have no peculiar effect upon the pregnancy. Two remarkable cases, however, occurred, where the catamenia did not appear *except during pregnancy*. The first was in the woman's second pregnancy: she stated that she had never menstruated until her first pregnancy, when the catamenia appeared and returned regularly during the whole time. She was delivered of a dead child in the eighth month, after which the catamenia ceased until she again became pregnant, two years afterwards: it now returned at regular intervals. When advanced four months, she applied at the hospital, fearing that the continuance of the discharge would have an injurious effect upon the life of the child. In spite of bleeding just before the expected recurrence of the periods, horizontal posture, &c., it continued to appear until the seventh month, when it stopped. Labour came on a few weeks before the time, and she was delivered with ease of a small living child.

Retroversion. A well-marked case of this displacement is given, which occurred in a multipara in the fourth month, after lifting a heavy weight. Violent pain of the loins, with obstinate constipation and retention of urine, followed. The distended bladder reached above the navel, the os uteri was high up behind the symphysis pubis, the fundus deep in the hollow of the sacrum. Six pints of urine were drawn off, and the uterus was easily restored to its natural position: abortion followed ten days afterwards. Prof. Busch states that, according to his large experience, evacuation of the bladder is usually sufficient, and that, where the uterus will not return, if an elastic catheter be left in the bladder for a short time, the uterus will either return spontaneously, or at any rate admit of being easily reduced. This by no means agrees with the experience of the English and American practitioners, who have shewn, in a great number of cases, that the uterus may be so impacted in the pelvis as to be incapable of reposition, although the bladder be regularly and completely evacuated.

Protracted Labour occurred in several instances: in upwards of thirty cases it lasted three days, and mostly without injury to the mother and child. Where no peculiar indication existed for the interference of medical or operative assistance, the patient was recommended to keep quiet, and the case was left to the natural powers.

Weak Labour-pains. The unripe *Secale cornutum*, which had not been kept above a year, was given in 175 cases of this sort, but never until the os uteri was fully dilated. The efficiency of this remedy was so distinctly marked, that in by far the majority of cases the labour was terminated without assistance: in 115 cases its action was effective and speedy; the aid of the forceps was required in 27, of turning in 6, and in 7 cases the body required extraction after the head had been expelled; in twenty cases it appeared to have no effect. In several cases of inertia uteri after delivery of the child, with retention of the placenta or hemorrhage, it was given with good effect. It was always given in the form of powder, in doses of ten grains, repeated every ten or fifteen minutes. In the 175 cases where it was given, 177 children were born; 142 were born alive, 18 in a state of asphyxia (from which they recovered), and 17 were born dead; of which 7 had evidently been dead some time previously, and of the 10 others, which died during labour, 2 lost their life by turning, 1 from presentation of the nates, 2 from prolapsus of the cord, 1 from contracted pelvis and long-continued impaction of the head in it, 1 from the protracted dilatation of the external parts, 1 from a peculiar deformity of all the extremities, and 1 from no very peculiar cause; so that, out of 177 children, only one death occurred which could be possibly attributed to the use of the ergot.

Convulsions. Eleven cases occurred: five were of the mild form, and six of the genuine and mostly fatal eclampsia: all were primiparae. Four died.

Varicose Veins of the Labia and Vagina were observed in several cases, in various degrees: in some the swelling was as big as a large fist, but labour passed over without any hinderance, except in two cases where the tumour burst. In one

no hemorrhage took place, but the blood infiltrated itself into the cellular tissue of the left labium, and distended it immensely: an incision was made to discharge the collected blood and allow the head to pass, and the wound healed in fourteen days. In the other case, the varix was on the anterior wall of the vagina, and burst, producing a smart hemorrhage: this was restrained by a cloth dipped in vinegar, and the labour terminated favorably.

Placenta prævia occurred in thirteen cases, varying considerably in degree, and requiring different modes of treatment. Two cases proved fatal to the mother, where there had been great delay in sending for assistance. In eight cases the child presented with the head, and in five with the shoulder.

Hemorrhage followed the birth of the child in 124 cases, only two of which proved fatal; and in these assistance had not been sent for until they had become quite hopeless. In speaking of those hemorrhages which arose from inertia uteri, where the placenta is still in the uterus, Prof. Busch recommends introducing the hand for its removal. The practice is very general in this country, but has been very little adopted upon the continent; and the reasons which he assigns are precisely those which have been given by the English authors. Prof. B. strongly opposes any officious interference in the expulsion of the after-birth: with this we cordially agree; but must differ from him most completely where he recommends it to be left entirely to the natural means of expulsion *for the first few hours*, when no unusually firm adhesions or other abnormal circumstances exist.

Deformity from Rickets. Prof. B.'s experience tends to confirm the fact that considerable distortion of the spine or thorax is rarely accompanied with deformed pelvis.

Five cases of extreme spinal deformity occurred: all were delivered of living children, and without any assistance, except the last, where the forceps was required. This was an extraordinary case of deformity: besides an extreme degree of spinal distortion from rickets in early life, the upper and lower extremities were so completely crippled and paralysed that the patient could neither walk nor stand, but was only able to lie down or sit. The pelvis was not very spacious, and so greatly inclined that the axis of it was horizontal; the child nevertheless presented with the head. It was no easy matter to apply the forceps, on account of the degree of pelvic inclination, the outlet being directed quite backwards: the operation, however, succeeded, and a living child was extracted. Three cases of an unusual species of deformed pelvis were observed, viz. the *pelvis aequaliter justo minor*, or pelvis equally too small in every direction: it has been scarcely, if at all, noticed in this country, and certainly is not of very frequent occurrence to any considerable extent. The first case was a presentation of the breech: the head was delivered by the forceps; the child was dead; the pelvis measured half an inch too small in every direction. In the second case, which was a head presentation, the delivery was effected by the forceps, but not without the greatest efforts: the child was stillborn, and the mother died in a day or two after from peritoneal inflammation. The third case required perforation: this also terminated fatally, the forceps having been previously applied, and considerable efforts made without success. On examination after death, every diameter of the pelvis was three-quarters of an inch smaller than usual: in appearance it resembled that of a child.

Artificial Premature Labour was induced in only three cases: we will briefly state the particulars of one. The conjugate diameter was only three and a quarter inches: it was determined to open the os uteri by the dilator. The bowels having been well opened on the preceding day, and the abdomen rubbed with some warm almond-oil, some infus. chamæm. and oil were thrown into the vagina, and the dilator, properly warmed and oiled, introduced about three-quarters of an inch into the canal of the cervix. Five gentle efforts at dilatation were made, producing no pain, but merely a sense of tension: this was repeated noon and evening, premising each time the injection; and she was put upon an antiphlogistic diet. The following day, the mucous secretion from the vagina had increased; its parietes were warm and more cushiony: the dilating efforts were repeated three times, and to a greater extent. On the third day, the os uteri was open about the size of a

shilling; the pains were tolerably good, but went off towards night, and the dilator was used again on the following day. Slight febrile symptoms appeared on the fifth day, and the dilator was intermitted until the sixth. The pains returned during the following night; the os uteri became fully dilated, the vagina was relaxed and soft, the bladder of membranes distended; but, as no presenting part could be felt, a faulty presentation of the child was surmised. It was determined to bring down the head, if possible: the patient was therefore placed upon her back, the right hand passed into the vagina, the membranes ruptured, and the head, which was resting against the left ilium, was guided into the brim of the pelvis. An immense quantity of liquor amnii was discharged; the head entered the brim in the first position; active pains quickly followed, and a living child was born in about a quarter of an hour after the rupture of the membranes. It weighed only two and a quarter pounds. The mother did well; but no mention is made of the child, which, from its diminutive size, in all probability died.

Anchylosis in the Extremities of a Fœtus occurred as a very unusual obstruction to labour. The head was delivered by the forceps, but the body would not follow. As no cause of obstruction could be discovered, a gentle, and then more powerful, traction was used: this was followed by a cracking sound, and the upper part of the trunk passed through the os externum: here, again, it stopped, but still, as no cause of obstruction could be discovered, and as the child was dead, another traction was made, with a repetition of the cracking sound, and the child was delivered. On examination, it was found that all the joints of the extremities were anchylosed in the usual position of the fœtus in utero; so that the ossa humeri, and then the ossa femoris, had given way. The child had been dead some time.

Vagitus Uterinus. A well-marked case of this singular phenomenon occurred in a shoulder presentation. The child was afterwards turned, and delivered in a state of asphyxia, from which it recovered. Professor Busch observes, that he has met with several cases of this sort, where the shoulder had presented and the liquor amnii escaped.

The Cord round the Child's Neck, in two cases, was drawn so tightly that a deep groove was produced, and yet without injury to the child's life. He very properly directs that the cord should be passed over the shoulders.

Length of the Cord. The longest cord measured forty-six, the shortest six, inches.

Insertion of the cord into the membranes was observed in ten cases. In four, the point of insertion was at the opposite side of the ovum to where the placenta was situated. In one case, there were three arteries and one vein which entered at opposite sides of the ovum. In an experience of thirty years, Professor B. has never observed any injury of the vessels in these cases where the membranes were ruptured.

Turning with the Feet foremost was performed in forty-four cases, with the highly favorable result that only three children lost their lives from the effects of the operation. Professor Busch ascribes this unusually small mortality among the children thus delivered to the circumstance of his instantly applying the forceps the moment the head experienced any hinderance when passing through the pelvis. The fact is interesting; and certainly the proportion of deaths is by far the smallest we have known of. In cases where turning was unusually difficult, he invariably premised a dose of opium, threw up an oleaginous injection into the vagina, and put the patient on her left side. He has given an interesting case where the shoulder presented in the same patient in six successive labours; but no attempt was made to ascertain the cause on which this peculiarity depended.

In giving directions as to the position in which the patient should be placed for turning, he says "the patient should be placed on the same side as the hand which the practitioner is about to use for turning the child." It is on the same principle that many accoucheurs in this country find it more convenient to use the left hand in turning, the patient being mostly placed upon the left side; and there is no doubt

that, where the feet are high up and forwards, we can thus reach them more easily.

Central Laceration of the Perineum occurred in a case where the delivery was effected by the forceps. As the head was passing, the perineum was felt by the supporting hand to tear slowly asunder, in spite of the greatest care, leaving the frænulum uninjured, forming, as it were, a bridge across the anterior portion of the opening. The patient was kept upon her side; Goulard lotion was constantly applied, and the opening contracted so rapidly, that on the third day it would barely admit the finger, and in four weeks it was quite cicatrized. We have ourselves observed a similar case, where the child passed *through* the perineum, but where a minute orifice remained, which did not give way and dilate in a subsequent confinement. We cannot, however, quite agree with the Professor that this is the most favorable species of perineal laceration: the prognosis must, of course, depend upon the extent of the laceration, and especially whether it extends into the rectum or not.

Perforation was used in six cases, and under circumstances which we cannot but deprecate. The forceps had been previously applied, and without success, in each case: to the injuries thus produced must the deaths of two patients be in a great measure attributed.

The Cæsarean Operation was performed three times: once on a living woman, on account of deformed pelvis: the patient died, but the child was preserved. The two other cases were after sudden death, in neither of which was the child saved.

Puerperal Fever shewed itself in thirty cases, although not appearing in an epidemic form. The prevailing character of the disease was inflammatory, requiring local and general abstraction of blood and the exhibition of calomel and antimony. Of these thirty cases, seven proved fatal: extensive effusions of sero-purulent fluid were found in the abdominal cavity. In one case only were there any marks of inflammation and purulent effusion in the veins. In every case the disease first shewed itself between the second and fourth day.

Phlegmasia Dolens occurred in five cases, of which one proved fatal: with the exception of this last, the disease did not appear till the seventh day. In every case the pain was very intense; the swelling commenced in the upper part of the thigh, and spread rapidly either over the whole extremity, or, at least, as far as the leg, extending also sometimes to the groins. Professor B. considers that these five cases belong to what he calls inflammation of the cellular tissue, no inflammation of the veins having been observed.

We subjoin a list of the fatal cases:

Hæmorrhage from placenta prævia	2
after birth of the child	2
Exhaustion after turning, which had been delayed	1
Hæmorrhage in the puerperal state	1
Metritis and gangrene	3
Putrescence of the uterus and phlebitis	4
Phlegmasia dolens	1
Eclampsia in partu	4
After the Cæsarean operation	1
Puerperal fever	7
Typhoid fever	3
Abdominal typhoid fever	2
Apoplexy	1
Mania apoplectica	1
Gangrene of the lungs after pneumonia	1
Phtisis	4
Total	38

Account of a Case of Ruptured Uterus. By Dr. NÆGELÉ, JUN.

[THE following case, from the extraordinary nature of the circumstances connected with it, we believe to be quite unprecedented.]

A healthy peasant woman, æt. 35, of middle size, and delicately formed, mother of four children, and in the latter half of her pregnancy, (she reckoned that she had still six weeks to go,) received a violent blow on the abdomen from the pole of a waggon. She felt at the moment a severe tearing pain, and was thrown down with considerable force, but managed to creep a little way from the waggon, where her husband found her, and conveyed her home to bed in a fainting state. On coming a little to herself, she complained of a constant bearing-down pain in the abdomen, which prevented sleep during the night; but she had no return of the fainting. Besides the above symptoms, Dr. N. found her with a flushed face; pulse ninety, full and sharp; the bowels had been only once moved after several injections; she could pass water, but it produced pain and scalding. The abdomen was slightly tympanitic, but not tense. Between the umbilicus and pubes, somewhat to the left, a round circumscribed tumour was to be felt, rising in the middle to the height of an inch and a half, but flattened off towards the sides, and about six inches in diameter. The parietes were unchanged, except that there was a small circular bruise. Beneath the integuments covering this tumour, the nates and foot of a fœtus could be felt with the greatest distinctness, over which they were easily moved. She complained of pain on motion or on pressing the tumour. On pressing his hand deeper into the abdominal parietes above the navel, Dr. N. fancied he could feel the fundus uteri. Examination per vaginam presented nothing unusual. The head presented naturally, and neither blood nor water had come away from the vagina; she had no pains, nor were there any signs of incipient labour. The movements of the child, which had been very brisk before the accident, had ceased immediately after; and this was moreover confirmed by auscultation. The alarming symptoms which are usually observed in cases of ruptured uterus were entirely absent: nevertheless, the nature of the accident, the peculiar character of the above-mentioned swelling, and the distinctness with which the parts of the child could be felt immediately beneath the abdominal parietes, were strongly in proof of the uterus having been ruptured.

Under the existing circumstances, Dr. Nægélé considered that neither gastrotomy nor artificial delivery *per vias naturales* were indicated; as, in the first case, the child was evidently dead, and, in the second, there was not even a trace of commencing labour: he therefore contented himself with bleeding her to fourteen ounces, on account of the pain of head and state of the pulse, enjoined strict quiet, and requested the midwife (who was the only medical attendant in the village,) to send for him as soon as labour made its appearance. On the following day, the report was that no change had taken place, except that a cough, to which she had been many years subject, had increased, and aggravated the pain of the abdomen. Some dec. alth. and ext. hyosc. were ordered, and several days passed without hearing of her. "On the 5th of July," says Dr. N., "a messenger at length arrived at midnight, with the information that pains had come on the day before, in the forenoon, and that she had been partly delivered of a putrid fœtus in the afternoon; but, as it was not entirely expelled, my assistance was required. I arrived at four in the morning, and learned from the midwife that the os uteri had gradually dilated, the head had descended lower into the pelvis, and had at length been expelled; but, strange to say, no bladder of membranes had formed, nor had a drop of liquor amnii come away during the labour; that, when the fœtus was delivered as far as the breast, it had there stuck, and, on her pulling at the body, it had given way in the vicinity of the lumbar vertebræ, leaving the rest behind. The placenta had passed of itself, without any serious discharge. On examining the portion of the child which had been expelled, and which was in a high state of putrefaction; I found everything as she had described. The unfortunate patient lay without motion in her bed, with well-marked signs of metritis: the face betrayed the peculiar and painful expression of suffering which is so constantly observed in severe uterine disease; the anxious countenance; confusion of mind; the pulse from 110 to 120, somewhat hard and tense; the skin dry and rough; the temperature of the abdomen increased, but the extremities cool; tongue dry, thirst and nausea; the bowels had

been relieved by an enema, and she had passed water; the breasts were flaccid, and there were no traces of lochia. The abdomen was more distended than before, but the tumour itself had diminished; the parts of the child were less evident, but the nates and a foot could still be distinguished. The slightest touch produced insupportable suffering." On passing his whole hand into the vagina, the temperature of which was much increased, Dr. N. found the os uteri externum very high up and directed backwards: it was sufficiently open to admit two fingers. A leg of the child projected into the vagina, round which, at the knee-joint, the os uteri internum was firmly contracted. Repeated attempts were made to extract the foot, without success; for, besides the firmness with which the os uteri was contracted, the putrid state of the extremity prevented any firm hold being applied, and only produced the severest sufferings. In the hopes of relaxing the os uteri, she was bled to sixteen ounces, but without effect: the leg was therefore removed at the knee, warm fomentations were applied to the abdomen, strict quiet enjoined, with general antiphlogistic treatment and attention to the bowels.

No further news of the patient was received until the 10th of July, when Dr. N. was informed that she had been in severe suffering for two days previously; that the tumour had inflamed, and burst on the 8th; that a foot had protruded, at which the midwife had pulled until the whole remaining portion had been removed, and that the opening was very large.

Dr. N. saw her again on the 14th. The portion of the child which had been expelled from the abdominal tumour was examined, and was found to consist of the pelvis, with the entire lower extremity of one side and the thigh of the other. "To our astonishment," says Dr. Nægelé, "we found the patient, who, at our last visit, we thought could scarcely survive twenty-four hours, in a very comfortable state. There was but little fever, the pulse was about ninety, thirst moderate, appetite tolerably good, strength improved; her only complaint was that she did not sleep; there was no trace of lochia, and but little milk in the breast. In the place of the abdominal tumour was a round circumscribed opening in the fleshy wall of the abdomen, of about five inches in diameter, from which a quantity of mucus was discharged, and about an inch and a half in depth. On the right, the whole thickness of the abdominal wall was as if it had been cut through with a knife, beneath which the finger could be introduced for some extent: on the left side of the opening was a red globular mass, which had united with the abdominal integuments, and which appeared to be the uterus, as it rose distinctly when the lower portion of the uterus was pressed upwards by the finger per vaginam. Inferiorly, the finger was stopped by the adhesions which had formed in all directions at the bottom of the abscess; on the right side, a portion of intestine was visible. The wound was covered with simple dressing, and above this with a poultice; bark was ordered, with a mild but nourishing diet, and directions were given for ensuring proper relief in the bowels.

On the 19th July, the opening had diminished to a third; the portion of intestine, as also the red coloured mass, had retracted somewhat; there was a free discharge of healthy pus; she was perfectly free from suffering, except that one of the inguinal glands had inflamed and suppurated, causing her a good deal of pain. The simple treatment, as above mentioned, was continued, and a poultice applied to the groin; a more nourishing diet was also directed to be used. The abscess burst in due time, the strength continued to improve, and, when he visited her on the 17th of August, Dr. N. found the opening of the abdomen entirely closed, leaving merely a small scar.

Her health and strength continued to improve, and the catamenia returned on the 12th of August.

Neue Zeitschrift für Geburtskunde. Vol. v.

Resuscitation of an Infant after Interment. By Dr. WAGNER.

[THE following curious case, which, we are assured by Dr. Wagner, rests on unquestionable authority, occurred last summer in Germany, and became the subject of a criminal investigation: it is interesting in many points of view, but especially in relation to medical jurisprudence. We content ourselves with subjoining to the case the pertinent but brief remarks given by the narrator.]

An unmarried servant-maid became pregnant, and, although herself aware of the fact, persisted in denying it, and took every pains to conceal it. At length she was

seized with labour-pains, in the day-time, and was obliged to give up work. She complained of severe pain in the belly, which she attributed to cold, and persisted, even afterwards, that she so thought at the time, not believing the period of her delivery arrived. She slept in the same room with two other maid-servants; and, on the night in question, as she stated on her examination, the pains having increased, she got up, towards morning, in order, as she thought, to relieve her bowels. For this purpose she went to a wooden tub in the room, and, as she was sitting down, the child passed rapidly from her into the empty vessel. It was only then, she said, that she became aware of the nature of her pains. She did not closely examine the child, but was certain that it neither moved nor cried. The cord was, no doubt, torn at the time, and she made no attempt to tie it. Regarding the event as a miscarriage, she took up the tub with its contents, carried it to a sandpit about thirty paces distant, and threw the child into a hole in the sand, which she found already made. She covered the place up with sand and turf, and pressed the sand firmly in with the hand, in order, as she said, that the dogs might not reach the child. She returned to her bedroom, first calling up the man-servant at the stable door, and found her fellow-servants still sleeping. She awaked them; and feeling tired, sat down on a stool while they were getting up. One of the women, who had been awaked during the night by her groans, seeing some blood on the floor, and suspecting what had happened, asked her if she had not been confined, and had made away with her child. She replied "Do you take me for an old sow?" Having their doubts, however, the women followed the traces of blood, and came to the sand-pit, where they were struck by the appearance of the recently-disturbed ground. Fetching a spade, they discovered the child about a foot below the surface. On the access of the air, by the removal of the turf and sand, the child, still lying in the pit, began to cry, and was immediately taken up and carried to its mother, (from whom in the meantime the placenta had come away,) who washed it, and laid it on the bed. About an hour afterwards, the mother carried the child to her own mother in a neighbouring village; and then, for the first time, the navel string was tied. Another woman gave the infant the breast; and the mother, after being confined to bed for a few days, was delivered over to judicial examination. Her confession corresponded with the testimony of her witnesses, but she persisted in asserting that she considered the event as a miscarriage, and denied all intention of making away with the child. The investigation furnished no ground for disbelieving her assertion, although the child was proved, by medical testimony, to have been carried to the full term, and was in every respect healthy, except that it had a club-foot. From the evidence adduced, and the confession of the mother, the child must have remained at least a quarter of an hour under-ground; and, as it lay a foot below the surface, and as the sand and turf, according to the mother's confession, had been closely pressed upon it, no air could have readily reached it. It is hardly possible that the child could have breathed before interment; as, if the respiratory process had been once begun, the subsequent inhumation must have infallibly suffocated it. On the contrary, it is quite conceivable that the child was born asphyxiated, was buried in this state, and only then began to respire and assume independent vitality, when it was for the second time exposed to the air.

The above case is interesting in regard to the physiology of asphyxia, or apparent death. It is no less interesting in a medico-legal point of view, and in several respects. It has an important bearing on the doctrine of the hydrostatic test, and the objection made to it, that it indicates only the preexistence of life with respiration, not absolute life with or without respiration. The possibility of such a condition, and even of longer duration, is proved by this case; and it hence becomes a question in how far such an event is to be admitted in judgment, since the existence of this kind of vitality can, in general, be neither proved nor disproved. It is also worthy of notice that the navel-string remained for such a length of time untied without hemorrhage. The subject is, moreover, interesting in a purely judicial point of view, inasmuch as no penal law bears on the case; and yet we cannot here regard the conduct of the accused as not meriting punishment. She cannot be punished for intended infanticide, since all the evidence tends to exculpate her from this charge: she cannot be punished for concealing the pregnancy and birth, although proved, because, according to the law (of Germany) punishment does not ensue if the child is living; and no

mention is made of any penalty for the interment of a living but apparently dead-born child.

Medizinische Zeitung. January 17, 1838.

Observations on the Etiology of the Thrombus, or Bloody Tumour of the Head of the Fœtus in tedious Labours. By VICENTE JOSÉ DE CARVALHO, M.D. of Oporto.

THIS is a paper of considerable length, and displaying much research into the subject mentioned. The writer describes, with much candour but some prolixity, his successive changes of opinion. He first entertained, and then rejected, the view which assimilates these tumours to sanguineous swellings the effect of contusion; his rejection of it being founded on the obvious fact that the tumour is formed, not on the part of the head exposed to pressure and bruising from the irregularity of the bones of the pelvis, but on the vertex corresponding to the orifice of the uterus, where there is little or no pressure. He then adopted the idea that the swelling arose from the impediment to the return of the venous blood from the dependent and unpressed part, where thrombus forms in consequence of the circumference of the head being tightly embraced by the neck of the uterus; the tumour being thus assimilated to the condition of the lower part of a limb, round the upper part of which a tight ligature is bound. Some observations made in the dissection of the head of a fœtus, on which such a tumour existed, led the author again to modify his opinion. The tumour being opened, and the interior of its base exposed, the occipito-bregmatic circumference of the head being bound firmly with a cord, dark blood was seen to ooze from within the cranium, through the veins which penetrated the parietal foramina situated at the base of the thrombus. On drawing the ligature more tightly, there began to flow from within the cranium vermiform coagula of some inches in length, proceeding from the superior longitudinal sinus; which, on subsequent examination, was found empty.

The following are the conclusions which Sr. V. J. de Carvalho thinks himself justified in deducing from this anatomical fact and all the phenomena of the tumour:

1. The thrombus, or sanguineous tumour of the head of the fœtus, ought to be considered as a true *diverticulum* of cerebral blood, in which a portion of this liquid is accumulated on each occasion that the head is delayed for a considerable period in the pelvis.
2. On this occasion the encephalon and the head of the fœtus diminish absolutely in volume, which will be less in proportion to the quantity of blood received in a short time into the *diverticulum*.
3. The blood contained in a thrombus should be considered as a cerebral bleeding, which nature performs in lingering labours, to prevent the apoplexies, effusions, and other accidents to which the fœtus would be liable on such occasions.
4. The cavity of a thrombus represents that of a vessel hermetically sealed, where the blood is preserved liquid in a temperature equal to that of the part whence it has proceeded.
5. Nature performs a true transfusion with the blood of the same individual, when she causes that which she had preserved for some time in the *diverticulum* to re-enter the circulation.
6. The thrombus communicating directly with the superior longitudinal and lateral sinuses, the tumour should never be opened, on account of the risk of the introduction of air into these canals, which would be necessarily fatal.
7. The oblong figure which the thrombus presents occasionally, rendering more acute the pyramidal form which the head possesses in occipital presentations, contributes, in the manner of a wedge, to the dilatation of the neck of the uterus and of the vagina in the act of parturition.
8. As the compression of the body and head of the fœtus, conjointly with the dependent situation of this part, was the cause of the formation of the thrombus, so the cessation of the pressure on these parts and the altered position of the infant, assisted by gentle compression on the tumour, suffice for its dispersion in a few hours.

[The part of the author's paper which we think the most questionable is the assumption of the fluid condition of the blood within the tumour for a considerable period, and the reasoning founded upon it. This is certainly contrary to what is observed of blood when extravasated, even within the body.]

The patriotic author concludes his paper by expressing the hope that this new etiology of thrombus, conjoined with other national discoveries, will tend to elevate Portuguese obstetrics above the contempt of certain foreigners of whom he complains.]

Jornal de Sociedade das Sciencias Medicas de Lisboa, for Dec. 1836.

mention is made of any density for the treatment of a living but apparently dead-born child. *Medizinische Zeitung. January 17, 1838.*

PART FOURTH.

Selections from the British Journals.

(FOR THE QUARTER ENDING FEBRUARY 28, 1838.)

ANATOMY AND PHYSIOLOGY.

The History of a Female who has four Mammæ and Nipples. By ROBERT LEE, M.D. F.R.S. Read before the Royal Med. and Chir. Society, Jan. 9, 1838.

The individual in whom the above-mentioned peculiarity presented itself was thirty-five years of age, and was prematurely delivered of a stillborn child, on the 21st of July, 1835. The mammæ having afterwards become excessively painful and distended, she was compelled most reluctantly to permit the author to make an examination of them; by which it was discovered that she had two mammæ and two nipples on each side. The inferior or pectoral mammæ were fully developed, and in the natural situation; and their nipples, areolæ, and glands presented nothing unusual in their appearance. Near the anterior margin of the axilla, a little higher up on each side, was situated another mamma, about one-sixth the size of the others. The nipples of these were small and flat, but, when gently pressed, a milky fluid flowed copiously and readily from several ducts, which opened upon their extremities. When milk was drawn from the lower breasts, a small quantity usually escaped from the nipples of the upper; and, when the draught came into the former, the latter invariably became hard and distended. From the flatness of the nipples of the upper breasts, the patient had never been able to suckle with them.

Med. Gazette. January 20, 1838.

Remarks on the Fallacy of Professor TIEDEMANN'S Comparison of the Negro Brain and Intellect with those of the European. By ANDREW COMBE, M.D.

[As we assisted, by a short notice of Tiedemann's paper, (*British and Foreign Med. Review*, Vol. IV. p. 529,) to disseminate the singular errors which it contains, we deem it the more necessary to notice Dr. Combe's triumphant refutation of them. Dr. Combe's paper, however, like everything that proceeds from the pen of that acute observer and philosophical reasoner, is in itself highly important. If all the advocates of phrenology were possessed of the calmness and philosophical caution displayed by Dr. Combe, in his communications on its principles and phenomena, it would make more rapid progress in the estimation of philosophers and men of science. We take this opportunity of recommending to our readers the journal in which Dr. C.'s paper appears, as containing in every Number much interesting and important information.]

Tiedemann's grand objects are to prove, 1st, that the opinion of Negro inferiority, expressed by Camper, Soemmering, Cuvier, and almost all naturalists of any eminence, is incorrect; 2dly, that the Negro brain is equal in size, and similar in structure, to that of the European; and, 3dly, that, consequently, the former is equally capable of civilization as the latter, and owes his present inferiority entirely to bad treatment and unfavorable circumstances, and will lose it when placed in the position in society which has been recently assigned to him by the "noble British government." These positions are urged by Tiedemann with so much philanthropic warmth, and with such hearty zeal in the cause of the Negro, that

we feel no small reluctance to enter the lists against him; but, having a thorough reliance on the supremacy of truth, and believing its diffusion to be fraught with more ultimate happiness to the Negro himself than he can possibly derive from the propagation of an amiable error, we offer no apology for attempting to shew that the prevailing opinion remains unaffected by any evidence brought against it by Tiedemann, and that *de facto* the Negro brain is inferior in intellectual power to that of the European.

In pursuance of the above objects, Tiedemann first enquires whether "the Negro has the same quantity of brain as the European?" and, to ascertain the fact, he institutes an elaborate comparison between the weight of the brain, as determined in upwards of fifty Europeans of different ages and countries, and its weight in several Negroes, examined either by himself or others; and the results obtained are not only full of interest to the phrenologist, but well worthy of the attention of those among our opponents who still continue to ridicule the principle of size of brain being, *cæteris paribus*, a measure of mental power. Every fact mentioned by Tiedemann adds to the already overwhelming proofs adduced by the phrenologists; but, coming in this instance from the pen of a hostile authority, they may probably carry more weight with them than if found in a phrenological essay.

After quoting the statements of many authors, and detailing the weights of fifty-two European brains examined by himself, Tiedemann mentions that "the weight of the brain in an adult male European varies between 3lbs. 2 oz. and 4lbs. 6 oz. troy. *The brain of men who have distinguished themselves by their great talents is often very large:* the brain of the celebrated Cuvier weighed 4lbs. 11 oz. 4 dr. 30 gr. troy, and that of the celebrated surgeon Dupuytren weighed 4lbs. 10 oz. troy. *The brain of men endowed with but feeble intellectual powers is, on the contrary, often very small, particularly in congenital idiotismus.*" Here, then, is ample confirmation of the phrenological evidence, and from a source which cannot be considered as biassed in our favour. Tiedemann proceeds—"The female brain is lighter than that of the male: it varies between 2lbs. 8 oz. and 3lbs. 11 oz. *I never found a female brain that weighed 4lbs.* The female brain weighs, on an average, from four to eight ounces less than that of the male; *and this difference is already perceptible in a new-born child.* This also corresponds entirely with the long repudiated statements of the phrenologists, and it is pleasant to see the fact thus broadly admitted.

Tiedemann goes even beyond the phrenologists in his applications of the principles of size being a measure of power. He says, "*There is undoubtedly a very close connexion between the absolute size of the brain and the intellectual powers and functions of the mind.*" This is evident from the remarkable smallness of the brain in cases of congenital idiotismus, few much exceeding in weight the brain of a new-born child. Gall, Spurzheim, Haslam, Esquirol, and others, have already observed this, which is also confirmed by my own researches. The brain of very talented men is remarkable, on the other hand, for its size," (p. 502.) Here certainly is ample corroboration of the influence of organic size on mental power; but Tiedemann has fallen into the very serious error of taking the absolute size of the brain as a measure of intellectual power only; whereas, it indicates, as might be expected *à priori*, absolute mental power, without determining whether that power lies in extent of intellect, in strength of moral feeling, or in the force of passion or affection. A brain of four pounds' weight may be large in the anterior lobe and smaller in the middle and posterior lobes; or its chief size and weight may be in the posterior lobes, and the anterior portions be actually small. In both cases, Tiedemann would infer equal "intellectual" power; whereas, the phrenologist would perceive at a glance that, in the former, the intellectual ability would far preponderate, while, in the latter, the power of mind would consist entirely in intensity of feeling, and the intellect, properly so called, be rather weak than strong.

If, for example, we compare the Charib with the Hindoo brain, we find the entire mass of the former considerably to outweigh the latter; and, according to

Tiedemann, we should find more intellectual talent in the Charib. The fact, however, is notoriously the reverse, and the explanation is very easy *when we distinguish the regions of the brain in which the size exists*. In the Charib, the anterior lobe is very small, in perfect harmony with his poverty of intellect; but the posterior and basilar regions of the brain are very large, also in harmony with his ferocity and energy of passion. In the Hindoo, again, the reverse holds: the anterior lobe is well developed, and so is his intellect; but the basilar region, so large in the Charib, is small in him, and, consequently, in vehemence of passion, active courage, and general force of character, the Hindoo is greatly inferior to the more savage Charib.

The same distinction occurs every day in social life. We meet with an individual (a criminal, for instance,) in whom the brain is absolutely large, but who is nevertheless stupid in intellect, and powerful only in the department of the propensities; while, on the contrary, we find many an amiable member of society possessed of a brain smaller in absolute size, but far superior to the criminal in the size of its anterior lobe, or organs of intellect, and consequently far superior to the criminal in thinking power and general talents; results at utter variance with Tiedemann's rule, but perfectly reconcilable according to the phrenological application of the principle.

Hence it is obvious that, of two brains, both precisely equal in absolute weight, one may be very deficient in intellectual endowment compared to the other, and this deficiency be perfectly apparent on inspection, when we attend to the region of the brain in which the preponderance lies. But as Tiedemann, throughout the whole of his experiments, utterly disregards this distinction, confounds intellectual power, moral feeling, and brute propensity under one head, and treats of the brain as if it consisted of only one lobe, with only one function,—namely, the manifestation of intellect,—his inference that, because the Negro brain is equal in weight to the European, therefore the Negro is also his equal in intellectual power, falls to the ground, as unwarranted by the evidence. To render his conclusion worth anything, he must shew, not only that the two brains are equal in absolute size, but that *the anterior lobe, or seat of intellect, is equally developed in both*; a position which he never attempts to substantiate, and which is at variance with some parts even of his own facts.

Having obtained the weight of a sufficient number of European brains, Tiedemann next endeavours to ascertain the weight of the Negro brain; but, from the very small quantity of Negroes to be found in Europe, he has great difficulty in obtaining anything like a fair average: in fact, he gives the weight of *only four* Negro brains: one of a boy of fourteen years of age, stated, on the authority of Soemmering, to have weighed 3 lbs. 6 oz. 6 dr.; a second, of a tall and handsome Negro, of twenty years of age, which weighed 3 lbs. 9 oz. 4 dr.; a third, of a large Negro, mentioned by Sir Astley Cooper, of 49 oz.; and a fourth, examined by himself, of a man twenty-five years of age, which weighed 2 lbs. 3 oz. 2 dr.

In comparing these results with the average weight of the European brain, as stated by Tiedemann himself, it is singular to observe the extent to which they are at variance with his inferences. The European average runs, he says, from 3 lbs. 2 oz. to 4 lbs. 6 oz., while the average of the four Negroes rises to only 3 lbs. 5 oz. 1 dr., or 3 oz. above the *lowest* European averages; and the *highest* Negro falls 5 oz. short of the highest *average* European, and no less than 10 oz. short of Cuvier's brain. And, as if these facts were not inconsistent enough with his conclusions, Tiedemann first affirms that in the Negro "the length and height of the cerebral hemispheres do not visibly differ from that of the European, their breadth only being somewhat less," (p. 515;) and immediately after subjoins three tables of the "Dimensions of the Cerebrum of Negroes," "Dimensions of the Cerebrum of European Males," and "Dimensions of the Cerebrum of European Females;" the figures of which directly contradict his assertion! This seems almost incredible; but, on summing up the averages, we find the following results, namely,

		Inches.	Lines
Average length of brain in	4 Negroes	5	11
	7 European males	6	2½
	6 females	5	10½
Average greatest breadth in	4 Negroes	4	8½
	7 European males	5	1½
	3 females	5	4½
Average height of brain in	3 Negroes	2	11½
	7 European males	3	4
	4 females	2	9½

From these tables it is evident that the dimensions of the brain are smaller in the Negroes measured by Tiedemann than in the European; but, for our own parts, we are not disposed to lay much stress upon results drawn from such a limited number of facts, and we notice them merely to shew that, such as they are, they directly contradict the arithmetical proportions or conclusions drawn from them by Tiedemann. The latter, indeed, grants all that we contend for, when, in his description of the Negro brain, (p. 515,) he states that "*the anterior portion of the hemispheres is something narrower than is usually the case in Europeans,*" because, as the anterior portion is the seat of *intellect*, this is really equivalent to conceding that the Negro is naturally inferior in intellectual capacity to the European.

Not having access to a sufficient number of the actual brains of Negroes, Tiedemann has endeavoured to supply the want of direct evidence by comparing the capacity of the Negro skull with that of the European, and thus obtaining an index to the relative size of their contained organs. For this purpose he filled the skulls with millet-seed, and carefully noted the quantity which each contained."

After giving several pages of tables comprehending the weight of the quantity of millet-seed required to fill Ethiopian, Caucasian, Mongolian, American, and Malayan skulls, Tiedemann says, "It is evident, from the comparison of the *cavum cranii* of the Negro with that of the European, Mongolian, American, and Malayan, that the cavity of the skull of the Negro, in general, *is not smaller* than that of the European and other human races. The result of Hamilton's researches is the same. I hope this will convince others that the opinion of many naturalists, such as Camper, Soemmering, Cuvier, Lawrence, and Virey, that the Negro has a smaller skull and brain than the European, is *ill-founded, and entirely refuted by my researches.*" (P. 511.) Now, we have already seen that the real question of interest, as regards Negro improvement, is not so much the general size of his brain as the relative size of its anterior lobe and coronal surface compared to the basilar and posterior portions; but, even as concerns the absolute size of the whole brain, it is an extraordinary fact that Tiedemann's own tables give a decided superiority to the European over the Negro, to the average extent of nearly four ounces! The average capacity of forty-one Negro skulls in his own tables amounts only to 37 oz. 1 dr. 10 gr., while the average of seventy-seven Europeans of every nation, also in his own tables, amounts to 41 oz. 2 dr. 30 gr. Of the Negroes, indeed, three are females; but, even subtracting these, the Negro average amounts only to 37 oz. 6 dr. 18 gr. Here, then, on Tiedemann's own shewing, we have, first, an inferiority in the dimensions of the Negro brain and a greater narrowness of its anterior lobe; and, secondly, a marked inferiority in the capacity of the Negro skull to the extent of about one-tenth, and yet he very strangely infers that *both are equal* to those of the European.

That a physiologist of Tiedemann's talent and merited reputation should have failed so signally in an investigation which he recognizes as one of so much importance, and upon which he has bestowed so much labour, and with so benevolent an intention, is much to be lamented; but the cause which has led to his failure is still more to be lamented, because it is humiliating to him as a man of science, and is the natural and just result of his own conduct. Well did Tiedemann know that the great discovery of his immortal countryman, Gall, lay directly in his way in

the enquiry in which he was engaged; and that, if true, it must be of immense use to him in conducting his enquiry. Had he availed himself of its aid, he would have seen at once the futility of any investigation based on considering the whole brain as the organ of *intellect*, and would thus have avoided becoming the instrument of authoritatively diffusing mischievous error, where he was anxious only for beneficent truth. Tiedemann, however, confiding in the strength of his own merits and the durability of his own fame, chose to treat the phrenological physiology of the brain with contemptuous silence, to disregard its facts, and to reject its aid as a guide. He has preferred being a leader in the train of error to being a subordinate in the march of truth; and, as he has chosen his path, so shall he be rewarded. His contribution to the Royal Society's Transactions, although hailed at present as an honour to its author, will ere long be regarded as a beacon to warn others how very little a first-rate talent, great industry, and a European reputation can accomplish when employed in a false direction, and how indispensable to true greatness is the direct and undeviating pursuit of truth.

Phrenological Journal. No. liv. Dec. 1837.

Second Report of the London Committee of the British Association for the Advancement of Science, on the Motions and Sounds of the Heart. Communicated at the Meeting at Liverpool, September 1837.

THE Committee appointed in London to investigate the motions and sounds of the heart, have to present to the Association a short account of some investigations of the abnormal sounds of the heart and arteries, in which they have been recently occupied, and which were not comprehended in their former Report.

Before describing these, the Committee would remark, that although their enquiries have not been specially directed to that subject, yet they have had many opportunities of verifying the conclusions on the natural sounds, as presented in their Report of last year; and these conclusions not having been since shaken by any experiment or rational objection, it may be considered as fairly established, that the first, or systolic sound of the heart, is essentially caused by the sudden and forcible tightening of the muscular fibres of the ventricles when they contract; and that the second sound, which accompanies the diastole of the ventricles, depends solely on the reaction of the arterial columns of blood on the semilunar valves at the arterial orifices. It further appears, that the first sound may be increased by an additional sound of impulsion against the walls of the chest under certain circumstances of posture, of increased action of the heart, and of particular stages of the respiratory act. It is also obvious, that the character of the first sound may in some measure depend on the closure of the auriculo-ventricular valves, and on the quantity of blood, inasmuch as these determine the nature and time of the resistance, against which the muscular fibres of the ventricles tighten. So likewise the vigour of the ventricular systole, the quantity of blood propelled by it, the sudden and complete character of the diastole, the fulness of the arterial trunks, as well as the perfect, mobile, and membranous condition of the semilunar valves, will determine the character and loudness of the second sound. An experimental illustration of one of these conditions was observed by the Committee during one of their experiments on the ass—a great diminution of the second sound on the carotid artery being freely divided.

As additional illustrations of the production of a sound like that of the heart, by muscular contraction, the Committee have noticed that accompanying the action of the panniculus carnosus of the ass during life, and the quivering contraction of muscles immediately after death. The sound produced in the latter case, in nature and frequency, closely resembled the first sound of the heart of the fœtus or of small animals.

In investigating the morbid sounds of the heart, the attention of the Committee has been chiefly directed to the causes of those remarkable and various accompaniments of the heart's action, called murmurs, which were happily compared by Laennec with the noises of blowing, filing, rasping, sawing, purring, cooing, &c.

This enquiry consists of two parts. 1. What is the essential physical cause of the phenomena in question? and 2. How does the apparatus of the circulation develop this cause in the various instances in which these phenomena are known to occur? To the first of these enquiries, the experiments of the Committee supply what they trust will be deemed a satisfactory answer; the second is to be answered by extensive clinical and pathological observation, rather than by direct experiment; and although a few physiological experiments will be quoted to this point, yet the Committee do not profess at present to do more than open this enquiry to all those who have the means of pursuing it.

Experiments on the production of Sound by the motion of Water through a Tube.

1. A caoutchouc tube, eighteen inches long, and three-eighths of an inch in diameter, was attached to the stop-cock of a reservoir, in which there was water to the depth of from eight to ten inches.

When the water flowed perpendicularly through this tube (the air being first expelled,* and the lower end of the tube kept under water in the recipient below,) no sound was heard; but on pressing any part of the tube, so as to diminish its caliber, a blowing sound was heard at and below the point of pressure, and this sound became louder and more whizzing as the pressure was increased. The loudest sounds were obtained at the lowest end of the tube, when they were sometimes quite musical; and by increasing the pressure at regular intervals, a periodic increase and raising of the sound was produced, which closely resembled the sound heard in the neck, to which the French have given the name of "bruit-de-diable."

2. A pin being stuck transversely through the tube, a slight blowing was heard. A similar phenomenon more distinctly resulted from the use of a split goose-quill placed in the same way. A stronger blowing was produced by two threads across the diameter of the tube, especially when they were rather loose; and a still louder and shriller sound ensued when a knot of string was fastened to the threads.

3. The same tube being adapted to the stopcock of a water supply-pipe, through which the current could be let to pass with great force, it was found possible to imitate every variety of blowing, whizzing, and musical murmurs, by varying the pressure on, or obstruction in, the tube, and by altering the force of the current. When the current was strong, the least obstruction caused a murmur, but, with weaker currents, greater obstructions became necessary for the same effect. An obstruction which, with a weak current, gave a blowing sound, produced with a stronger current a sound of a more whizzing character. Grating or rasping sounds were best obtained by the effect of a strong current on a knotted thread across the diameter of the tube. The musical or uniform sounds resulted from a moderately strong current through a considerable impediment; increasing the force of the current or the degree of obstruction, rendered them whizzing and imperfect; diminishing the current or the obstruction, converted them into a simple blowing. When a sound was of an appreciable pitch, its note was high in proportion to the force of the current and the amount of the obstruction, a fine forcible stream producing the highest note. Sometimes, however, with a strong current, a loud trumpet-note would be set up, which was not altered in pitch, but only in force, by changing the strength of the current. This kind of note produced visible vibrations of the tube below the obstruction, and seemed to have relation to the length of the tube. In many instances these vibrations resembled closely the purring tremor and thrilling vibration sometimes felt in the region of the heart and large arteries. Musical sounds of a more variable character, like the cooing of a dove, the humming of an insect, or the whistling of wind, were produced with weak currents passing through a tube much obstructed. The pressure of a column of water only two or three inches high was sufficient to give acute whistling notes, which were sustained, although varying, even when the water that passed only fell in drops from the end of the tube.

4. Bending the tube to an angle produced a murmur; but no sound resulted from any curve that did not infringe on the caliber of the tube. A circular constriction, by a thread drawn round the tube, caused a murmur, which was blowing or whizzing according to the strength of the current.

5. The current issuing from the end of a tube, or from the mouth of an India-rubber bottle, produced a blowing sound when it impinged directly on an opposite surface, such as the side of the recipient or the end of the stethoscope, but unless the current were very strong, this sound was not produced when the current played on the surface very obliquely.

6. When the tube, with a weak current, was pressed on at two points, the murmur was heard at the point where the pressure was greatest; and by increasing the pressure at one point, the sound was stopped at the other. When the current was strong, it was easy, by a pretty equal pressure, to cause a murmur at both points.

7. With a strong caoutchouc tube, two feet long, and one inch in internal diameter, the same results were observed, but in a more remarkable degree, in consequence of the increased size of the tube. When the current was strong, and the pressure on the tube considerable, sounds were produced loud enough to be heard without applying the stethoscope to the ear, and the vibrations of the tube below the obstructions were so strong that they threw the water in little jets from the outside of the tube.

8. In making the last experiment, the pressure of the water suddenly distended a portion of the tube into a globe about three inches in diameter, constituting a good representation of a circumscribed true aneurism. As long as the force of the current was sufficient to keep the walls of the dilated portion distended, no murmur was produced; but when these walls became flaccid, the passing current caused a kind of dull fremitus in them. Slight pressure on the dilatation or bending the tube to form an angle at the point, also sometimes occasioned a murmur.

9. A globular India-rubber bottle, three inches in diameter, being adapted to an aperture in the side of a tube, so as to form an elastic sac communicating with it, and the air being expelled, the current of water was directed through the tube. The same was done with a smaller tube, and a bottle one inch and a half in diameter. In some positions the current, in passing the lateral sac, occasioned a slight whizzing; but in others, as when the tube was straight, there was no sound. A sudden increase of current, or the removal of pressure from the sac, caused a whizzing, by the entry of water into the sac. Independently of the current, sudden forcible pressure on the sac occasioned a whizzing with the expulsion of the fluid; and a similar whizzing attended its rapid reflux into the sac on the removal of the pressure.

10. Some of the preceding experiments being repeated with water made slightly glutinous with size, it was found that the various sounds were not quite so readily produced as with plain water, and required a greater force of current.

From all these results (1, 2, 3, 4, 5, 6, &c.) it is sufficiently plain that a certain resistance or impediment to a liquid current is the essential physical cause of all murmurs produced by the motion of fluids in elastic tubes. That any condition of the walls of the tube beyond the obstructing point is not, as it has been supposed, essential to the production of these sounds, is proved by the fact, that they may be produced by an obstruction at the terminal orifice of the tube, or at the mouth of a gum-elastic bottle, where there is no tube or wall beyond to cause them (1, 5); and usually this is the situation where they can be produced best, because here the current has acquired its greatest momentum, and finds a freer passage beyond the obstructed point. The more flaccid state of the portion of a tube beyond an obstruction is a necessary effect of the impediment caused by that obstruction to the passage of water. It is therefore a necessary concomitant of the obstruction, but it is not the cause of the sound. When, however, the sound occasioned by the obstruction is strong, its vibrations may be communicated to the whole contents and walls of the tube beyond (3, 7,) which will then vibrate in system with it, and be capable of modifying its note; just as the tube of a reed instrument affects the note which is generated exclusively in the reed. On the other hand, when the sound is weak and varying, the condition of the tube or walls beyond it will not affect it. In short, the laws of the production of sound by liquids so closely resemble those which regulate the same phenomena in air, that it is unnecessary to enter into fur-

ther detail respecting them. It may be necessary to advert to an objection to this view, that a murmur is sometimes caused where there is no impediment to the course of a liquid, as when it passes suddenly from a small into a large tube or into a sac. In the first place, it is not quite correct to assume that in this case there is no impediment; for the liquid in the large tube or sac, having less velocity than that in the small one, must itself be an impediment. But besides this, the course of the small swift current becomes changed by spreading into the larger channel, and instead of running smoothly parallel to the tube, now strikes its walls at an angle, causing a series of impulses and resistances which, if forcible and rapid enough, constitute the vibrations of sound. It is in the same way that a current produces a sound by impinging against an opposed surface (5.) It may be observed, however, that these indirect kinds of impediment to a moving current, are not so constantly attended with the production of sound, as the direct obstacle presented by a narrowing of, or projection into, the caliber of the tube.

Experiments on the production of Murmurs in the Living Body.

11. About two inches length of the common carotid artery of a young donkey was laid bare. Different degrees of pressure, either by the stethoscope, or by a probe pushed under the artery, occasioned a variety of murmurs, blowing, sawing, filing, and musical or cooing, at each pulse. When the stethoscope was merely placed in contact without pressure, no murmur was heard, but only a simple impulse and sound, which was distinct only when the heart acted strongly.

12. The artery was scratched for a few seconds with the point of a scalpel; it gradually became sensibly smaller for the length of half an inch about that point. A strong solution of salt being applied, the contraction increased; but it was still of a gradual and tapering kind, and the stethoscope could not detect any murmur in it; very slight pressure was enough to cause a whizzing. The pulse at this contracted portion was felt to be much harder and sharper than above or below it.

13. A small incision being made into the artery, a jet of blood issued, and a whizzing, sometimes continuous like the bruit-de-diable, sometimes only in pulses, was heard beyond the orifice, but no sound on that side of the orifice nearest the heart; the sound being, as usual, carried in the direction of the current. The incision being made larger, the blood spouted out with whizzing to the distance of more than six feet, and the animal died in the course of ten minutes. After this last incision the beats of the heart were frequent, short, and pretty loud, but without a second sound, and without any murmur to the last. They continued for nearly two minutes after the respiration and consciousness had ceased, becoming gradually slower.

14. The Committee repeated the observation that has often been made before, that a murmur can easily be produced by pressure on the subclavian, carotid, or femoral artery of the human subject. This murmur is generally of a grating or filing character, and is prolonged in proportion to the degree of pressure.

15. Whilst making the observations on the carotid, they found that a continuous murmur of very remarkable and variable characters could be produced by pressure on the jugular veins. The most common sound thus produced was like the humming of a gnat or fly, but occasionally it resembled the whistling of wind, the singing of a kettle, the cooing of a dove, and sometimes it was perfectly the remittent whirring noise, which the French have called the bruit-de-diable. Dr. Ogier Ward, of Birmingham, had previously come to the conclusion that this sound is produced in the jugular veins; and the observations of the Committee confirm this inference; but they do not agree with this physician in the opinion which he adopts from MM. Andral and Bouillaud, that the presence of these sounds always denotes a chlorotic state of the system, in which steel is indicated, or that they are essentially morbid symptoms at all. They may be produced in the healthiest subjects by moderate pressure applied to the lower part of the jugular veins, and are then found to be modified by various circumstances, which can only affect the venous current. Thus, they may be arrested or diminished by pressure on the vein above, by the

horizontal posture, or hanging down the head, and by forced efforts to expire with the glottis closed. They may be restored in increased degree by suddenly desisting from any of these acts or circumstances. The occasional pulsatory or remittent character of these sounds seems to depend on the momentary increase of pressure caused by each pulse of the neighbouring artery; and when, as it oftentimes happens, these pulses are attended with a whizzing, this is in a measure incorporated with the venous sound, and increases its periodic swell. The size and downward current of the jugular vein peculiarly adapt it for the production of sound, but it is probable that sounds may be produced by pressure on other veins, when circumstances accelerate the current within them. The Committee have succeeded in detecting an obscure murmur in some of the large superficial veins in the arm and thigh. It is to be distinguished from muscular sound, which it resembles, by its being heard only when the small end of the stethoscope is applied on a vein, and by its being stopped by pressure on that vein. A louder continuous sound is sometimes heard at each side of the upper part of the sternum, which, from its resemblance to these venous sounds, and from its being stopped by forcible expiration, may be supposed to have its seat in the large venous trunks underneath.

Although it appears from these facts that the venous sounds are not necessarily signs of disease, yet the circumstance proved by the Committee (10), that water is thrown into sonorous vibrations more readily than a liquid of a more glutinous character, renders it probable that these, and other sounds depending on the motion of liquids in the apparatus of the circulation, may be more easily produced where the blood is thin and deficient in quantity; and under these circumstances they may occur in the neck from the mere pressure of the muscles on the jugular veins.

The Committee had planned several experiments for the further elucidation of the second part of the enquiry, namely, by what changes, functional and structural, does the apparatus of the circulation develop the physical causes of abnormal murmurs and sounds in various instances in which they are known to occur? Having failed in obtaining animals in time for this Report, the Committee propose to resume at a future time this part of the enquiry, so important for the elucidation of several obscure points in pathology, diagnosis, and practice; and to report the result of their labours at the next meeting, if the Association should think fit to reappoint them for that purpose.*

(Signed)

C. J. B. WILLIAMS, M.D. F.R.S.,

ROBERT B. TODD, M.D., Professor of Physiology, and General
and Morbid Anatomy, King's College, London.

Med. Gazette. December 2, 1837.

On the Cerebral Extremity of the Optic Nerve. By SAMUEL SOLLY, Esq.

I AM desirous of putting upon record a fact regarding the cerebral extremity of the optic nerve, which I do not find noticed in any author on this subject. It may be interesting to many, inasmuch as it appears to afford additional evidence of the important office of the cineritious neurine; especially when taken in connexion with those views of the function of the grey matter which Mr. Grainger has lately so materially elucidated by his minute dissections of the central extremities of the spinal nerves, and the deductions which he makes in confirmation of Dr. Marshall Hall's views of the spinal cord as a true centre of power. I will also take this opportunity of bearing my testimony to the accuracy of Mr. Grainger's statement regarding the roots of the spinal nerves.

I traced fasciculi of both the anterior and posterior roots of the spinal nerves, into the grey matter of the cord, in presence of Dr. Todd, at King's College, on a dog which he had procured for that purpose; since which, I have seen them exposed by Mr. Grainger himself, at his school in Webb-street.

The connexion of the optic nerve with the brain itself has been variously described by different authors; by some it has been described as arising only from

* The Committee was reappointed, and 25*l.* placed at their disposal for the further prosecution of these researches.

the thalami nervorum opticorum; by others, only from the optic tubercles; and by most, in latter days, from both of these bodies, "by a flat band of white fibres." These fibres, which come from the surface of the thalamus and the optic tubercles, are well known and easily demonstrated; but they are not the only fibres of communication between this nerve and the brain.

If the optic nerve is carefully traced from its commissure backwards, it will be found to be connected to the tuber cinereum, as described elsewhere; after crossing the crus cerebri, to which it is connected with membrane, it divides into a superficial and deep layer: the superficial layer is that which is described in most anatomical works; the deep layer, which is thin and flat, plunges partly into the substance of the thalamus and partly into the corpus geniculatum. Those fibres which go through the corpus geniculatum are separated into delicate threads by cineritious neurine, as the motor tract of the spinal cord in the corpus striatum. These fibres which plunge into the thalamus are stronger and more distinct, and after spreading into rays are lost in its substance.

This arrangement may be seen after raising the nerve from the crus cerebri, either by tearing its fibres very carefully in a brain previously hardened in alcohol, or by making a longitudinal perpendicular section of the optic nerve in a recent brain, right through the corpus geniculatum and thalamus; when one layer of white neurine will be seen on the surface of the corpus geniculatum, and another just passing through its anterior part, but principally through the substance of the thalamus, separated from the first by the posterior and superior portion of the grey matter of the corpus geniculatum.

Med. Gazette. February 24, 1838.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Another instance of supernumerary Mammæ. By W. H. MOORE, Esq.,
Woodbridge.

ELIZA SNELL, a healthy-looking woman, aged twenty-three, in October last, being in the fifth month of pregnancy with her second child, observed a tumour in the right axilla, as large as a walnut, which gave her no pain, nor sensation of tenderness on pressure. As it did not produce any inconvenience she took no further notice of it until her admission into the poor-house of the Woodbridge Union, where she was placed for delivery; and in January last she shewed it to me. It had not enlarged since her first perceiving it. I found it moveable, not very hard, free from heat, pain, and tenderness, and ordered it to be poulticed. This was done for eight days, but the poultices seemed to create pain, without either softening or diminishing it. She was delivered early in the present month, and to-day my attention was again called to it by the nurse, who stated that there had been a discharge of milk from it. On examination I saw upon its surface eight or ten papillæ, with a minute pore at the apex of each, and an inflamed areola around its base. On pressing the tumour a white fluid issued in five distinct streams, from as many of the papillæ. These were checked on removing the pressure, but could be made to jet forth to a distance of seven or eight inches on resuming it. I collected nearly a teaspoonful of the fluid, which had just the colour, consistence, and sweetish taste of milk. There is a similar tumour in the axilla of the other side, with papillæ also, but I could force no milk from them; this had never been poulticed. There is nothing remarkable in the woman. The breasts are very abundantly supplied with milk.

Lancet. February 24, 1838.

On the Frequency of the Presence of the Trichocephalus Dispar in the Human Intestines. By O'B. BELLINGHAM, M.D., Second Surgeon to St. Vincent's Hospital, &c.

[THIS paper gives a very good account of this worm, which has been much more observed by foreign pathologists than by our countrymen. Dr. Bellingham, how-

ever, shews that it is extremely prevalent in this country, or at least in Dublin. The following passages are the most important in Dr. B.'s communication.]

This particular species (the *Trichocephalus Dispar*) was formerly called *trichuris* (derived from *τριχ*, a hair, and *ουρα*, a tail,) from the erroneous supposition that the capillary portion was the tail of the animal; it is from an inch and a half to two inches long, the capillary part composing about two-thirds of its length, at the extremity of which is its mouth. The male is smaller than the female, and the thicker part of its body is spirally twisted, in the female it is straight or nearly so; the penis of the male is a simple spiculum, contained in a sheath, the orifice of which is somewhat bell-shaped. The opening, which, in the female, serves the purpose both of vagina and anus, is small, and situated close to the termination of the posterior extremity.

The part of the intestinal canal which the *trichocephalus dispar* most commonly occupies is the cæcum, more particularly the neighbourhood of the ilio-cæcal valve; I have found them however through the whole tract of the colon, in the ileum close to the cæcum, and in the appendix vermiformis. We sometimes find the head of the animal implanted in the mucous membrane, lining the intestine; but this is rare; more commonly they are quite unattached, and when an opening is made into the cæcum, they come out in its fluid contents.

With a view to determine between the contradictory statements of the English and continental pathologists, I examined successively the intestinal canal of twenty-nine individuals who died in St. Vincent's Hospital during the last twelve months, (of whom eleven were males and eighteen females,) and in twenty-six out of the twenty-nine I found a greater or less number of these worms. The ages of these individuals varied much, the youngest being but eight years old, and the oldest upwards of seventy. The diseases which proved fatal to them also varied: some died of injuries, others of acute, and others of chronic diseases. In several instances I found but two or three *trichocephali*; in others upwards of eighty; the largest number having been found in a boy aged fourteen, who died of dropsy, with disease of heart and kidneys, in whom I counted one hundred and nineteen. In some instances I examined the proportion which the male *trichocephali* bore to the females; in one individual I found nineteen males and twenty-five females; in another sixty-one males and twenty-four females; in another one male and one female; in another four, all males; and in another six, all females. The three individuals in whom I failed in detecting these worms, were females; one died of scirrhus of the pylorus; and the communication between the stomach and duodenum, was so contracted as not to admit even a probe to pass, hence we may naturally suppose, that if they had previously existed they were starved out; another died of cancer, which commenced close to the orifice of the urethra; and the third died of protracted diarrhœa, with extensive ulceration of the mucous membrane of the cæcum, colon, and lower part of the ileum; she had been taking for some time before her death a combination of sulphas cupri with opium and other medicines, which most likely acted as poisons to these animals, and caused their expulsion. Not one of the twenty-six individuals in whom these worms existed had complained, either before or during the illness which proved fatal, of any symptom which could lead to the suspicion that these worms had been in the least degree prejudicial to their health.

Dublin Journal of Med. Science. Jan. 1838.

Enquiry as to whether Laryngismus Stridulus is a Spasmodic or Paralytic Affection. By WILLIAM GRIFFIN, M.D., Limerick.

[THIS is an elaborate critical enquiry, and also contains several new and interesting observations on the important subject treated of. We have noticed it so fully on more than one occasion, that we can only here extract Dr. Griffin's concluding paragraph, which places his modesty and candour in a favorable light.]

I have offered these few unsatisfactory observations without wishing to attach more importance to them than they merit. I believe both the pathology and treatment of the disease are still very uncertain, and that it will require all the consid-

ration and enquiry which observant practitioners can bestow upon the subject for many future years to attain a just knowledge of either. The following summary of the amount of our present information and of the facts connected with the disease, may be useful to subsequent enquirers:—1. By the concurrent testimony of almost all who have noticed the affection, it occurs for the most part, if not wholly, in strumous habits. 2. It is frequently found in connexion with enlarged glands in the neck, and perhaps in the thorax. 3. It is frequently found in connexion with eruptions on the face, ears, or scalp. 4. It frequently terminates in convulsions, and is sometimes, though very rarely, ushered in by them. I believe it may be said that nearly half the fatal cases on record terminated in convulsions. 5. It is met with in families in which children are subject to head affections or convulsions, but who have also the strumous disposition. 6. It is sometimes met with in connexion with an apoplectic or comatose state from the commencement, as in the cases of crowing apoplexy which I have described. 7. In a great proportion of the cases which terminated fatally there was not the least symptom of head affection through their whole course, if we do not look upon the occasional fits of breathlessness and crowing as indicative of it, and the children were as well, apparently, a few moments before death as they were previous to the first attack of the disease, or as any children could be. 8. The complaint is sometimes, but rarely, attended by cough and permanent difficulty of respiration. 9. Perhaps it may be said that from one-third to half of all the cases of which we have any account, terminated in death.

Dublin Journal of Med. Science. Jan. 1838.

Observations on the Use of the Stethoscope in the Practice of Midwifery. By DAVID C. NAGLE, A.B., M.B., one of the Physicians to the Dublin General Dispensary.

[THIS paper does not contain much novelty; but as it states briefly and clearly the chief facts of an important subject and gives some striking illustrations of its practical value, we recommend it to our readers. The following extracts give the more important observations.]

Auscultation affords but two signs of pregnancy: the pulsations of the foetal heart, and a murmur, that should, correctly speaking, be designated the uterine murmur. The first is absolutely unequivocal; the second equivocal; but, perhaps, the least so of those that should be considered as such. The pulsations of the foetal heart may be detected, at all events, between the fourth and fifth month, and vary from 120 to 180 in the minute; though I have found them, and that but momentarily, to sink as low as fifty or sixty. They remarkably resemble the ticking of a common watch, and may be detected in various parts of the abdomen, but generally in the iliac regions, particularly during the advanced periods of pregnancy. They are, though occasionally nearly masked by the uterine murmur in the parts, where that is most intense, easily distinguished by an experienced ear, especially if the cylinder be moved a little from the principal site of the murmur; or when the foetus takes a roll in the womb. They are sometimes liable to be confounded with the pulsations of the mother's heart; but the distinction is drawn with facility by attending to the rhythm, and gradually moving the stethoscope towards the region of the parent's heart. In no case will the beatings of a foetal heart, when at all energetic, escape detection, if the examination be properly conducted, and the ear of the auscultator be familiar with the rhythm and peculiarity of sound. But this familiarity is absolutely requisite, otherwise the examiner will be left in a state of perplexing uncertainty, where a practised ear would scarcely experience the slightest embarrassment. By an intimate acquaintance with the nature of this sound we can readily ascertain the life or death of the fetus in the womb; from which knowledge we can derive vastly important practical benefits. We shall also experience no great difficulty in detecting the existence of twins; and, what I have myself found of the utmost, the most gratifying, practical advantage, we can, in most cases, easily discover the nature of the presentation. The other phenomenon, the uterine murmur, is the first of the two stethoscopic signs that occurs; and may,

in vigorous constitutions, be detected about the third month. It is not in the least affected by the life or death of the fœtus in the womb; and is quite independent of the fœtal circulation. Its principal site is invariably in the lateral regions, along the course of the lateral uterine arteries; commencing, apparently, opposite the space between the anterior spinous processes, and closely to the iliac arteries. It should not be sought for in the anterior parts of the abdomen; as its occurrence there never happens, unless some branches of the lateral uterine arteries should extend across in an unusually enlarged state. This we can easily verify by moving the stethoscope gradually from the median line towards the ilium; for thus we shall find the murmur to increase in intensity, as we approach the latter. It is frequently detected in both iliac regions at the same time, though usually louder on one side than the other; and by repeating our examination, we shall often hear it on the side, where, in our previous enquiry, we could not, perhaps, discover a vestige of it. To this fact I would beg leave to invite the attention of those, who would persuade us that the placenta must be the seat of this important phenomenon.

Reflection will point out many ways, in which the knowledge, I am endeavouring to inculcate, might be made most usefully available. The following may be considered as no bad instance of it. An intelligent friend of mine was consulted by a female, not long in town, and then appearing to be affected—as she would fain persuade people to think—with some symptoms of dropsy. He suspected the nature of the case, but could get no vaginal examination. He wished me to examine this person, and had a friend of his present, who, I believe, had seen the patient before, but did not consider it a case of pregnancy. We met. It was with difficulty we prevailed on the patient to allow an examination with the stethoscope even. I instantly heard the pulsations of a fœtal heart, and unhesitatingly pronounced it a case of pregnancy. One of the physicians, though long in practice, was sceptical: I wished to remove his uncertainty by getting him to apply his ear to the cylinder. The pulsations of the fœtal heart were very energetic, and about 160 in a minute; whilst those of the mother were remarkably slow, not more than fifty. He could derive no information from the examination, as he was then unacquainted with the use of the instrument. No persuasion could induce the woman to admit the possibility of her being pregnant: but the extreme repugnance, with which she permitted even the most delicate stethoscopic examination, would be sufficient to create a suspicion that she was not conscious of her own innocence. I requested of my friend to inform her, that I, at least, had not the slightest doubt of her pregnancy; and I even predicted, what actually took place, that she would in a very few days retire into the country for the better concealment of her real state.

The practitioner in midwifery may infer from the case, with which I shall conclude this paper, what important service might be derived from an accurate acquaintance with the use of the stethoscope in that critical department of our profession. We are well aware that infants often appear to be still-born, and are laid by as such, though the labour seemed to proceed quite favorably, and the attendant physician had most carefully discharged his duty. This calamitous result may be occasioned by the pressure exerted on the head at the moment of its escape: or by other circumstances over which the physician could have no control. Auscultation, judiciously employed, would have shewn that, in many such cases, the fœtus had probably enjoyed vigorous existence a few moments previous to its birth; and thus the physician be encouraged to persevere in the use of efficient means for restoring suspended animation. How many valuable lives may be thus preserved! how gratifying to the conscientious physician must success in his professional exertions, under such circumstances, always be! and how simple are the means for the attainment of such encouraging information, may be thus satisfactorily illustrated!

On the 4th of October, 1835, I attended, in her confinement, Mrs. Cooney from Trinity College. The labour proceeded favorably, and the presentation was quite natural. The action of the fœtal heart, examined frequently, as is my custom, was ascertained to be perfectly regular, and retaining its energy undiminished; yet the child, when born, seemed absolutely lifeless; and all appearances, combined with an

intolerable fetor of the liquor amnii, which passed off as the head was emerging, were well calculated to leave on one's mind an impression, that the infant was dead for some time. Without the benefit of auscultation I should have been myself under a similar impression; but from the regularity and strength of the heart's action about ten minutes before delivery, when I last explored it, I concluded that the vital spark could scarcely have been quite extinct. No benefit appearing likely to accrue from allowing the cord to remain any longer undivided; and having rapidly provided for the mother's safety, I succeeded, after nearly an hour's unremitting, because not hopeless, perseverance, in restoring the infant to vigorous animation.

Dublin Journal of Med. Science. Jan. 1833.

Observations on Animal Magnetism. By Dr. SIGMOND, of London.

[WE give the following narrative without any comment. It will possess greater interest to many of our readers as coming from an eminent physician of our own country. We omit several passages of Dr. Sigmond's paper, and among others his attempt to explain the phenomena on common physiological principles.]

I entered the field of enquiry as a sceptic, and as such, after my enquiries, I remain, as to the belief that any individual is in possession of a power, save that which the strong mind exerts over the weak one, by which he can exercise a preternatural effect over the human frame. I totally disbelieve the existence of any fluid which can, at the will of an operator, be made to pass from his body into that of another, and thus, at his command, produce unwonted sensation.

I merely wish to state what I have observed, and to offer to shew that certain consequences result from a peculiar kind of manipulation, which may easily be acquired, and which, if practised with dexterity, in some instances, might be productive of considerable influence in different conditions of the body. Some weeks since the Baron du Potet de Sennevoy, did me the honour to invite me to be present at a trial of his magnetic power at the University College Hospital. I there saw him perform a series of actions upon different individuals, and he, in two instances, produced what may be termed artificial sleep upon two females, and this was the full extent of his success; his other attempts were failures. The successful cases, however, arrested my attention; they seemed to be the result of simple means, nor could there, at least I thought, be a doubt that the same power existed in any individual who chose to exert it.

I immediately determined to investigate the subject, and for that purpose tried a great number of experiments; but I was most unwilling, for a great length of time, to make my observations at all public, because I thought that I might be accused of seeking notoriety by investigating a subject which rather belonged to the community than to the profession, and one which seemed to be addressed to the popular feeling so easily excited, rather than to the calm and dispassionate consideration of the followers of science. Finding, however, that two distinguished members of the profession, Dr. Elliotson and Mr. Mayo, thought the subject worthy their attention, I persevered in my observations.

The extent of my examination has been such as to satisfy me that I can produce a sleep of a very unusual character, by certain manipulations which do not require me to be in actual contact with the person upon whom the operation is intended to be performed; that I have acquired a certain degree of experience, by which I know how to accommodate the manipulations for the purpose required; and that I can communicate to another individual, in a short space of time, all the information necessary for the production of this sleep.

I commenced my series of experiments by imitating the actions of the Baron Du Potet. My first subjects were of the uneducated class; but I found them so prone to believe in the marvellous—so anxious for extraordinary results, that they deceived both themselves and me. I have since tried the same manipulations upon the higher classes, and though I find them much more sensitive to every impression, and their nervous system more easily acted upon, and although, occasionally, the imagination has led some of them away, yet I have succeeded in giving a very

peculiar sleep, amounting almost to stupor, to a vast number of individuals. I have constantly found females much more susceptible of the influence than men; nor does it produce upon them all precisely the same state of sleep. For while in some it is a sort of trance, during which, as often occurs in that unnatural state, pain is scarcely felt, in others it produces hysteria, convulsions, and I have likewise known fainting occur. The most remarkable case that has fallen under my observation, and which, while it excited in me great anxiety, and the deepest interest, has taught me to prosecute my researches with extreme caution, has occurred to me within the last two days. I was enjoying the hospitality of a most amiable family in Fitzroy-square, when animal magnetism became the topic of conversation, and I related the trials I had already made. One of the young ladies proposed to become the subject of experiment, to which I very willingly assented; for, having on former occasions attended her during momentary sickness, I was fully aware of the natural strength of her constitution, and the absence of that nervous temperament which renders this system totally inapplicable. I began what are technically called "the passes." They, as is not unusual, excited laughter and incredulity. I proceeded for about five minutes, and then stopped and enquired if any sensation was produced, and the answer was, "a slight sleepiness;" and ridicule was again thrown upon the subject. I recommenced the manipulations; I observed the eyelids falling, and at last they closed; but, as the same incredulous smile remained, I persevered for three or four minutes, when I, almost doubting whether any influence had been produced, enquired what the feelings were; to this no answer was returned. I found my young friend was in the most complete trance I had ever yet witnessed as the result of my magnetism. The stupor was most profound; and I then tried the usual means to arouse her, but they were vainly exercised. After a few minutes I found the hands become icy cold, the face lost its natural hue, and became perfectly pallid; the extremities became quite cold; the respiration was imperceptible; the stimulus of light did not affect the eye; on speaking to her a faint smile was excited, and a quivering of the lower jaw, which seemed to indicate a wish but an incapability of answering; the pulse became gradually feebler, whilst the external appearance altogether bore such a decidedly deadly cast that naturally some apprehension was excited amongst her family, by whom she was surrounded. Of course I could not but feel a certain degree of anxiety and regret that I had produced such a state, and much uneasiness at the thought that I had inflicted a moment's alarm to my kind friends. These feelings were, however, less acute, from the full knowledge I entertained that the family had long reposed the most perfect confidence in me, and that no member of it had that nervous susceptibility, which would have embarrassed me had any untoward accident presented itself.

I placed the perfectly unconscious subject of this distressing scene in a horizontal position, and directed the application of warmth and of friction to the extremities. Circulation and animal heat were gradually excited, but she presented a most singular appearance of suspended animation. In this condition she remained more than four hours, for I had commenced a little after ten in the evening, and it was about half-past two, that, on some slight effort being made to rouse her, she uttered some of the most piercing shrieks I have ever heard; there were convulsive efforts to raise the limbs; the face, too, became convulsed; she opened her eyes and stared wildly around; she was placed in the upright posture, and seemed sensible. Advantage was taken of this circumstance to carry her to her apartment; before, however, she could reach it, she fell into a profound slumber, but its character was more natural. She was placed in her bed, appearing perfectly composed; the countenance had acquired its natural hue; the respiration was perfectly easy, and the pulse natural. In this state she remained during the whole of the day, until nine o'clock in the evening, once only opening her eyes, and addressing a few words to an anxious and affectionate sister who never left her side. In the evening the young lady joined her family perfectly restored to her wonted cheerfulness. She expressed no complaint whatever. She stated that the feelings that first came over her were those of extreme quiet and repose,—a species of ecstasy,—a gradual languor seemed to steal over her; that she heard something passing around her; felt

an inclination, but an utter impossibility, to reply. The first waking-up she, however, described as almost terrific. It was as if she was bursting from a narrow and confined space, and as if she arose from interminable darkness. The lesson that I have thus learnt will not be lost upon me.

I have now exercised this art upon nearly a hundred persons, and with very general success in the fairer part of creation; I have quieted delirium, and given sleep where it has been for many nights vainly solicited. I have magnetised in the presence of many medical men who have been in attendance on the Baron du Potet's lectures, and they have declared that the sleep appears identical with that he produces, and that it is proved by the fact that animals may be sent to sleep by the same movements. I am very anxious that the members of the profession should try the same process. *Lancet. December 9, 1837.*

Practical Observations on Delirium cum Tremore.

By WILLIAM MUNK, M.D., Lugd. Bat.

ALTHOUGH this paper contains no great novelty it is well worthy the reader's notice. It is always useful to have practical subjects fairly and sensibly discussed; particularly when, as in the case of this disease, there is one line of practice very successful in one class of cases but injurious in another. Its object is to shew "that delirium tremens results from very different causes, that it is frequently complicated with, or even caused by inflammatory affections, that in very many cases the disease is curable by antiphlogistic means only; and that in some of these instances narcotics and stimulants are not only unnecessary but at the same time injurious."

Dr. Munk considers the following statement as an approximation to the truth as to the numerical relation of the different classes of cases. "Delirium tremens," he says, "usually presents itself as a disease of debility, and is benefited by stimulants and narcotics. Next to these, in point of frequency of occurrence, are cases complicated with cerebral inflammation; then follow those cases of the disease of an asthenic nature, but accompanied or complicated with gastric inflammation; and, lowest on the scale, that is to say, of least frequent occurrence, are cases presenting the connexion on which Broussais insists."

Of this form of the disease Dr. Munk relates three cases successfully treated by him; in general terms, he characterizes it as "occurring in individuals of the most abstemious habits, but likewise (and this is the point on which we would particularly insist) in invariably presenting symptoms during life, and appearances after death, which unequivocally denote a well known, because appreciable, organic lesion—that lesion being either gastritis, enteritis, or both conjoined."

These cases were treated by leeches to the epigastrium, mild aperients, and one of them with morphia in addition. With regard to the employment of this remedy Dr. M. observes: "a point in practice may, however, be agitated; we refer to the employment of morphia in these cases. Morphia appears to us injurious when given in intense inflammation of the stomach. The symptoms will be best alleviated by the usual antiphlogistic means—leeching, cold water, ice, &c., but after the violence of the inflammatory symptoms has abated, when it has arrived at the subacute degree, then we know of no remedy possessing equal power with that now under consideration."

Med. Gazette. February 10, 1838.

On the Anatomical Structure, and on some of the Morbid Conditions of the Valves of the Aorta. By T. WATSON, M.D.

In a lecture, reported in the Medical Gazette, (vol. xvi. pp. 56, &c.) it was stated by me that the wart-like appearances, seen upon the semilunar valves of the heart, after death, in rheumatic carditis, often form "a kind of double festoon on each valve, from the sesamoid body in the centre to either extremity of its margin, following a natural line of division between the thinner and thicker portions of the valve." The natural structure here referred to, which I believe to be constant, has

received but little notice from anatomists. Its relation, however, to a peculiar morbid condition of these valves, makes it worthy of a more particular description. The cardiac valves consist of a loose duplicature of the delicate membrane that lines the heart; the *endocardium* of modern pathologists. Between its folds is received a thin prolongation of fibrous tissue from the tendinous rings surrounding or constituting the several orifices which are furnished with a valvular apparatus. In the semilunar valves this fibrous substance does not interpose itself between the entire space of the folded membrane. It reaches the free edge of each valve at three points only, namely, at the centre, where it forms the *corpus aurantii*, and at the two extremities. Between these points it stops short, and has a definite limit and outline—a scalloped edge—and so leaves two crescentic portions of the valve, formed merely by the doubled endocardium. In this manner arises the distinction alluded to in my lecture, between the thinner and thicker parts of the valve. The crescentic edges are thin and transparent; the remainder of the valve is more or less thick, firm, and opaque. This structure obtains in the semilunar valves on both sides of the heart, but it is much more conspicuous in some hearts than in others; and more so always in the valves of the aorta, than in those of the pulmonary artery.

It is along the line of union between the scalloped edge of the thicker portion and the inner convex margin of the two thinner crescentic portions, that the minute vegetations (as they have been called) most commonly arrange themselves. Sometimes they follow that double festoon very exactly and completely; sometimes their continuity is broken, and they straggle a little from the line; but still the general tendency to adhere to it is obvious.

Medical Gazette. December 16, 1837.

A Selection of Cases presenting aggravated and irregular Forms of Hysteria; and an Analysis of their Phenomena. No. I. Hysterical Ischuria. By THOMAS LAYCOCK, House-Surgeon to the York County Hospital.

THIS paper contains the detail of two cases (one given at great length) of a singular form of disease, which must, some time or other, have engaged the attention and puzzled the judgment of every practitioner of experience. It also contains references to many others of a similar kind, and an abridged account of several. The principal case is that of a young girl, (and all, or almost all, these cases occur in young women,) who, during a period of twelve months or more, presented an infinity of odd, and anomalous, and unaccountable symptoms, comprising, among others, the exhalation of the catamenial and urinary secretion from the umbilicus, mouth, ears, &c. It is remarkable that in this case, as in many others of a similar kind on record, there coexisted manifest imposition on the part of the patient with the real morbid phenomena. On a former occasion (Vol. IV. p. 252,) we referred to this class of diseases, and expressed a wish that the subject were investigated in a philosophical manner. There is none more interesting in the whole records of physic. As contributing some new and authentic materials, Mr. Laycock's paper will be useful: it is highly creditable to his zeal and industry.

Edinburgh Journal. January, 1838.

Statistical and Pathological Report of the Royal Infirmary of Edinburgh, for the Years 1833, 34, 35, 36, and half of 1837. By JOHN HOME, M.D.

It appears from this Report that the managers of the Royal Infirmary, much to their credit, "towards the close of the year 1832, appointed an additional clerk, under the name of Pathological Clerk, whose duty it should be to perform all the post-mortem examinations,—to draw out a particular account of the morbid appearances,—and to insert them in a journal kept for the purpose, called the Register of Dissections, along with a short history of each case, extracted from the daily journals." This office has been filled, since October, 1833, by the author of the present paper, which, although containing nothing very original, is sufficient evi-

dence of his industry and pathological knowledge. The portion of the Report now published is confined to the subject of Phthisis Pulmonalis, and contains a well-digested analysis of the statistical and pathological facts deducible from one hundred fatal cases of this disease. Owing to the numerous classical works that have, of late years, been published on the subject of consumption, more particularly those of Louis and Sir James Clark, the details given in Dr. Home's paper supply us with little that is new: it is, however, an important document and well worthy the attention of the medical statist and pathologist. It is divided into two sections: the first embraces the following circumstances in the history and progress of the disease during life—1, sex; 2, age; 3, occupation; 4, duration of the disease; 5, the influence of season; 6, comparative frequency and mortality; and, 7, peculiarities presented by some of the symptoms. In the second is considered the pathology of the disease, under the following heads:—1, the degree and frequency of the affection of the larynx and trachea; 2, the nature and extent of the affection of the organs of the chest; and, 3, the nature and extent of the affection of the abdominal organs.

Edinburgh Journal. January, 1838.

Observations relative to Changes in the Crystalline Lens, and the Cause and Cure of Cataract. By Sir DAVID BREWSTER, K.H. F.R.S.

1. THE method of ascertaining that the change on which presbyopia depends commences generally at the margin of the lens, is to place a taper, or small luminous image, at the distance of thirty or forty feet from the eye. The image of the taper, in place of being perfect, will be elongated in one direction, very frequently downwards, so that the eye is often unable to see the black wick of a candle, in consequence of part of the flame being brought down upon it. If the change were general or symmetrical, a convex lens would remedy the imperfection of vision which it occasions; whereas, it is well known that, until the change has gone round the margin of the lens, the eye does not derive any aid from spectacles. I have especially studied this fact in my own case, and seen it in many other cases.

2. When this change has become symmetrical, and advanced regularly in a sound eye, the application of a lens remedies the imperfection of vision arising from the increase in the focal length of the crystalline. But, if the change has not advanced favorably, either from an improper use of the eye, from general bad health, or from other causes, the fibres, and even the laminae, *separate*; that is, they cease to be in *optical contact*, in consequence of the lens not being supplied with a sufficiently aqueous secretion. Now, when light passes through a part of the lens where this separation has taken place, it is reflected and decomposed by the fibres, so as to produce not only the prismatic colours, but irregular luminous figures, round the candle. If we now take a plate of brass, with a small hole in it, we may so place it before the eye as to exclude all the light, except that which passes through the diseased part. When this position is found, the eye is *blind*: it can see no objects distinctly, because all the sound part of the lens is shut up. If, on the contrary, we take a small-headed pin, and place the head of it so as to prevent any light from falling on the diseased part of the lens, while the sound part receives rays from any visible object, the vision will be *perfect*. If only a small part of the lens is sound, we may enable a blind eye to see distinctly, by excluding all the light except that which falls on the sound portion of the lens. I have repeatedly performed this experiment, to the surprise of the patient. The colours above referred to are always seen in the crystalline lenses of *animals*, when the laminae or fibres are separated.

3. The *evidence* that a derangement of fibres arises from mismanagement of the eye when presbyopia commences is not very ample, and has more the character of inference than of demonstration. The separation of the fibres, indicated by coloured images, and irregular luminous branches round and near the principal image, took place in my own eye, and continued for nearly eight months; during which I studied the changes which took place, and made drawings of the luminous figures. As I was in the habit of exposing my eyes to the severest trials, often

for twelve and fifteen hours a day, I could not doubt that this *first stage of a cataract* was owing to the greatest mismanagement. It is, besides, universally admitted that, at those periods when the constitution undergoes a change, the greatest attention to the general health is necessary; and, surely, in such a delicate fabric as the crystalline lens, composed of *millions* of fibres, it is highly necessary, when it begins to change its mechanical condition, that it should not be exposed to the same hard work which it might at other times be capable of performing.

4. The experiments by which the seat and extent of the disease may be ascertained have been stated in section 2.

5. I fear it would be presumption in any one, not a medical practitioner, to answer your correspondent's fifth query, respecting the proper remedies for stopping the progress of these changes. I cannot err much, however, in stating what I did in my own case, and leaving your readers to judge whether or not it had any share in the result of restoring my eye to a degree of strength which has enabled it, during the last fifteen years, to perform the hardest work to which that organ can be applied.

Considering the disease as arising from a defect in the secretions by which the lens is supplied with aqueous fluid, I deemed it necessary to give up all experiments,—to abandon reading at night,—to preserve the eye from strong and numerous lights,—to pay the greatest attention to diet,—to take regular exercise; but, above all, to keep clear the *primæ viæ*, which I did by taking daily the *Pulvis salinum compositum* for nearly eight months. It is a curious circumstance that I first saw the coloured images, when I was making experiments with my eye, while waiting for the moves of a slow chess-player; and eight months afterwards, in the same room, and similarly employed with the same companion, I all at once observed the images disappear, the fibres and laminae having then been brought into optical contact. Ever since that time my eyes have been exposed to severe work, and the one which had begun to give way is now the one which I invariably use in all difficult experiments.

6. I believe it is by no means common to see the coloured images that have been above described, but it is extremely common to have the lens so disorganized by inequality of density in different parts, arising, no doubt, from differences in the refractive power of the secretions, that a distant luminous object is not only seen of an irregular form, but is often subdivided into two, or even more, amorphous images. This I have found to be the source of a great many defects of vision which had been ascribed to very different causes.

7. If spectacles are so constructed as to exclude the rays which fall upon the diseased part of the crystalline, the patient will have the advantage of good vision; but, if the sound part is very small, it will be difficult to keep the small aperture opposite to it.

8. As long as the sound part of the lens is of such a magnitude that the patient can see through a small aperture, the art of applying and using which he will acquire by experience, the continued use of the aperture, or the spectacles in which it may be placed, will of course be beneficial.

I have thus answered your correspondent's queries in the way I would have done fifteen years ago; but I have recently been led, by experiments on the crystalline lenses of animals, to more extended views, of which I shall endeavour to give a brief and general idea, referring the reader for more minute information to a paper in the forthcoming volume of the *Philosophical Transactions*, and to the *Reports of the Proceedings* of the last two meetings of the British Association.

In these researches I was led to the discovery that the capsule of the crystalline lens is a membrane which performs the functions of endosmose and exosmose, keeping up a due proportion between the aqueous element in the aqueous chamber and the lens. Even in the dead state this membrane imbibes distilled water so greedily that the lens which imbibes it becomes quite soft, expands, and bursts.

Viewing the capsule in this light, there can be no doubt that *soft cataract* arises from an excess of aqueous humour imparted to the lens through the capsule; and that *hard or dry cataract* arises from a defect of water in the aqueous humour, or an excess of albumen.

Let us now suppose that the skilful oculist has ascertained that there is a disorganization of the lens which does not yet appear as cataract. He will, of course, first try the remedies already mentioned, as they are equally suitable for both varieties; and he may probably succeed in restoring a healthy action in the parts: but, if he finds the disease gaining ground, he must resort to more direct methods before the lens becomes irrecoverably injured.

In this state of matters he should puncture the cornea, so as to get a small portion of the aqueous humour, in order to ascertain, from its refractive power, whether it contains too much or too little albumen. In either case, it might be advisable, by a partial evacuation of the aqueous humour, to reduce the quantity of the diseased secretion, in the hope that a healthier one might be supplied; but, if the disease should continue to advance, distilled water, or a solution of albumen, should be injected into the aqueous chamber, to restore the aqueous humour to its proper condition. If the aqueous humour first taken out should prove to have the ordinary refractive power of that humour, and to appear otherwise in a healthy state, in that case it would not be advisable to introduce either water or albumen. It is quite possible that the capsule of the lens might lose its power of supplying aqueous fluid to the lens, or might supply it too copiously; but such a defect is likely to shew itself either in partial or general opacity. At all events, this opinion will only be entertained in the case where the aqueous humour is found to be in a healthy state.

Although these views are applicable principally when the disease is taken early, yet, even if a lens had *white opacity*, which might not be accompanied with any other disorganization than the mere want of a fluid to restore the optical contact of the fibres, I would not despair of effecting the desired change by the methods above given.

Med. Gazette. Dec. 30, 1837.

Case of Pericarditis. By THOMAS WATSON, M.D.

[THIS case is interesting, as shewing the security of the physical diagnostics in the hands of an experienced auscultator. We retain only such parts of the case and of the dissection as bear on the diagnosis.]

On the 11th of October, 1836, I was introduced to a gentleman, thirty-nine years old, with a pale face and sharp thin features, sitting up in bed, breathing shortly and laboriously. His legs were anasarcous, and his belly was tense and fluctuating. I had been previously informed that for years he had been given up to intemperance in drinking, and to indolent and low habits of life. Twice or thrice he had been affected with delirium tremens. He complained to me that he was troubled with wind, shooting upwards through all the left side of his chest: on further enquiry, I found he meant that he had much pain there. A diffused sibilous wheezing was audible in the upper portions of the lungs on both sides: both sides were dull on percussion, in front and behind, at their lower part; and on the right side no sound of respiration could be heard in that part. The jugular veins were swollen and tortuous. His pulse was frequent and very feeble. On applying my ear to the præcordial region, I was immediately sensible of a very loud and harsh *to and fro* sound; a noise of rubbing, apparently close to the ribs, drowning all other sound in that space, keeping time with the alternate motions of the heart, and equally manifest when the patient held his breath. This sound was distinctly perceived by Dr. Sweatman. I stated to him my conviction that our patient was labouring under recent and acute pericarditis. I concluded that he also had hydrothorax; and that, with respect to the heart, there was dilatation of its right cavities at least. I ventured to predict that either Mr. S. would speedily die while the rubbing sound continued, in which case the pericardium would be found inflamed but unadherent, or only partially adherent, to the heart; or, if he survived a few days, the remarkable sound of friction would altogether cease, which would indicate with certainty that adhesion had taken place.

Mercury and some diuretic medicines were prescribed, and leeches were applied to the painful side. I visited Mr. S. again on the 15th. He had been greatly

relieved by the treatment adopted, being free from pain and less swelled. The *to and fro* sound, however, continued; but it was less harsh, and confined to a smaller space.

On the 20th I saw him for the third and last time alive. The rubbing noise was entirely gone, and a dull, systolic, deep-seated bellows' sound had taken its place. He was making a vast quantity of urine, the dropsical affection had greatly diminished, and he professed himself to feel much better; but his pulse was scarcely perceptible.

On the 31st it was noticed that he spoke in an odd and unusually loud tone, and his pulse, as Dr. Sweatman informed me, could not be felt; but he said he suffered no pain, and he seemed to breathe with tolerable ease. In the course of that evening he sat up in bed to swallow a dose of medicine; and, having done so, he leaned his head against the nurse, who was supporting him, and presently expired. The body was opened the next day by Mr. Shaw, in the presence of Dr. Sweatman and myself. I did not hesitate to express my certain persuasion that the heart and pericardium were adherent.

There was much fluid in each pleura also. The right lung occupied exclusively the upper part of the cavity, its inferior margin being turned upwards, and fixed by a strong band of adhesion, half an inch in length, to one of the ribs, about midway between the diaphragm and the clavicle; so that the fluid must have been poured out, and have floated the edge of the lung to that height before it adhered. The left pleura was quite healthy. The heart was large. The cavity of the pericardium was entirely obliterated, the opposed serous surfaces of the membrane being united by means of a thick layer of soft mottled lymph: they were easily separated by very gentle traction. All the cavities of the heart were larger than natural: those of the right side were much dilated, the left were filled with dark clotted blood; the walls of the ventricles being considerably thickened. Both the mitral and the aortic valves were morbidly thick and stiff, but there were no "vegetations."

Ibid.

On the Use of Arsenic in Plethora. By Mr. LAW.

[WE think the following case important, because it affords one instance at least of effectual relief in a most common and, in general, a most untractable disease. Every practitioner of experience will immediately recognize in this case an old acquaintance: we wish we could as surely promise that it exhibits a means of cure likely to be often useful.]

M. F., aged thirty-seven, has for seven or eight years been occasionally subject to violent headach, continuing for some days, and, though not always, very generally proceeding to a hysterical paroxysm, which, along with the headach, is only dispelled by free bleedings of from sixteen to twenty ounces of blood, and sometimes a larger quantity. She is rather above the middle stature, and of a full habit, without much complexion; the bowels apt to be slow, but the necessary aperients regularly employed. She is rather apt to be sedentary, but partakes most sparingly of sleep, or even the recumbent posture, and takes food as well as liquid in much less quantity than the average of her own sex, avoiding vinous stimuli almost entirely. Catamenia regular. Every practitioner summoned to her assistance, wherever she may have been residing, has, from the urgency of the symptoms, been led to the use of the lancet, which alone ever relieved her effectually at the time, the blood flowing with unusual force from the arm when I have had occasion to open a vein in this person. In a few hours after one of these full bleedings, she will leave the recumbent position, proceeding with her usual occupations as if nothing had been the matter. These attacks became of more frequent occurrence towards the end of last spring, when it seemed to me that, as it was next to impossible to diminish the ingesta here, and as the degree of exercise necessary to subdue so strong a disposition to plethora would be of very difficult enforcement, independent of other collateral circumstances in this case, arsenic, administered in

small doses at the commencement of an attack, would, from its sedative influence on the system, lessen what we are contending against, and for a longer time than the bleedings, which are so apt to demand repetition. Accordingly, she had a watery mixture prepared, with five minims of the arsenical solution in each teaspoonful, directing her to take a teaspoonful in a little more water, morning and evening, just after a meal, when threatened with an attack, and to intermit it entirely when the tendency disappeared.

It is now six months since the trial was first made, the medicine being, according to these directions, only occasionally employed: nor has she ever since suffered, but in a very slight degree, from what had begun to assume a more alarming aspect. She has on several different occasions in this time found it necessary to have recourse to the arsenic for three successive days, but with the same marked benefit; and, what may appear less accountable, the disposition to such a disease has been thus controlled under even some increase of appetite, using rather more food than formerly.

Edinburgh Journal. January, 1838.

On the Inhalation of Iodine and Conium in Tubercular Phthisis Pulmonalis.

By Sir CHARLES SCUDAMORE, M.D.

OUR readers are aware that, several years since, Sir Charles Scudamore published a small work, in which this practice was strongly recommended. Having omitted to notice the exact formula used by him in the cases narrated, he was (we think very properly) found fault with by the critics, and afterwards published the formula which, as amended in his present communication, is as follows:—R. Iod. gr. viij.; Potassæ Iod. gr. iij.; Alcohol. ℥ss.; Aquæ dist. ℥vss. M. Of this solution, from one drachm to six, and from twenty to thirty-five minims of a saturated tincture of conium are used in each inhalation, which is continued from half an hour to forty minutes. “It is better always to add the conium at the time of using the inhaling. At the temperature of 90°, the volatile properties of iodine are given off very sensibly; but the conium requires more heat, and that of 120° is not too much for the iodine. This degree, therefore, I most recommend; or, if the patient have not a thermometer, let the instruction be to put the water into the inhaler, (first warming it a little to prepare it,) quite as hot as the finger can bear without pain. The inhaler should be kept immersed in rather hotter water during the process. A good glass inhaler also is a material consideration. If it be small, and the tubes too contracted in the bore, the difficulty of inhaling would be great to the invalid, whose respiration is easily embarrassed; whereas, with a fit apparatus, the process is perfectly easy, and not fatiguing.” . . . “In the employment of the inhalation, perseverance is necessary, and in some instances for many months. The object sought to be obtained is not merely palliative benefit,—not merely a temporary impression on the morbid function,—but the superseding of the diseased action by a healthy one, and the effecting some organic change.”

In the present communication, Sir C. S. professes to give a concise summary of his further experience in phthisis and bronchitis. He refers to cases formerly published by him, and states the favorable result of several of these. The new cases are six, related by Dr. Davidson, including his own case; several of which are much in favour of the practice. Sir C. S. contents himself with stating that he could “relate the cases of a gentleman, aged fifty-four; of a young lady, aged twenty; and of a medical practitioner, aged thirty; in which the most unequivocal symptoms of tubercular disease were strongly developed, in which there was every threatening of danger: and in all of them I was happily quite successful.”

It is but justice to Sir C. S. to state that he employs the other ordinary means of treatment together with the inhalation; and our own experience leaves us no room to doubt the great value of the practice as a palliative in phthisis, and as an important remedy in bronchitis. It is true, however, that the relief has often been as great from the simple aqueous as from the medicated inhalation.

Med. Gazette. February 17, 1838.

On Rupture of the Heart, and on Hemorrhage into the Pericardium, without Rupture of the Heart or great Vessels. By JOHN THURNAM, Esq.

[THIS is an interesting paper, containing three well-detailed cases. We give the last entire, (omitting that part of the dissection not having reference to the subject of the paper,) as it contains a curious explanation of the manner in which blood found in the pericardium may sometimes reach that cavity.]

William Shingleton, aged forty-two, was brought to the Westminster Hospital, December 2, 1837, with very extensive injuries of the bones of the head, ribs, and clavicles: he died almost immediately after being seen by Mr. Bury Dasent, the house-surgeon. It appeared that, during the dense fog which prevailed at the time, he had been knocked down by a runaway horse and chaise.

Upon opening the pericardium, it was found to contain a considerable quantity of black fluid blood. Both the right and left cavities of the heart contained semi-fluid black grumous blood, but there was no rupture in any part of this organ, nor in the large vessels contained within the pericardium. In the upper portion of the mediastinum, and reaching to the root of the neck, there was found a considerable layer of coagulated black blood surrounding the bifurcation of the trachea, the œsophagus, superior vena cava, and ascending aorta; and, upon tracing this ecchymosis carefully downwards, it was seen to terminate on the surface of the cava within the pericardium at its highest part, and at the same point there was found to be an irregular laceration of the fibres forming this membrane, through which the blood had evidently gained access into its cavity. Although I did not succeed in demonstrating it, yet I have little doubt that the extravasation had originated in the rupture of some of the large veins in the front of the trachea above the sternum, and that this rupture had been occasioned by the displaced portions of the fractured clavicles.

Ibid.

SURGERY.

Report of Surgical Cases, treated in the Glasgow Royal Infirmary, during the Years 1836-7. By WM. DAVIDSON, M.D., Surgeon to the Glasgow Royal Infirmary.

THIS is a long and very valuable Report; but, as it consists, for the most part, of histories of single cases, we are prevented from laying a general analysis of it before our readers: it will, however, well repay a careful perusal. The cases are graphically described, and the remarks on them highly judicious and instructive. We can only find room for one or two brief extracts.

Hydrocele. Two cases are related, which were treated by acupuncture, but without permanent benefit. On this subject Dr. Davidson remarks: "Were it in general ultimately successful, it would be the most simple, and at the same time the most extraordinary, operation that is recorded in surgery. Even upon the supposition that it is only a palliative or substitute for the common plan of tapping with a trocar, the discoverer is entitled to much credit; for a puncture with a sewing-needle is attended with scarcely any pain, and the most timid of men would submit to it without apprehension. The results arising from the treatment of two cases are not enough for drawing any certain conclusions; but certainly they tend to establish this point, that no particular change is effected upon the internal coat of the tunica vaginalis, and that the fluid reaccumulates as after ordinary tapping with a trocar.

"It may be stated that the cases above mentioned were not well adapted for the plan; for in both the testicles were enlarged. This certainly is an objection to the success of acupuncture, as well as to every other plan for a radical cure of hydrocele; but a little enlargement of this organ is a very common occurrence in this disease, and in the first case detailed the enlargement had almost completely subsided before he left the house. It appears to me that this operation is not likely to supersede the radical cure by injection, but that as a palliative it ought generally to be preferred to

the use of the trocar; at the same time, I am of opinion that a frequent and long-continued use of the needle may in some cases effect a radical cure; and this view is supported by the fact, that after acupuncture has been repeated several times, the re-accumulation of fluid goes on less rapidly than after the first or second operation."

Erysipelas. This disease infested some of the wards in the infirmary notwithstanding the greatest care to avoid all the probable causes of it, and its attacks rendered abortive several operations and proved not unfrequently fatal. Some interesting cases are related. We quote the following, not, however, as a specimen of the disease, but as exhibiting a novel method of evacuating the contents of abscesses.

"In the latter end of January, 1837, a lad, about sixteen years of age, who was admitted for *necrosis* of the leg and arm, and where a circular piece of necrosed bone, about two inches and a half long, had been extracted from the tibia, was seized with the disease in about eight days after the operation. The disease made its first appearance in the knee, and spread rapidly upwards to the groin; pulse 130, feeble, and he had frequent vomiting. An abscess took place on the dorsum of the foot, which was opened, and the knee-joint became much swollen on both sides of the patella, attended with a throbbing pain. It was evident that purulent matter had collected in the joint; but as making a free opening into it would have most probably increased the constitutional irritation, the following method of treatment was adopted. A grooved needle, about a line in diameter, was introduced into the most prominent part of the swelling, on the inner side of the joint, which gave exit to a sero-purulent fluid. A cupping-glass was now applied over the puncture, and about three ounces of the same fluid were evacuated. The wound was found completely closed next day, and the same operation was repeated every successive day, for a few days, with the evacuation of from one to two ounces of fluid each time. He left the hospital in about a week afterwards, his limb being nearly restored to its former state, with the exception of the knee, which was still somewhat swollen.

"This plan of using the grooved needle and cupping-glass might be advantageously employed, not only in evacuating collections of matter in joints, but also in some chronic abscesses, where high constitutional irritation is apt to follow a free incision."

The following extract exhibits the general mode of treatment adopted, and its results:

"In the general treatment of erysipelas, the antiphlogistic plan was only employed in a few cases in the commencement of the disease; for it was found that symptoms of debility in general appeared pretty early, more especially if diarrhœa was a concomitant, which was not unfrequent. The tonic plan was, therefore, found the most successful, and it consisted of wine, sulphate of quinine, light nourishing diet suited to the state of the digestive organs, laxatives or laxative enemata, and occasionally opiates at bed-time. The external treatment consisted of leeches, punctures, incisions, mercurial ointment, nitrate of silver applied in the form of a weak solution to the whole erysipelatous surfaces, or applied in the solid state in the form of a circle, with the intention of insulating the disease. Leeches and punctures were not found so beneficial as incisions; and the latter were generally practised, and made in various parts of the region affected, to the extent of from one inch and a half to two inches and a half in length, through the skin and cellular texture. In the slighter cases, where the disease appeared to be superficial, mercurial ointment, and a solution of nitrate of silver, consisting of ten grains to an ounce of water, were applied; but the latter was found the most efficacious of the two; and generally, on the day following its application, the swelling and redness were much diminished. The solid nitrate of silver succeeded, in the great majority of cases, in preventing the spreading of erysipelas; and the following points require to be attended to, in order to ensure success. 1st. It must be applied to a sound part of the integuments, viz. a part where there is no swelling or redness; but as near as possible, so as to avoid this. 2d. The inflamed surface must be completely encircled by the caustic line. This may be effected in the following way. Take a pretty large hair-brush and moisten thoroughly with water the part that has been selected to the breadth of about an inch; then rub a cylinder of lunar caustic very freely over this moistened portion of the skin. Distinct vesication

over the whole surface to which the caustic has been applied should be produced; for if this does not follow, the disease may extend beyond the line. And this is perhaps the reason why a saturated solution (consisting of equal parts of the salt and water) is not so certain as the solid caustic, for erysipelas seems to extend its boundaries by creeping along the cutaneous surface, before it affects the cellular tissue; hence if its progress over the integuments can be checked, its extension in the textures below will at the same time be prevented. In general, after the caustic has been thus applied, the inflamed integuments in the immediate vicinity of it become partially shrunk and puckered; but the state of the previously affected parts appears to be uninfluenced by it, and they proceed to resolution or suppuration, according to the nature of the case. Many cases could be quoted from the journals of the house, besides those already noticed, where this practice was adopted, in proof of the general efficacy of this mode of insulating erysipelatous inflammation; but their introduction would render this report too long."

Edinburgh Journal. Jan. 1838.

Singular Case in which a Steel Fork was Extracted from the Back.

By DAVID BURNES, M.D.

ROBERT SYMS, aged twenty-three, was entered on the sick list of H. M. ship *Belvidera*, about the middle of June, 1831, complaining of pain at the inferior angle of the right scapula, close to the base of which was a small phlegmon, as I then considered it, in the early stage of suppuration. On the 19th of June, I opened "the boil," and ordered poultices to be applied, thinking it would heal kindly in a few days. On the 23d, however, on probing the wound, I felt what I first thought was the edge of the scapula, but, on more minute examination, something black and shining was seen in the wound. On the 24th, it being evident that there was some foreign body in the wound, the opening was enlarged directly upwards, and a piece of steel, about the thickness of a common ram-rod, presented itself, but resisted strongly any efforts to extract it. Being unwilling to put him to further pain, while there was a chance of its coming away by poulticing, and pulling it with the forceps daily, this gentler course was agreed on in preference to making a further enlargement of the wound. Being questioned as to the nature of the piece of steel, he expressed himself as much astonished as we were at its presence, and said he should not have known it had we not told him, and had he not felt pain from our pulling it with the forceps. He had never been in action, having been only two years in the king's service, nor did he recollect having received any wound by which anything of the kind could have been introduced. About two inches below the opening made on the 19th, we observed a small white speck, or mark, rather resembling the mark left many years after vaccination, than a cicatrix of a wound. This was the only vestige of anything like a wound that we could detect in his back.

July 2. The poulticing has been continued, and there is now a free discharge from the wound; the steel has been pulled daily by the forceps, and admits now of farther motion, especially laterally, but is yet forcibly retained at its upper part; its direction is nearly parallel with the base of the scapula, close to which it lies, and in its course upwards it seems to incline deep into the substance of the muscles. About an inch of it can be seen when the integuments are retracted. He is averse to further measures; has no pain except from the use of the forceps. Continue the poultices.

16th. Though the poulticing has been continued, and the steel pulled daily, there is no material alteration since last report, further than that the steel may be moved more freely in every direction, except when pulled directly downwards, when it seems to be retained as forcibly as at first; the probe can be introduced into the wound, upwards and inwards, nearly four inches, and can with some difficulty be made to move round the steel; but no information as to its size or shape can be gained from this mode of examination. It occurred to me, at this time, that it was a hook, and that it might be retained by catching on one of the ribs. Having no pain except from the pulling, and being still averse to the use of the knife, the same treatment was pursued.

August 5. The foreign body having become very little loosened, and now causing

more pain on its being moved, I made a deep incision of about three inches in length over its course upwards, using it as a director, when it was easily extracted, and found to be a common kitchen fork, broken off close to its handle, and with one of its two prongs wanting about an inch from its point; it was blackened, and, in some degree, rusted. It seemed to have been retained by a bridle of muscular fibres embracing its shoulders, for it was immediately liberated when the part was divided by the knife.

The wound was dressed simply, and healed so soon that in ten days the man was doing duty in the boats and aloft.

Strange as it may seem, even after its extraction, the man persisted in adhering to his original statement of his being ignorant, how and when it had been introduced; and during the two months I remained in the ship, I was not able to gain further information on the matter.

The patient continued to serve in the *Belvidera* till December, 1833, when he joined H. M. ship *Blonde*, going to South America. Being anxious to trace his future history, in the hope of obtaining some clue as to the introduction of the fork, I was enabled, through the kindness of Sir William Burnett, the Physician-General of the Navy, on the arrival of the *Blonde*, at Portsmouth, about a month ago, to communicate with him by letter. The result was, that he came up to London, and, on the 18th of November, called upon me to show himself. He then stated that, about eighteen months ago, while washing himself, he felt a small, hard body on the *left* side of the neck, which he was inclined to believe was part of the fork. On examining the part I had no doubt myself of its being the portion of the broken prong, and which I asked permission to extract. He readily assented.

On the 20th, in the presence of Mr. C. Smith, surgeon, I made an incision over it (its position being just behind the middle part of the posterior edge of the sternocleido mastoideus muscle, where it is crossed by the external jugular vein, when it was easily removed, and proved to be the prong, which had the same bronzed appearance as the fork itself, and was coated with rust at its fractured end. It does not exactly join with the fork, and I am inclined to think some very minute splinters may have been broken from it when fractured, or some chemical action, while in the body, may have corroded it.

It is singular that he had never suffered pain from it, although it has crossed from the *right* side of the back to the *left* side of the neck. I was only induced to extract it from its superficial position, and the singularity of the history, yet it is possible it might, in time, have advanced still further, and have injured the carotid artery, or trachea. Though cross-questioned by all who saw him, he still repeats his former story of being innocent as to the introduction of the fork.

Lancet. December 2, 1837.

Case of Rupture of an Aneurism of the Common Carotid, and Ligature of that Artery near its Origin from the Innominata. By T. ARGYLL ROBERTSON, M.D., F.R.S.F.,
Lecturer on Surgery in Edinburgh.

[THE following case, as the author observes, is deserving record as being the only one in which a ligature has been applied to one of the larger arteries of the body, nearest to their common origin from the heart, with perfect ultimate success.]

Major —, the subject of the case, is now in his fifty-second year. In April, 1836, while hunting, his horse, when at full speed, put its foot into a rabbit hole, by which both it and its rider were brought to the ground with great violence. Major — received a severe wound over the left parietal bone, which bled profusely, and he remained for a short time stunned, and in a state of insensibility. From that period he suffered from stiffness and pain in the right side of the neck, resembling what is usually termed a crick, accompanied by shooting pains over the whole right side of the head: occasional attacks of giddiness. . . . About the middle of January last he first discovered a swelling on the right side of the neck, accompanied by enlarged tonsils, slight sore throat, and some difficulty in swallowing. The swelling in the neck was supposed to be simply an enlarged gland, and did not attract particular notice. . . . On the 20th of March, without any premonitory symptoms, at ten

o'clock, P.M., a sudden gush of blood took place from the mouth; it was discharged in gulps or mouthfuls in rapid succession; it ceased spontaneously after half an ordinary wash-hand basinful of blood had been lost; he retired to bed, and slept very soundly during the whole night. The following morning he rose at eight o'clock, but had scarcely reached his dressing-room when the hemorrhage returned, and, to use his own expression, the blood literally poured from his mouth; he soon fainted and fell upon the floor, breaking a foot pail in the fall; about fifty ounces of blood were at this time discharged by the mouth, and a considerable quantity must have passed into the stomach, as the stools afterwards consisted almost entirely of coagulated and grumous blood. By the two hemorrhages he must have lost upwards of one hundred ounces of blood. The bleeding now ceased, and he rallied a little and procured assistance. . . . I reached Major ——'s seat about midnight, and found him perfectly composed and tranquil, with the pulse scarcely perceptible at the wrist, and continuing to beat about forty-five strokes in the minute. On examining the neck I found a tumour extending from near the angle of the jaw to within one inch of the sternum, and projecting laterally to about three inches. Its surface was smooth, equal, and rounded, and a very obscure pulsation could be detected. Judging from the state of the circulation, and from there not having been the slightest return of bleeding since eight A.M., that there was not immediate danger, I thought proper to postpone attempting to secure the carotid below the seat of the aneurismal swelling, (evidently the only surgical resource that was left me,) in order that I might have the benefit of daylight for the operation.

In consequence of the aneurism being seated so low down in the neck, the external incision was limited to little more than an inch, following the course of the sternomastoid from the sternum upwards. On dividing transversely the sterno-thyroid and a few fibres of the sterno-hyoid muscles, a narrow projection of the aneurismal tumour, passing between the artery and trachea, was brought into view; so narrow that I at first supposed it to be the artery somewhat dilated, and passed the aneurism needle round it. It was, however, about double the size of the artery, and its coats were thinner than natural. On examining the parts more minutely, I discovered the carotid displaced laterally by this prolongation of the sac. The pulsations were feeble, though perfectly distinct; the situation at which it was exposed was within a finger's breadth of its origin from the innominate, and when the finger was applied to this point the carotid was felt pulsating on its palmar, the innominate on its lateral surface. Immediately above this point the vessel swelled out into the aneurismal tumour. The ligature was therefore applied within half an inch of the origin of the artery. During the performance of the operation neither vein nor nerves were seen. The operation itself was necessarily tedious and difficult, in consequence of the limited extent of the external incision, the deep situation and unnatural displacement of the artery, and the importance of the organs by which it is surrounded. At one time I thought it would have been necessary to have divided the sternal attachment of the sterno-mastoid, but this was avoided by relaxing the muscle and drawing it outwards. The vessel was no further separated from its attachments than was necessary for the simple passage of the aneurism needle. On tightening the ligature all pulsation ceased in the tumour, and it was reduced nearly a third in bulk. No peculiar sensations were experienced by the patient, who bore the operation with the greatest possible fortitude. He was placed in bed, with the head considerably elevated to relax the parts, the lips of the wound having been previously brought together by suture, and supported by a strip of adhesive plaster. Strict antiphlogistic regimen and perfect rest were enjoined. The bowels were regulated by enemata to avoid any risk of sickness, vomiting, or hypercatharsis from the exhibition of cathartics. The wound healed by granulation; at first the discharge was thin and slightly tinged with blood, but gradually it assumed the characters of healthy pus.

On the seventeenth day the ligature separated, and the wound speedily healed. The aneurismal tumour rapidly disappeared, and now no trace of it whatever can be discovered. From the period at which the ligature was applied up to the completion of the cure not an untoward symptom appeared. On the second day after the operation the pulsation in the branches of the external carotid was distinct. During the

third week a few drops of blood were discharged from the right nostril, accompanied by a little irritation giving rise to a great desire to sneeze; probably depending on the new arrangement of the circulation. At the present time, 28th September, six months from the date of the operation, Major _____ is in the enjoyment of the most perfect health.

Dublin Journal. January, 1838.

Observations on the Pathology of Staphyloma.

By T. WHARTON JONES, Esq. London.

THIS is a valuable paper, exhibiting the author's familiarity with the diseases of the eye and the nice operations indicated in the removal of so many of them. We regret that our space permits us to lay only a portion of the paper before our readers.

"The only mode of formation of staphyloma which I could ever trace is the following:

"If, in scrofulous, catarrhal, or catarrho-rheumatic ophthalmia, there be a penetrating ulcer of the cornea, the aqueous humour escapes, the iris falls forward into contact with the cornea, and a small part of it is perhaps prolapsed through the ulcerated opening. The progress of the ulceration being stopped by the yielding of the inflammation, the prolapsed portion of iris, and the ulcerated part of the cornea, are involved in one cicatrice. The opening in the cornea being thus closed, the aqueous humour again collects, and the anterior chamber is restored, though somewhat diminished, in consequence of the partial adhesion between the iris and cornea (*synechia anterior*.) There is no prominent distension on the front of the eye in this case, because, as the inflammation subsides, the small protruded portion of iris shrinks and flattens; but if the destruction of the cornea has gone on farther, either by ulceration or the giving way of an onyx, and considerably more of the iris has protruded, the prolapsed portion of the iris does not shrink when the inflammation begins to abate, as in the former case, but remains, and forms a projection at one part of the cornea, generally the lower or lateral. This projection is at first merely a bag of the iris distended by the aqueous humour; but by and by its exposed surface becomes covered by an opaque firm tissue, of the nature of the tissue of cicatrices, and this tissue is incorporated at the base of the tumour with the sound cornea. The projection, the mode of origin I have just described, is a partial staphyloma; it is not a distension of the cornea itself, but a protruded portion of the iris covered by a new tissue, intended to supply the loss of substance which the cornea has sustained. The mode of origin of a total staphyloma is essentially the same, but differs only in degree. The whole or greater part of the cornea being destroyed, as occurs in gonorrhœal, purulent, and very often in variolous ophthalmia, as also that of new-born infants, the whole iris falls forward, the pupil becomes closed, and the aqueous humour being thus allowed to accumulate in the posterior chamber, the iris is kept distended in the form of a tumour on the front of the eye. Its surface gradually gets covered with an opaque cicatrice-like tissue, or pseudo-cornea, which assumes a greater or less degree of thickness, and a total staphyloma is the result. Sometimes the central part of the cornea only is destroyed, a ring of the circumference still remaining; the staphylomatous projection has then the form of a small globe stuck on the front of a larger.

"Founding my reasonings on those views, I conceived that the supply of aqueous humour in the still-existing posterior chamber was what kept up the distension of the iris, and the consequent moulding of the pseudo-cornea on its surface in the form of a round prominence on the front of the eye. If, therefore, it was natural to infer the source of the aqueous humour could be destroyed, we should not have the development of the staphylomatous projection, or, if already formed, it would disappear. For this purpose puncturing of the tumour is not found to answer well. To break in upon the integrity of the posterior chamber, I conceived the simplest and most effectual plan would be to extract the lens, an operation which I put into practice in the following case.

"A young man, about twenty-two years old, came to me labouring under the effects of severe purulent ophthalmia of both eyes. In the right eye I found the cornea destroyed, and the iris protruding and distended with aqueous humour, the pupil

being closed. The left eye had also suffered very much; there was penetrating ulcer, prolapsus iridis, and consequently considerable distortion and contraction of the pupil. Both eyes were still affected with the inflammation, and it was very doubtful whether the left eye could be prevented from getting worse, especially as it was evidently kept in a state of additional irritation from the presence of the staphyloma in the right. By an incision with a Beer's cataract knife in the protruding and distended iris, the lens was extracted. Severe re-action followed; less perhaps in consequence of the operation, than in consequence of the patient not having been in a situation to take proper care of himself. The iris did not again become distended; on the contrary the eye shrunk, and irritation being thus removed, the left eye progressively improved, as far as the organic changes it had already undergone allowed, and further than there had been previously reason to hope for, as vision was preserved sufficient to enable him to resume his employment as a porter."

"The conclusions which I draw from my observations are the following:

1st. "That the iris and cornea do not unite surface to surface, and if they unite at all it is only partially, and that in consequence of penetrating ulceration of the latter, and prolapse of the former.

"That the tissue composing a staphyloma is not degenerated and opaque cornea, but a new tissue, of the nature of the tissue of cicatrice, developed on the anterior surface of the iris exposed by the destruction of the cornea itself."

Med. Gazette. February 24, 1838.

Case of Fatal Convulsion during the Injection of Nævus. By T. PAGET, Esq.,
and F. FULLAGAR, Esq. Leicester.

"THE following impressive case," say the relators, "seems due to the medical world, as an instance of the distressing casualties which beset the hopes of our profession." No blame whatever can attach to the operators; while every one must sympathise with them, and admire the candour with which the facts are stated.

"The nævus was situated over the angle of the right maxilla, in a healthy and remarkably precocious child, nearly two years old, and had increased from a slight speck at birth to the size of half a small orange. It was principally subcutaneous, but had a coloured portion in its centre, which seemed covered by cuticle only. On the 19th of October the tumour was pierced with a lancet used for the nasal duct, and injected by the transfusion syringe; the mixture used being nitric æther, with one fifteenth of nitric acid. No decided effect was produced, so that either too little of the injection had entered or the mixture was too slightly stimulating, and in about a week the operation was repeated; the proportion of nitric acid being increased to one-tenth. No more effect, however, followed this than the former operation. On the 15th of November the injection was again had recourse to: using, instead of the nitric acid and æther, the liquor ammoniæ, weakened so that the nose could remain applied to the bottle. The perforation was this time made from the opposite side, and more attention being paid to the compression of the tumour, the piston was felt to descend more, and the tumour appeared more injected. At this instant the poor child suddenly ceased its crying, and at the end of a slight convulsion, continuing not a minute, lay upon the table a corpse.

"As to the cause of this melancholy result, the first hurried impression at the conclusion of this harassing scene was, that the attack was merely coincident with the operation; that, in fact, it was one of those sudden and fatal succussions of the nervous system which now and then carry off children, apparently without reason."

"Yet though it is impossible to deny that such was the secret of this appalling case, and the harassed feelings cling to the supposition, it seems at least as impossible to deny, that some of the stimulating fluid was forced into the divided veins, thence carried along the external jugular and subclavian to the heart, and thus produced the death; or that a strong irritant, applied to the branches of the seventh and third nerves, is as likely to excite a fatal succession of the whole nervous system, as those doubtful yet accredited agents, dentition and gastric sordes. Either of these occurrences would be decisive against the operation, and their possibility demands the publication of the

case. No post-mortem examination was allowed. However unlikely it is that air should be forced any where but into the cellular tissue of the nævus, it may be well to state, that it was impossible in this case, as the syringe was first filled with particular care, and afterwards held nozzle upwards, to insure the perfect filling of its pipe, and the escape of any air, if possibly any were left in.

Med. Gazette. December 30, 1837.

MIDWIFERY.

Spontaneous Amputation of the Limbs in Utero. By RICHARD SMITH, Senior-Surgeon, Bristol Infirmary.

THAT limbs are so separated from the body of the fœtus can scarcely be controverted after the printed statements of Drs. Montgomery and Simpson. The *quo modo*, however, the manner in which the occurrence takes place, is not quite so clear; and I perceive that the question has been mooted, particularly by Dr. Robert Lee. Having, in my museum at the Infirmary of Bristol, a preparation which, I think, may throw some light upon the matter, I feel it a duty to address these lines to you. It was sent to me by my former pupil and friend, Mr. Charles Bleeck, Surgeon, of Warminster, Wilts. In 1835 that gentleman attended a female with her second child. It was a breech presentation, and, in due course, the lower parts and trunk up to the axilla, were expelled. Mr. B. proceeded to bring down one of the arms, experiencing no difficulty, but the other resisted all his efforts; it was, in fact, so immoveably fixed, that after using as much force as he felt justified in doing, he desisted; in about ten minutes after this a violent pain brought into the world the head, the arm, and the whole of the secundines at the same moment; the child was alive; the cause of the resistance was now manifest. From the upper part of the funis, the circulation of which was not at all impeded, passed a very strong and tough band, about an inch and a half long, with its end firmly attached to the surface of the placenta; this band was perforated, and grasped, in the aperture, the arm so tightly just above the elbow, that the soft parts were, if I may be allowed the expression, cut down to the bone; the limb was enlarged to four times its usual bulk; the arm was liberated from the band, and the child appeared at first to be going on well, but in twenty-four hours it became restless and uneasy; the limb, too, grew hot, put on a livid appearance, and was soon covered with vesications; the vitality of the limb being apparently at an end at the expiration of forty-eight hours, Mr. Bleeck deemed it advisable to disengage it; this was easily effected, and not the slightest hemorrhage followed, although the axillary artery continued to beat slowly. The child seemed conscious of relief from its incumbrance, and quiet during thirty-six hours, at the end of which time it was seized with convulsions, and died. Now, Sir, my view of the matter is this,—I consider that at an early period of utero-gestation a band of coagulable lymph had been thrown out from the funis to the neighbouring part of the placenta; in closing or being coiled around the arm of the fœtus, that this band became vascular, thickened, and tough; thus circumstanced, it would yield but imperfectly to the growth of the limb, but rather continued to grasp it more and more firmly, and thus, by its continued pressure, produced absorption of the soft parts actually down to the bare humerus. It is easy to imagine how easily a struggle of the child might snap off the latter, and thus circumstanced it would probably have been the case in the present instance, but that the period of utero-gestation was at end, and that the bone formed as yet an attachment to the body. If I am right in my conjectures the above forms an easy solution to the marvellous riddle, and if it may happen thus in one instance, it may occur in several.

Lancet. February 17, 1838.

PART FIFTH.

Medical Intelligence.

EXPERIMENTS AND OBSERVATIONS ON HANGING. BY DR. CASPER, OF BERLIN.

THE principal object of these experiments was to determine whether any evidence of hanging during life could be obtained from the *impression left by the cord on the neck*. Dr. Casper has derived his observations from medico-legal practice, as well as from experiments on rabbits and recently dead human subjects. The results at which he has arrived correspond with those of Orfila and Beaudé, previously published in France. They completely refute the ancient notion, relative to the constant production of ecchymosis by the cord in the *living*; an error which, however, is still adopted and acted on by numerous modern medical practitioners. The other and even more serious mistake of the non-production of ecchymosis, being regarded as a certain proof of hanging after death, is also strikingly exposed.

The following remarkable case was communicated to the author by Dr. Hinze, of Waldenburg, in the year 1826:

A young man, in a fit of drunkenness, hung himself with a stout cord. He was cut down in about half an hour afterwards; and Dr. Hinze and others endeavoured to resuscitate him. The cord had merely produced a superficial mark on the neck, destitute of any appearance of ecchymosis. Signs of returning animation began to manifest themselves, the efforts of the medical attendants were continued for several hours, but the traces of vital reaction disappeared. To the astonishment of all present, when life was about to become again extinct, the *mark on the neck became deeply ecchymosed*. This ecchymosed condition was verified by an inspection made the following day. Death was due to sanguineous apoplexy.

Dr. Casper commenced by performing some experiments on rabbits; but we hold all such experiments to be utterly valueless, in a practical point of view. The skin of animals differs too widely from that of a human being to allow of any safe analogical inferences being drawn from observations of this kind. At any rate, we think a court of law would have fair ground for objecting to the opinions of that witness whose evidence was influenced by such experiments. Several instances are quoted, in which hanging a body has been resorted to by a murderer for the purpose of concealing his crime. The two following cases Dr. Casper met with in his own practice.

A boy was found hanging perfectly dead. On examination, a round ecchymosed mark, about the size of a dollar, was seen on the larynx, with several impressions on the surrounding skin. There was neither depression nor ecchymosis in the course of the cord around the neck. The inspection left no doubt that the deceased had died from asphyxia; and it was discovered that the boy had been first strangled and afterwards hanged.

In the second case, a man, sixty years old, was found hanging in a room. His body was so suspended to a hook in the door, that the nates were not more than nine inches from the floor; and his legs were stretched out at full length. The cord by which he was suspended was from two to three feet long, and was loosely passed around his neck. The furniture of the room was in great disorder, and some marks of dried blood were seen on one part of the floor. The right side of the head and face of the deceased was in several parts ecchymosed and excoriated. A circular impression had been produced in the neck by the cord; but there was no extravasation beneath. A little above this was a strongly ecchymosed mark,

extending round about one-half of the neck to the occiput. In the skin beneath this, blood was found extravasated. Death was owing to asphyxia. The medical opinion expressed, was to the effect that the deceased had been first murdered and afterwards hanged. Evidence subsequently adduced confirmed this opinion.

Dr. Casper's experiments on the dead human subject amounted to eight.

1. A man, æt. twenty, *an hour* after death from typhus, was hanged by a double cord passed above the larynx. In about twenty-four hours the body was cut down and examined. Around the neck between the larynx and os hyoides was a double parallel mark about three lines deep, of a *brown colour with a slight tinge of blue*. There were traces of cadaverous ecchymosis about the body. The whole appearance of this subject was such that any individual, not acquainted with the circumstances, would have supposed on looking at it that the deceased had been hanged while living. Some spots on the right side of the neck were strongly coloured. The skin of this part was hard, like leather; and in patches slightly excoriated. There was no extravasation of blood in the cellular texture; but the muscles of the neck beneath were of a deep violet colour. The large vessels of the neck were not congested.

2. A young man, æt. twenty-three, *an hour* after death from phthisis was hanged with a double cord, and his body was examined on the following day. A double depression was seen around the neck, in which were marks produced by the prominent portions of the cord. The depressions were of a *yellowish brown colour without ecchymosis*. The cutis was as if burnt and like parchment, both when felt and cut. There was no blood extravasated in the cellular tissue beneath.

3. A man, æt. seventy, who died from dropsy was hanged *two hours* after death. The impressions on the neck presented the same characters as in the preceding case.

4. The next subject was a man who died of apoplexy. *Thirteen hours* after death, a cord was very tightly drawn around the neck above the larynx. Six hours afterwards, on examining the constricted part, a soft impression, easily removed by pressure, was perceptible. There was no colour, nor was there any change whatever in the skin.

5. *Six hours* after death the double cord was tightly drawn around the neck of a female above the larynx. On the following morning it was removed, and the skin examined. There was no particular appearance, the part where the cord had been applied was scarcely distinguishable.

6. *Twenty-four hours* after death, the double cord was very tightly drawn around the neck of a man. On the following day, a slight double depression was perceptible: but there was no alteration of colour, nor any change in the skin either on the surface or beneath.

7. In a similar experiment, after the same period of time, the spot where the cord had been applied could scarcely be seen.

8. The last experiment was on the body of a child about a year and a half old. On the day after death, a small cord was tightly drawn and secured around the neck. Twenty-four hours afterwards, it was found that a small bluish-coloured mark had been produced by the constriction. This mark, although very superficial, was still visible enough to strike the eye. There was no trace of extravasated blood on cutting into it.

Before giving a summary of conclusions from his experiments, the author presents us with an interesting statistical account of 106 cases of hanging; the greater number of which are borrowed by him from various medico-legal reports. Out of the 106 cases, there were 77 males and 29 females. The means of suspension used were various:

A cord was employed in 51 cases.
Handkerchiefs, bandages, stockings, in 25 cases.
Unknown means 30 cases.

In sixteen cases out of nineteen, it was found that the use of a handkerchief did not prevent the formation of a visible mark around the neck; and on the whole, the author thinks that the article used for suspension has but little influence on the local

changes produced. The exact site of the ligature is also of little consequence, either in respect to the formation of a mark by the cord, or to the manner in which death takes place; i. e. whether by apoplexy or by asphyxia. In regard to the position of the ligature, it was found to be:

Between the os hyoides and larynx in	59
On the larynx or thyroid cartilage in	9
Position undetermined in	38

The local changes in the mark itself are as follows:

Accompanied by ecchymosis and subcutaneous extravasation, in	21
Of a yellow colour, without ecchymosis or extravasation	50
Unknown	35

Thus we learn that, in seventy-one well-ascertained cases of hanging during life, only twenty-one were accompanied by true ecchymosis in the depression produced by the cord, which is in the ratio of about *two cases to seven*. In three of these cases, there was no greater change of colour in the skin, externally, than had been remarked in experiments on the dead. The white or colourless depressions were mostly met with in very fat subjects.

This fact is most important in Medical Jurisprudence: it shows that a ligature in destroying life by hanging, does not necessarily cause a bloody impression; on the contrary, in many cases the mark is precisely similar to that which is produced in the suspension of subjects *soon after death*. The time which the ligature remained around the neck of the individual hanged was observed to make no difference as to the production or non-production of ecchymosis by it. When it was removed sooner or later after death, ecchymosis was sometimes found, and at other times not.

Remer imagined that the absence of ecchymosis in the course of the ligature indicated death by apoplexy; but Casper found that the mark was not ecchymosed, as well when the vessels of the brain were empty as when they were congested. It is generally assumed that, in hanging, death takes place from apoplexy, from suffocation, or from the two kinds of death simultaneously: but this opinion requires some modification. In the 106 cases recorded by Casper, death took place:

From apoplexy in	9
suffocation	14
both conditions	62
neither	5
Unknown or unexamined bodies	16

In not one instance of the apoplectic cases was blood effused on the brain: there was merely more or less congestion in the sinuses and vessels. It is in this sense that we are to understand the meaning of the word apoplexy, as applied to death by hanging. The word, however, thus employed conveys an ambiguous meaning. We ought rather to say, that death is caused by an obstruction to the circulation in the brain. The lividity of the countenance depends upon this obstruction; for when the circulation of blood is impeded in the chest before the cerebral obstruction takes place, the face of the hanged person will not be livid. In some instances Casper found it extremely pale.

The emission of semen, or rather of the *liquor prostaticus*, in the male, has been considered a good sign of hanging during life. In seventy-seven cases, Casper met with this emission in nineteen, about *once in three cases*. Evacuation of fæces was observed only four times in 106 cases. Renier asserted that the female organs of generation were often vascular and congested in death from hanging: but Casper found these appearances but once in twenty-nine cases. The erection of the penis in the male may take place more commonly than is imagined: for, unless the examination be speedily made, the organ will again collapse. Guyon saw fourteen negroes hanged; and, at the moment of suspension, erection took place *in each*. In nine of these cases, traces of this erectile state were perceived *an hour* after death.

The author derives the following conclusions from his observations:

1. Death from hanging is in most cases to be ascribed to an obstruction of the circulation.

2. The *mark* produced by the ligature cannot be relied on as evidence of hanging during life.

3. A ligature applied to the neck a few hours *after death* produces the same local changes as are met with in most of those subjects who have been hanged *during life*.

4. These local changes consist in the skin becoming *brown or yellow in the course of the cord*, as if it had been burnt. There may be also a well marked depression. The skin feels and cuts like leather. In more rare cases, i. e. about 1:3½, a true ecchymosed impression is produced by the ligature.

5. The mark produced on the neck of a subject hanged long *after death*, presents none of these characters.

6. The kind of *material* used for the ligature does not affect or modify the local changes produced by it.

7. The exact position of the ligature, in relation to the larynx, has but little influence on these local changes.

Since obtaining the above results from his experiments, the author has met with an opportunity of making a practical application of them in a case which involved a charge of murder.

A girl was delivered secretly of a child in a cellar. The body of this child was found some days afterwards, under suspicious circumstances. The account given by the mother, who was accused of the murder, was that she heard her child cry during delivery, but it soon died. She then covered its body lightly over with straw, and left the place. She returned to the cellar in about an hour; and having twisted a few straws into a band she tied this tightly round the neck of her child, in order, to use her own language, "to prevent it from awaking." On examining the child, it was found to be natural and well formed: and it had evidently respired. Apoplexy was assigned by the examiners as the cause of death, and they affirmed that this had most probably resulted from the child having been strangled by the band of straw found around its neck. The accused was condemned to be publicly whipped and to be confined for life.

An appeal was made from this sentence, and our author's opinion was called for. The medical witnesses had stated, in their evidence relative to the mark produced by the band, "that the depressions caused by each straw were clearly seen on the neck. These depressions were *whiter* than the rest of the surrounding skin; but the little folds or elevations of skin between the straws were *red*. On cutting into these red spots, it was found that in some of them the discoloration was owing to true ecchymosis in the cutis." The ecchymosis is stated to have been very slight: but still, from the appearance, the witnesses believed that the mark had been produced during life: in other words, that the child had been born alive and strangled by the mother.

Our author, after observing that no coagulated blood was found in the skin beneath the mark, and then neutralizing the effect of this observation by admitting that such an extravasation is seldom or ever met with in death from strangulation, proceeds to contend that the slight degree of ecchymosis, in the course of the mark in this case, *might* undoubtedly have resulted from the application of the straw-band *soon after death*, and while the body was yet *warm*. From the depositions, it appeared that the child could not have been dead more than *an hour* when the ligature was applied: for the prisoner confessed that she returned and put it round her child's neck, about an hour after her delivery. She might not have been absent so long; but allowing that her statement as to the time was correct, the mark might have been produced, since the body would not during that time have become cold. Hence Dr. Casper declared, that it could not be positively assumed that the child was living when the ligature was placed around its neck: therefore it was not established that it had died by strangulation. In consequence of this declaration, a mitigation of punishment followed.

[REMARKS. Among the experiments performed by the author of this paper, the

earliest period after death at which a ligature was applied around the neck was an hour: and, in this case, there was no well defined ecchymosis produced by it. In those experiments, in which six, thirteen, and twenty-four hours had elapsed after death before the ligature was employed, the skin remained wholly unaffected. In others, where the individuals had been dead not more than one and two hours, the mark left by the cord presented those characters which are most generally met with in hanging during life: i. e. the skin which had sustained the compression was *hard* and of a *yellow colour*. It still remains to be determined, whether these appearances can be produced at intervals of three, four, or five hours after death, so that the period at which such effects would cease to be produced has yet to be ascertained. It is to be regretted that our author has not stated, whether all of his cases were cases of suicide or not. The mark produced by a cord is generally much stronger, and presents a more decidedly ecchymosed character, in individuals who have undergone the sentence of the law or who have fallen victims to homicidal violence, than in those who have died by their own hands.

Dr. Casper seems to us to have represented the cause of death in its true light. It is clear that if a mere congestion of the cerebral vessels be taken as conclusive evidence of death from apoplexy, two thirds of the cases of death from organic disease, as of the liver, kidneys, or other remote parts of the system, might be with equal justice referred to the same cause. No language can be more vague than that which ascribes death to an apoplectic attack, simply because the vessels and sinuses of the brain are found somewhat full. Our author truly observes that, in hanging, this fulness furnishes evidence of the cerebral circulation having been, from some cause, obstructed: but such an obstruction, where it has existed, is not to be confounded with that morbid condition of the brain which is generally met with in cases of apoplexy.

We agree in all the conclusions of Dr. Casper except the sixth. Whether a cord or cravat be the material of suspension in suicidal hanging, the mark on the neck is generally destitute of ecchymosis, presenting the yellow horny character already described; but we firmly believe from actual observation, and we see nothing contradictory to our views in the cases quoted in this paper, that a hard ligature like a cord is more liable to cause ecchymosis and excoriation, as well as to leave a more visible depression on the neck, than a soft material like a cravat.

We have one remark to make on the case with which Dr. Casper concludes his essay. We learn from the report of this case, that the ecchymosis produced by the ligature on the neck of the child was slight; and that it was only perceptible in the little elevated portions of skin corresponding to spaces between the folds of straw. Now this description of the appearances will apply to a mark produced during life, or, as allowed by Dr. Casper in this case, within *an hour* after death. We admit this latter position from facts with which we are acquainted, relative to the production of ecchymosis in the dead; but we do not find among the experiments performed by the author of this paper, any one which bears out the somewhat exclusive view which he took of the circumstances. In the two experiments where the subjects were hanged within an hour after death, there was not even the semblance of ecchymosis, while in the case of the child there was evident, although interrupted, subcutaneous effusion. We will, however, allow that the ligature might have been put round the neck of this child either living or dead. But which was the most probable circumstance, and whether it were more likely, that a strong ligature should have been thus tightly applied *after death* to prevent a child from "awaking," or *before death* for the assumed purpose of destroying it, were facts upon which, of course, it was for a jury and not for a medical witness to decide. When medical evidence is thus nicely balanced, circumstances alone can establish the guilt or clear up the innocence of an accused party. If there was nothing more in this woman's favour than her own statement, relative to the time at which she applied the straw ligature, and to the object which she had in applying it, we must declare our opinion that, in her appeal, she not only met with a very humane medical witness, but with a very merciful tribunal.]

Wochenschrift für die gesammte Heilkunde. Januar, 1837.

THE CONTROVERSY BETWEEN DRs. GRANT AND HALL, AND MR. NEWPORT,
RESPECTING DISCOVERIES IN THE NERVOUS SYSTEM OF INSECTS.

IN our Number for January, 1837, on the occasion of announcing the award of the royal gold medal of the Royal Society to Mr. Newport, we gave a brief notice of the investigations and discoveries in the anatomy and physiology of insects for which this splendid reward was conferred. In our Number for October, of the same year, we gave a more detailed account of a subsequent paper by the same gentleman, on the Temperature of Insects, and its Connexion with the Functions of Respiration and Circulation. At that time we were so well satisfied of the qualifications of the Council of the Royal Society to decide in such matters, that we never entertained the least doubt as to the propriety of the award of the gold medal, nor, consequently, of the justice of Mr. Newport's claims to that high honour. Very recently, however, the justice of this award, and the legitimacy of these claims, have been disputed by Dr. Grant and Dr. Hall; and, as the questions at issue are not merely important as affecting the credit and fame of physiologists of distinction, and the reputation of the most eminent scientific body in this country, but also as matters of interest in the history of physiology, we purpose taking a review of the whole subject on some future occasion, when the war, which now rages so hotly, has ceased, and left men's minds in a fitter disposition for examining more coolly, and deciding with less risk of being accused of partisanship. In the mean time, we have taken the trouble to put in an English dress the opinion of one, who will be allowed by all to be both an impartial and a competent judge, respecting an important point in the controversy,—the extent of Mr. Newport's discoveries in relation to the previous labours of continental physiologists. Whenever we do enter upon the consideration of this controversy, we shall, of course, entirely pass over the personal matters that have been so improperly and indelicately mixed up with it by Dr. Grant and Dr. Hall; but we cannot help remarking, on the present occasion, that, for our own parts, we would much sooner forego the credit of having made the discoveries contended for, than bear the discredit which must inevitably attach to those who could make attacks of such a nature as every person of well-constituted mind must reprobate.

Extract from Müller's Jahresbericht über die Fortschritte der Anat. Phys. Wiss. im Jahre, 1836. (Archiv. Jahrgang, 1835. Heft i. s. 81-4.)

"AMONG the most distinguished labours in comparative anatomy during the past year, is that of NEWPORT on the nervous system of the *Sphinx Ligustri*, during the last stages of its pupa state; the continuation of former investigations in the Phil. Trans. for 1832. The author gives a very accurate description of the visceral nerve, the cephalic portion of which consists of three ganglia: the first of these lies before the brain, and is connected to it by two roots; the two others lie behind the brain and are also connected with it by roots. From these three ganglia, which give off twigs to the mouth and œsophagus, the main trunk of the visceral nerve arises.

"This investigation has enlarged our knowledge of the facts already adduced by Lyonet, Müller, Brandt, and Straus-Dürkheim; and the author has supplied us with new and important facts in his description of the *nervi accessorii respiratorii*. We have been long acquainted with nerves which arise, in common, from the upper surface of the abdominal cord, between every two ganglia, in the median line, and then divide transversely into two similar nerves. Newport names these nerves, which are principally destined to the muscles of respiration,—*nervi transversi accessorii*. In the larva and pupa of the *Sphinx Ligustri*, twigs of these nerves unite with *animal nerves* (animalischen nerven) which originate in two roots, one from the abdominal cords, the other from their ganglia. The author has made it appear that these nerves form a particular system, inasmuch as the thread from which they originate is continued over the ganglia and abdominal cord into the median line. The *nervi transversi*, immediately after their origin, give off, each, a twig which runs over the outer edge of the surface of the nerves and ganglion; and, converging with that of the other side, again unites in the middle line of the abdominal cord, from which the nearest middle thread again arises and

comports itself precisely in the same manner in regard to the next ganglion, in order to form the next *nervi transversi*. The last pair of these nerves are spread over the rectum. This system of the *nervi transversi* is connected anteriorly with the lateral ganglia of the cephalic portion of the visceral nerve: in the *Gryllus viridis* the middle thread of this system terminates in a small ganglion; in the *Carabi* and *Gryllotalpa*, at each origin of the *nervi transversi*, there is a small ganglion in the middle thread. The purpose of this system seems to be complex: it is chiefly distributed to the sphincters of the air-pores; it is partly motory; but from its connexion with the visceral nerve and its ganglionic structure in some insects, it would appear to contain also organic fibres. Moreover, since this system is connected with the nerves of the extremities and wings, which come from the animal system or that of the abdominal cord, these connexions are probably for the purpose of intermixing organic branches with the animal system. [This is our interpretation of the interesting observations of Newport.] The termination of these nerves is always in the anal muscles. The author, instigated by an earlier observation of Müller respecting a third column in the abdominal cord of the scorpion, has also again found this nerve here. An important discovery of the author is, that he has found the abdominal cord of insects and crustacea to consist of an anterior and posterior pair of columns. The upper pair of columns has no share in the ganglia of the abdominal cord, which belong exclusively to the under pair of columns. He has made this discovery in examining the nervous system of crustacea and insects hardened in spirits. In this way the analogy of the abdominal cord of insects with the system of the spinal cord of vertebrata becomes much more evident than before, and the discoveries of Bell respecting the motory and sensitive system of spinal nerves, become, at once, applicable to the invertebrata. The columns which want ganglia are the uppermost, and therefore, probably, the motory; the under without ganglia, probably the sensory. If this be the case, the relation is the reverse of that which exists in the vertebrata, in which, as is well known, the motor columns lie undermost. By means of the observations of Newport, the conjecture of Treviranus and E. H. Weber, becomes almost proved, —viz. that the ganglia of the abdominal cord of the articulata, correspond with the ganglia of the spinal nerves (the ganglia of the sensitive roots.) The mixed nerves of the abdominal cord, according to Newport's investigations, originate, in the *Astacus marinus*, in roots which belong partly to the ganglia and partly to the superior ganglionless columns. In these animals Newport also saw nerves which originate exclusively from the upper columns and not from the ganglia, and are distributed only to muscles. Finally, the author describes the changes of the nervous system during the period of transformation, and here his observations correspond with those of Herold."

"THE RIVAL DISCOVERERS."

WE recommend to the attention of our readers an excellent paper with this heading, which appeared in a late number of the Medical Gazette, relating to a subject which we also find ourselves called upon, as journalists, to notice. On more than one occasion, since the commencement of our critical labours, have we felt ourselves under the necessity of animadverting, in somewhat strong terms, on the very unphilosophical temper in which some members of our profession set about their scientific investigations, and particularly, on the envious and uncharitable spirit with which certain physiologists are accustomed to assert their rival claims of discovery. "Such (we have said) are the fretful labours of controversialists who allow the love of fame to predominate over the pure love of truth. Their restless obtrusion of their real or supposed discoveries, their fidgety fear of being run over and forgotten, their challenges and reclamations, their attacks and replies and rejoinders, whilst they doubtless disqualify them for being sound sleepers, neither amuse nor instruct nor convince, but make the paths of science thorny and unpleasant, which should be full of peace and delight." . . . "Thus mortal men, using the intellect which God has given them, make ever slow advances along the boundaries of knowledge, sometimes in column, and some-

times in equal line of march; and happy is it when, on lighting on some undiscovered truth, they do not turn round or aside, and contend with their followers or fellows, as is the wont of carnivorous creatures over their precarious food, disgracing the understanding and mocking the dignity of the human creature by the passions of the lower tribes of animals. It was a saying of Sir James Mackintosh, that, when asked in another world what he had been doing all his lifetime in this, he should have no better answer to make than that 'he had been talking.' Of how many a physiologist the reply must be—'I have been fighting;' not from the calmness and silence of the study, with unwrinkled brow advancing, but with flushed cheeks and soul-marks of intellectual blows."

Since these strictures appeared in our last Number, our literature and our science have been alike disgraced by an exhibition of the kind referred to, and from the same quarter, but much more outrageous than any that has preceded it. The readers of the weekly Journals will be at no loss to know that we allude to the recent controversy between Drs. Grant and Hall and Mr. Newport, touching their respective claims to certain discoveries, and to the promulgation of certain opinions, relating to the nervous system of insects. Of the nature of these claims we shall take another occasion to speak, and shall endeavour to award impartial justice to the contending parties, forming our judgment from the coolest and most careful examination of the evidence produced. But the question of claims so fiercely contended for by the parties immediately interested, and even the correctness of our own decision, (so long as we have the consciousness of knowing that we wished to decide correctly,) are to us matters of little moment in comparison with the injury which has been inflicted on the character of our profession by the manner in which the controversy has been conducted. In the position in which we are placed, as one of the public organs of the profession in this country, we hold it to be our duty, in vindication of the honour of that profession, to declare that its members, as a body, hold no sympathy with the low and evil passions disclosed in the communications of Dr. Grant and Dr. Hall, and repudiate the employment, in scientific warfare, of the weapons of mere obloquy and personal invective, so fiercely and lavishly wielded by them against Mr. Newport. Had every one of the charges of delinquency brought against this gentleman been true, instead of being, as he maintains, utterly groundless, and the offspring of misapprehension or malevolence; although we might, in this case, have made allowance for much of the severity of the language used by Dr. Grant and Dr. Hall, it would have been impossible to justify or excuse, or even to regard without disgust, the detail of private circumstances intruded into the controversy by the last-named physician. Often before now has it happened,—and often, we trust, it will again happen,—that men of science have mutually given and received assistance, both personal and pecuniary; but we hardly think that it ever before occurred, among men in the upper ranks of society, that such pitiful and paltry obligations as those conferred on Mr. Newport by Dr. Hall, were trumpeted forth to the world by the individual himself, and with all the pomp and circumstance of offended patronage. We will not disfigure our pages by enumerating them. It is no wonder that Mr. Newport should say, "Surely there never was such a practical sophistry attempted to be maintained, and hardly ever was there put on record charges more unjust or more insidious;" or that he should add, "I know not how my statement may be received by Dr. Hall, but I shall think much worse of him than I now do, if he feels no remorse on reading the details he compels me to publish; and I shall have formed a very incorrect judgment of his friends and my own, and of the members of the profession generally, if the history of our acquaintance is not considered to be at least as humiliating to him as to myself." We think the circumstances disclosed in no degree humiliating to Mr. Newport; but we are greatly mistaken if Dr. Hall will not ere long make the discovery,—a discovery which all that he has already made or imagined will not lightly counterpoise,—that, on the present occasion, he has allowed himself to be betrayed by his passions into a forgetfulness of his own dignity and self-respect, and that he has sustained, in consequence, a proportionate loss in the estimation of his friends.

COMPLETE ANTICIPATION OF DR. MARSHALL HALL'S DOCTRINE OF "THE REFLEX FUNCTION," BY PROCHASKA.

WE unaffectedly declare that we take shame to ourselves for having overlooked, in the composition of the article on the *Physiology of the Spinal Marrow*, in a preceding part of the present Number, the writings of PROCHASKA. Never, perhaps, was there a more remarkable anticipation of any doctrine supposed to be new than the anticipation of the doctrine of the so-called *Reflex Function* by the learned professor of Prague, as proved by the subjoined extract. This extract is taken from the third fasciculus of his *Annotationes Academicæ*, published at Prague in 1784. The anticipation of Prochaska differs from most of the other cases quoted in our article in this particular, that he does not notice the subject merely incidentally or as a matter of no importance: he clearly and explicitly announces the phenomena as depending on a distinct and important property of the nervous system, to which he devotes a whole chapter of his treatise. It is for this reason, and to avoid all possibility of cavil respecting the author's meaning, that we give the extract in the original,—and, for the present, without any translation. On reading this extract, we think even Dr. Hall will admit that nearly all that is defective in the views of Prochaska, respecting the doctrines of the reflex function, is a necessary consequence of his ignorance of the existence of the distinct properties of the roots of the spinal nerves, afterwards discovered by Sir Charles Bell. If we, as reviewers, confess ourselves ashamed of our previous ignorance of this portion of Prochaska's writings, we hope we may be forgiven for remarking that Dr. Hall's ignorance of it is still less pardonable.*

"CAPUT IV.—*Functiones Sensorii communis.*

§. 1.—*Quid Sensorium commune, quæ ejus munia, & quæ sedes ?*

Impressiones externæ, quæ in nervos sensorios fiunt, per totam eorum longitudinem celerrime ad originem usque propagantur; quo ubi pervenerunt, reflectuntur certa lege, & in certos ac respondententes nervos motorios transeunt, per quos iterum celerrime usque ad musculos propagatæ motus certos ac determinatos excitant. Hic locus, in quo tanquam centro nervi tam sensui quam motui dicati concurrunt, ac communicant, & in quo impressiones nervorum sensoriorum reflectuntur in nervos motorios, vocatur, termino plerisque Physiologis jam recepto, *Sensorium commune.*

Non unus locus est, in quo sedem sensorii communis constituerunt Clariss. Viri. Bontekoe, Lencisius, de la Peyronie in corpus callosum posuerunt *Sensorium commune*; Willisius perceptionem sensuum & motus scaturiginem a corporibus striatis repetiit; Cartesius Glandulæ pineali munus sensorii communis tribuit; Vieusensius centro ovali; Boerhaave illud aggregatum punctorum pro *Sensorio commune* statuit, in quod omnes nervi sensorii terminantur, & ex quo omnes nervi motorii oriuntur, illudque ponit in medulla fornicata circumstante cavitatem ventriculorum†); in posteriore opere de morbis nervorum Boerhaavius *Sensorium commune* in confinio medullaris substantiæ cum corticali ponit, quam sententiam etiam verosimillimam esse autumatur Ill. Tissotus, & comprobari observatis Wepferi existimatt); Cl. Mayer *Sensorium commune* in medulla oblongata ponere videtur§); hujus sententiæ quoque esse videtur Cl. Vir. I. D. Metzger||), Celeb. Camperus dicit, si *Sensorium commune* locum aliquem habet, illum debere esse in Glandula pineali, & natibus, testibusque, opinionemque Cartesii non esse adeo

* It is but justice to Professor Sharpey to state that we are indebted for our knowledge of the subjoined extract to him. Dr. S. noticed it recently in one of his lectures; and this was stated to us by a pupil.—Ed.

† Prælect. acad. in proprias Inst. cum notis Halleri. Tom. iv. § 574.

‡ Abhandl. über Nerven und Nervenkrankh. 1ten Bands, 2ter Theil, § 236.

§ Abhandlung vom Gehirn, Rückenmark, &c. Seite 34, 38.

|| Advers. med., p. 15. Vermisch. Schrift. 1ten Bands, Seite 56.

ineptam*). "Totum cerebrum cerebellumque certe non videtur ad Sensorium commune constituendum spectare, quæ partes systematis nervosi videntur potius instrumenta esse, quibus anima immediate utitur, ad actiones suas, animales dictas, peragendas; sed sensorium commune proprie dictum se per medullam oblongatam, crura cerebri cerebellique, etiam thalamorum opticorum partem, & totam medullam spinalem, verbo, quam late patet nervorum origo, extendere non improbabile utique videtur. Ad medullam spinalem usque Sensorium commune extendi†) docent motus in animalibus decapitatis superstites, qui sine nervorum ex medulla spinali oriundorum consensu ac commercio fieri non possent; nam rana decapitata si pungitur, non tantum punctam partem retrahit, verum etiam repit, & saltat, quod absque consensu nervorum sensoriorum & motoriorum fieri nequit, cujus consensus sedes in medulla spinali, superstitute Sensorii communis parte, sit oportet.

Impressionum sensoriarum in motorias reflexio, quæ in Sensorio communi sit, non peragitur juxta solas leges physicas, ubi angulus reflexionis æqualis est angulo incidentiæ, & ubi, quanta fit actio, tanta etiam sequitur reactio; sed leges peculiare, a natura in pulpam medullarem sensorii quasi scriptas, sequitur ista reflexio, quas ex solis effectibus tantum noscere, neutiquam vero assequi ingenio nostro valemus. Generalis tamen lex, qua commune Sensorium impressiones sensorias in motorias reflectit, est nostri conservatio: ita ut impressiones externas corpori nostro nocituras sequantur certæ impressiones motoriæ, motus producturæ eo collimantes, ut nocumentum a corpore nostro arceatur, amoveaturque; & vice versa impressiones externas seu sensorias, nobis profuturas, sequantur impressiones internæ seu motoriæ, motus producturæ eo tendentes, ut gratus ille status ultro conservetur. Hanc generalem reflexionum Sensorii communis legem probant certe plurima exempla, quæ adduci possent, quorum pauca tantum adduxisse sufficiet. Irritatio in membrana narium interna facta excitat sternutationem, quia impressio illa ab irritatione in nervis olfactoriis facta per eos ad Sensorium commune defertur, ibi certa lege reflectitur in nervos motores, musculis respirationi dicatis prospicientes, & per hos validam expirationem per nares producit, qua ab aëre vi transeunte irritamentum avellitur, & ejicitur. Ita fit, ubi irritatio in aspera arteria per micam cibi, vel guttulam potus illapsam causatur: facit hæc irritatio ad Sensorium commune delata, & ibidem in nervos respirationis motui dicatos reflexa, ut excitetur valida tussis, aptissimum ad expellendum irritamentum remedium, quæ prius non desinit, donec irritamentum ejectum non fuerit. Si amicus digito suo appropinquat ad oculum nostrum, licet persuasi simus nihil mali nobis inferendum esse, tamen jam impressio illa per opticum nervum ad Sensorium commune delata, in Sensorio ita reflectitur in nervos palpebrarum motui dicatos, ut nollentibus claudantur palpebræ, & arceant molestum digiti ad oculum attactum. Hæc & innumera, quæ afferri possent, exempla manifeste ostendunt, quantopere reflexio sensoriarum impressionum in motorias per Sensorium commune facienda conservationem nostri corporis respiciat. Propterea recte etiam Illustr. Tissotus actionem Sensorii communis illis viribus adnumerat, quarum summa atque connubium naturam corporis nostri viventis constituit†).

Cum itaque præcipua functio Sensorii communis consistat in reflexione impressionum sensoriarum in motorias, notandum est, quod ista reflexio vel anima inscia, vel vero anima conscia fiat. Motus cordis, ventriculi, & intestinorum certe ab animæ conscientia nequaquam pendent, cum tamen nullus motus muscularis fieri possit, nisi stimulus nervis sensoriis applicatus in nervos motores reflexione quadam transeat, & musculi contractionem cieat, tunc certum est reflexionem im-

* Kleine Schriften, &c. Leipzig, 1782. 1ter Band. Nachricht von der Zergliederung eines jungen Elephanten. § 21.

† Medullam spinalem ad Sensorium commune quoque referendam esse contendit Marherrus in Prælect. ad Instit. med. Boerhaavii, tom. ii. p. 404.

‡ Von Nerven, 2ten Bands, 2ter Theil, § 55, in nota prima. Et ibidem, § 6. Nro. 6. Qua de re etiam legere oportet Cl. Thaer, Diss. jam citatam de Actione de Systematis Nervosi in Febribus, & præprimis, § viii. ix. &c.

pressionum istis motibus excitandis aptarum, si in Sensorio communi fiunt, fieri sine animæ conscientia. Verum quæritur, utrum istæ impressiones ad Sensorium commune usque ascendunt reflectendæ, an vero sine hac ambage citius in gangliis, unde plurimos nervos istæ partes habent, reflectantur? Hac de re postea adhuc agetur. Sed fieri tamen reflexiones impressionum sensoriarum in motorias in ipso Sensorio communi anima prorsus nescia docent actiones quædam in appoplecticis, quibus tota conscientia ablata est, superstites: nam & pulsu forti gaudent, & valide respirant, & etiam manum elevant, locoque affecto persæpe admovent inscii. Agit etiam Sensorium commune sine animæ conscientia convulsivos motus epilepticorum producendo, & etiam illas, quæ in profundo somno sepultis præter motum cordis & respirium aliquando observantur, artuum punctorum & leviter vellicatorum retractiones. Huc quoque spectant omnes motus, qui in corpore decapitati hominis aut alius animalis aliquo tempore supersunt, & vellicato corpore, præprimis vero medulla spinali, excitantur, qui certe sine conscientia animæ fiunt, & per residuum Sensorii communis partem, quæ in medulla spinali est, reguntur. Omnes istæ actiones ex organismo & physicis legibus, Sensorio communi propriis, fluunt, suntque propterea spontaneæ ac automaticæ. Illæ actiones quæ in corpore animali fiunt anima conscia, vel sunt tales, in quas anima voluntatis suæ nullum habet imperium vel vero tales, quas anima coercere & impedire pro libitu potest: illæ cum solo Sensorio communi, quatenus ab anima non dependet, regantur, etiam nihilominus, quam quæ inscia mente fiunt, automaticæ actiones sunt: tales est sternutatio a stimulo naribus applicito, tussis a stimulo traheæ illapso, vomitus a titillatione faucium aut emetico assumto, tremores ac convulsiones in chorea S. Viti, & in paroxismo febris intermittens, &c. Actiones vero, quas anima suo imperio dirigit ac moderatur, quamvis etiam in iisdem producendis Sensorium commune suam partem habeat, vocamus nihilominus animales, non automaticas, de quibus capite sequente agetur."

THE CHOLERA IN NAPLES.

[In our preceding Number, (p. 260,) we have given a brief account of the prevalence of cholera at Naples, from official documents; for the following additional particulars we are indebted to the kindness of a friend, resident in that city.]

Extract of a Letter from a Physician, dated Naples, 17th November, 1837.

"THE first well-attested case of cholera occurred on the 2d October, 1836, on the person of a *Doganiere*, or custom-house officer. Here, as in every other place, the faculty was divided into contagionists and non-contagionists. Dr. de Renzi, the editor of the *Filiatre Sebezio*, was then a very violent contagionist, and attempted to prove that the disease was introduced into Naples by a person who came from Puglia. I took considerable pains to ascertain the facts connected with this case. I went to the house where the man lodged who came from Puglia. I found that he had been ill and was sent to a hospital, where I went; and, after a great deal of trouble, I got hold of the books of the hospital, which gave an account of his admission, the medicines he had taken, and the day of his dismissal. The keeper of the hospital flatly denied its being a case of cholera, and gave me the name of the physician who had attended him. I went to him, and had some conversation with him on the subject; he was decidedly of opinion that it was not cholera which this man had, but a common biliary fever; he was treated, in the usual way, with diaphoretics, purgatives, and afterwards with infusion of gentian. I must, however, in justice to De Renzi, say, that, in the same house where this man lodged, another man was taken ill with symptoms strongly resembling those of Asiatic cholera, and died on the second or third day of his complaint. The government, however, did not acknowledge this to have been cholera, although the medical man who attended him, and to whom I wrote for his opinion of the case, said that he believed the disease to have been so. The custom-house officer, however, was not attacked till nearly a fortnight after this last case; and no other person (several slept in the same room with these men,) was attacked in this house.

"Since that time De Renzi has recanted, and has published his recantation in a late number of the *Filiatre*; and by far the greater number of the Neapolitan physicians are now persuaded that the disease is not contagious. This change of opinion among the medical men has already produced its effect on the government; and they are now, apparently, as anxious to remove their quarantines and cordons sanitaires as they formerly were to lay them on. For my own part, I am in some doubts respecting the nature of the disease. I have seen it in four different places, viz. in Bengal in 1825, in China in 1826, in Naples in 1836, and lately in Sorrento during the past summer; and although I have generally declared myself a non-contagionist, yet several cases which I lately witnessed have rather tended to shake this opinion. Be that as it may, one thing is certain, that, whether the disease be contagious or not, we have never been able, with all our cordons and quarantines, to stop its progress; and, therefore, the sooner all restrictions of this kind are removed the better it will be for the world. The disease, as I said, began here on the 2d of October, 1836, and raged with great violence till the beginning of December, when it began to decline; and by the end of the year it had almost disappeared. The rest of the winter was passed quietly; but, on the 13th of April last, it again shewed itself; and by the end of May it was occasioning great apprehensions; in June and July it was more severe than it had ever been before; but in August it again began to subside; and we are now, thank God, entirely free from it. During the first attack of the complaint the mortality was supposed to have amounted to about eight thousand, and in the second attack to nearly twice that number. Many estimate the mortality much higher; but, as the government evidently concealed a number of the deaths, it is impossible to say exactly how many were carried off. The mortality in proportion to the cases is supposed to have been nearly as six to ten. This does not say much for our improvement in the treatment of the disease; indeed, I am not sure that we have advanced one step; for, even with all the disadvantages of climate against them, I believe that the medical men in India were as successful in their treatment (from the very commencement, too,) as their European brethren have since been. The mortality at Palermo was dreadful: out of a population of 150,000, about 30,000 of whom are supposed to have left the town at the breaking out of the complaint, the deaths were nearly 25,000, or about a fifth part of those who remained!"

ROYAL MEDICAL APPOINTMENTS FOR SCOTLAND.

St. James's Palace; March 3.

The Queen has been pleased to appoint the following Gentlemen upon her Majesty's Medical Establishment for Scotland:—Physicians in Ordinary: John Abercromby, M.D.; James Home, M.D.; John Thomson, M.D.; William P. Alison, M.D.—Surgeons in Ordinary: Sir George Ballingall, David M'Lagen, M.D.; James Syme, Esq.—Physicians Extraordinary: Andrew Combe, M.D.; Robert Spittal, M.D.—Surgeon Extraordinary: John Scott, M.D.—Surgeon Oculist: William Mackenzie, M.D.—Surgeon Dentists: Robert Nasmyth, Esq.; David Wemyss Jobson, Esq.

Additional Appointments for England.

Surgeon Oculist: Henry Alexander, Esq.—Aurist Operator: William Maule, Esq.

[All the above appointments are not merely unexceptionable but excellent, and fully justify our observations at p. 584, respecting the character of the preceding nominations of the same kind. It has heretofore been, we fear, a somewhat rare event, in the bestowal of courtly honours, to see merit alone determining the selection of the individuals to be honoured. On the present occasion, as in the case of the Royal Medical Staff for England, we feel assured that the nominations

will give general satisfaction to the profession; a circumstance, we will venture to assert, that has not before occurred since the accession of George the Third. If the rule now adopted is adhered to in future, the Royal Calendar will become a register of merit.]

OBITUARY.

HENRY EARLE, F.R.S.

Surgeon Extraordinary to the Queen.

DIED, on the 18th January, 1838, at his house in George street, from an attack of fever, HENRY EARLE, Esq., Surgeon of St. Bartholomew's Hospital, and one of the most eminent and most respected members of the profession. Mr. Earle was the third son of Sir James Earle, by a daughter of the celebrated Percival Pott, and was born June 28th, 1789. He manifested, very early in life, a strong predilection for the profession of surgery; and was therefore apprenticed to his father, at the age of sixteen. At the early age of nineteen, he held the appointment of house-surgeon at St. Bartholomew's Hospital; and in 1811, being then twenty-two years of age, he commenced practice. He very early distinguished himself among the pupils of the hospital, by the invention of a bed for patients suffering from fractures of the lower limbs and diseases of the spine; for which he received, in 1812, a reward from the Society of Arts, and which he afterwards so much improved as to induce the Society, in the year 1821, to confer on him a second and larger prize. In 1813, the College of Surgeons awarded to Mr. Earle the Jacksonian prize, for an Essay on the Diseases and Injuries of Nerves; an honour which was shortly afterwards followed by his election as surgeon of the Foundling Hospital, on the death of Mr. Ramsden. In the year 1815, a wider field was opened for the display of his talents and the exercise of his activity and zeal, by his election to the assistant surgeoncy of St. Bartholomew's Hospital; in which situation he remained until 1827, when he was promoted to the office of surgeon, which had become vacant by the resignation of Mr. Abernethy. Ever alive to the interests of the students, Mr. Earle immediately availed himself of his new position to supply a deficiency which had been long felt by them, in regard to clinical instruction: he instituted a gratuitous course of clinical lectures, (the first that had ever been given at that hospital;) and these he continued to deliver, on all suitable occasions, during the remainder of his life: a boon which was very highly esteemed by the pupils of the hospital.

Mr. Earle was for several years a Member of the Council of the Royal College of Surgeons; and, in 1833, gave his first course of lectures, as Professor of Anatomy and Surgery, in the College. He held the office of President of the Royal Medical and Chirurgical Society, from March, 1835, to March, 1837, having previously acted as Secretary and Treasurer for many years. On the accession of the present Queen, he was nominated one of the Extraordinary Surgeons to her Majesty.

In all the relations of private life, Mr. Earle was most exemplary: he was an affectionate husband, an indulgent father, and a sincere and constant friend. His public duties at the Hospital were performed with such ardour and exactness, that to a casual observer, the labour might have seemed a source of direct enjoyment to him, instead of being the result of a sense of duty. To all his private patients Mr. Earle was no less the friend than the medical adviser, and beloved even beyond the usual measure of regard accorded to those characters.

Mr. Earle has left eight children, (two daughters and six sons) who, we are happy to hear, are very comfortably provided for.

The following is a list of the principal writings of Mr. Earle:

In the Medico-Chirurgical Transactions,

Vol. iii. Feb. 1812.—Case of diseased Testicle, accompanied with disease of the Lungs and Brain.

- Vol. v. May 10, 1814.—On Contractions, after burns or extensive ulceration.
 Vol. vi. Jan. 3, 1815.—On the Use of Nicotiana in retension of Urine.
 Vol. vii. Feb. 20, 1816.—Cases and Observations, illustrating the influence of the nervous system in regulating animal heat.
 Vol. x. Dec. 21, 1819.—On Affections of the Meatus auditorius externus.
 Vol. xi. March 28, 1820.—On the Danger of Extracting large Calculi, with a description of an instrument for breaking down stones of considerable magnitude.
 June 20, 1820.—On Renal Calculi.
 Vol. xii. April 2, 1822.—Cases of ununited fracture of the Humerus, treated by seton and caustic.
 Dec. 10, 1822.—On the Influence of Local Irritation in the production of diseases resembling Cancer.
 Jan. 14, 1823.—On Chimney Sweepers' Cancer.
 Vol. xiii. June 19, 1827.—On Paraplegia.
 Vol. xiv. April 14, 1835.—Observations on Fractures of the Bones of the Pelvis.

In the Philosophical Transactions,

- April 12, 1821.—On the Re-establishment of a Canal in the place of a portion of the Urethra, which had been destroyed.
 April 25, 1822.—On the Mechanism of the Spine.

Separate Publications:

- Practical Observations in Surgery. 8vo. London. 1823.
 Two Lectures on the Primary and Secondary Treatment of Burns. 8vo. London. 1832.

DR. JOHN HOME.

ON the 18th March, at Edinburgh, Dr. John Home, son of Professor Home, a meritorious and very promising young physician. At p. 601 of the present Number we have briefly noticed the first-fruits of the zeal and industry of Dr. Home, which promised a future harvest of abundance,—alas, never to be realized!—Dr. Home fell a victim to typhus fever, caught in the discharge of his duties as physician to the Fever Hospital. It is melancholy to reflect how many of the rising lights of our profession have been extinguished by this fell scourge of the poor,—in Scotland and Ireland more especially.

BOOKS RECEIVED FOR REVIEW.

ENGLISH.

1. Researches into the Physical History of Mankind. By J. C. Prichard, M.D. F.R.S. M.R.I.A. &c. Third Edition, Vol. II. —London, 1837. 8vo. pp. 373. 15s.
2. Outlines of the Principal Diseases of Females. By Fleetwood Churchill, M.D., Lecturer on Midwifery, &c. Dublin.—Dublin, 1838. 8vo. pp. 402. 10s. 6d.
3. A concise Treatise on Operative Surgery, describing the method adopted by the English, Continental, and American Surgeons; selected for the use of junior Practitioners and Students. Illustrated by 12 Plates. By W. P. Cocks, Surgeon.—London, 1837. 8vo. pp. 375. 14s.
4. A Letter to Lord John Russell, M.P., Secretary of State for the Home Department, on the evil Policy of Quarantine and Restrictive Police employed against the

Asiatic Cholera, &c. By Joseph Ayre, M.D.—London and Hull, 1837. 8vo. pp. 39.

5. Transactions of the Medical and Physical Society of Calcutta. Vol. VIII. Part I.—Calcutta, 1836. 8vo. pp. lxxix. 171.

6. The Quarterly Journal of the Calcutta Medical and Physical Society. Edited by H. H. Goodeve, M.D., and W. B. O'Shaughnessy, M.D. Nos. I. and II. January, April, 1837.

7. An Essay on the Antiquity of Hindoo Medicine; including an introductory Lecture to the Course of Materia Medica and Therapeutics, delivered at King's College. By J. F. Royle, M.D. F.R. & L.S., Sec. G.S., &c. Professor of Materia Medica and Therapeutics, King's College, London.—London, 1837. 8vo. pp. 190. 6s. 6d.

8. The Works of John Hunter. Edited

by J. F. Palmer. Vol. IV. Containing Observations on certain parts of the Animal Economy; with Notes, by Richard Owen, F.R.S.—London, 1837. 8vo. pp. 506. 17s. 6d.

9. Ophthalmia. The various Inflammations of the Conjunctiva or Mucous Membrane of the Eye. By J. Slade, M.D. &c.—London, 1838. 8vo. pp. 120.

10. Elements of Chemistry. By the late E. Turner, M.D. Sixth Edition, Part II.—1837. 8vo. 5s.

11. Elements of Anatomy. By Jones Quain, M.D. Fourth Edition, Part II.—London, 1838. 8vo. 5s.

12. Report of the Proceedings under a Brieve of Idiocy, Duncan against Yoolow, tried at Cupar-Angus, 28-30 Jan. 1837. By L. Colquhoun, Esq., Advocate.—Edinburgh, 1837. 8vo. pp. 135.

13. On the Statistics of English Lunatic Asylums, and the Reform of their public Management. By William Farr.—London, 1835. 8vo. pp. 46. 2s.

14. The Evils of Quarantine Laws, and Non-existence of Pestilential Contagion; the Privy Council and College of Physicians; the Means of Prevention and Method of cure of the Cholera Morbus, and the atrocities of the Cholera Panic. By Capt. White, late H. E. C. Service.—London, 1837. 8vo. pp. 176. 8s.

15. Medicine and Surgery one inductive Science; being an attempt to improve its Study and Practice, &c. By George Macilwain, Surgeon to the Finsbury Dispensary, &c.—London, 1838. 8vo. pp. 551. 12s.

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